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OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND THE ARTS.

MECHANICS' INSTITUTE N.Y.

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MECHANICS' N. Y. INSTITUTE

A
JOURNAL
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APRIL 1800.

ARTICLE I.

On the Antiquity of the Art of Etching on Glaſs. By Mr. FRED. ACCUM.

LOOKING over a parcel of old foreign newſpapers, I was not a little ſurpriſed to obſerve that the art of etching on glaſs had been known and practiſed amongſt our anceſtors upwards of a century ago.

In order that every one may judge for himſelf, whether this diſcovery belongs excluſively to Scheele, the Swediſh chemiſt, I ſhall tranſcribe an advertiſement inſerted in an old German periodical publication*, which is likewiſe copied into Beckman's Collections towards a Hiſtory of Inventions. The original advertiſement, literally tranſlated, runs thus: "*The diſcovery of a powerful acid, by means of which every imaginable kind of figures* "*may eaſily be etched upon glaſs.*

"Take ſpiritus nitri per deſtillationem, put it into a retort, and apply a ſtrong heat. "When it has paſſed over into the receiver, throw into it ſome powdered *green Bohemian* "*emerald* (which, when heated, ſhines in the dark), otherwiſe called *heſphorus*. This "being done, place the receiver, containing the mixture, on a heated ſand-bath, for about "four-and-twenty hours, and it will be fit for the purpoſe. To uſe this corroſive acid,

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* Breſlawer's Collections for the year 1725: January. This receipt is likewiſe inſerted by Krunitz in his Economical Encyclopedy, XI. p. 678.

“ take a pane of glaſs of any kind, clean it well and free it from greaſe, by waſhing with a ley, and when dry, trace out upon it, with ſulphur and varniſh, whatever you chooſe. Put a border of bees'-wax round it, about one inch high, and then pour the corroſive acid, prepared as before directed, carefully over the whole ſurface of the glaſs, and let it ſtand undiſturbed for ſome time, the longer the better. The glaſs will become corroded, and all you had traced before, will now appear as if raiſed, or elevated, above the ſurface of the glaſs, in a very diſtinct and pleaſing manner.”

This advertiſement is ſigned *I. G. Wigand*, and dated from *Goldingen*, in Courland. In a ſubjoined note, the author of it confeſſes that he is not the inventor of the corroſive acid, but that he had the receipt from *M. Pauli*, a famous phyſician reſident at Dreſden, and lately deceaſed. He further aſſerts, that the late doctor, during his life-time, had etched on glaſs ſeveral curious creſts, names, and figures, in a manner no leſs beautiful than ſurpriſing.

In a ſupplement to the before mentioned publication, the ſame author very earneſtly endeavours to convince the public, that the art of etching on glaſs he had made publiſhed ſome time before, was very different from the art of engraving on glaſs by means of emery, flint, red-hot iron, or other ſharp pointed inſtruments of the hardeſt kind. Nor is it that art, ſays he, by which Matheſius, the famous apothecary, operated, who formed elevated, or engraved figures, feſtoons, and other ornaments on glaſſes, drinking cups, &c. for all this I know very well is done by means of diamond pencils. But the method I have made public, ſeems to be, in fact, the very ſame admirable and ſecret art which was formerly practiſed by a ſkilful artiſt of Nuremberg, who worked in that art ſolely for the emperor Charles II.

On purſuing this ſubject a little further, there remains no doubt, but that this old artiſt of Nuremberg, to whom our author alludes, is the perſon of whom Doppelmair * gives the following account: “ Amongſt the artiſts of Nuremberg muſt be enumerated *Henry Swanhard*, a ſkilful engraver in ſteel and ſtone, who found out by the glaſs of his ſpectacles, upon which ſome corroſive liquid had fallen, a new art of etching upon glaſs, which diſcovery he made in the year 1670.” Wagenſeil †, on the contrary, relates, that the inventor derives more merit from his diſcovery, than accident entitled him to claim. “ Swanhard,” ſays he, “ by the acuteneſs of his genius, proved what was hitherto deemed impoſſible. He was the firſt who ever compoſed a corroſive, ſo active as to diſſolve the hardeſt cryſtal glaſs, which hitherto reſiſted the force of the moſt powerful ſpirits of the apothecaries. By means of this corroſive, Swanhard delineated and etched on glaſs figures of men, ſome naked and ſome dreſſed, and all kinds of animals, flowers, plants, trees, and ſhrubs, in a manner perfectly natural, and perfectly eaſy.” The ſame is mentioned in Schneider's Biography of the Artiſts of Germany, 1740, p. 37.

How the above artiſt prepared his corroſive is not mentioned by any of the above authors. It is, therefore, very probable, that he kept his art a ſecret, as it ſeems that the receipt for that purpoſe was made public, for the firſt time, in January, 1725*, by the before mentioned M. Pauli. How he came to know it, I have not been able to learn.

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* Doppelmair's Hiſtory of the Artiſts of Nuremberg, 1734, p. 16.

† Wagenſeili Comment. de Civitate Norimbergenſi, Altdorf, 1697. 4to. p. 154.

That the inventor employed his corroſive to a purpoſe different from that for which it is uſed at preſent, is obvious from the effect it produced upon the glaſs. At preſent the glaſs is covered either with wax, iſinglaſs, or a varniſh, and thoſe figures which are intended to be etched, are traced through this covering, by means of a pointed inſtrument; but our ancient artiſt covered his figures, when traced, with a varniſh and ſulphur, and then applied the acid to corrode the glaſs around them, ſo that the figures which remained ſmooth and clear appeared, when the varniſh was removed, as if raiſed from a dim or dark ground. He adopted this method probably in order to render his invention more palpably different from the art of cutting the figures on the glaſs, as if engraven, long known and practiſed before this diſcovery was made.

That the Bohemian emerald, or heſphorus, made uſe of in this receipt, was our fluate of lime, can hardly be doubted. In old German authors who treat on the working of mines, this ſtone is claſſed amongſt the fluſſe (fluxes) becauſe it was an uſual ſubſtitute to accelerate the fuſion of certain ores. *Agricola*, who changed the German names of quartz, zinc, biſmuth, cobald, &c. into quartzum, zincum, &c. changed fluſs into fluor, which became afterwards very common †. If a paſſage of the ancients can be quoted that ſeems to allude to fluate of lime, it is that of Theophraſtus, where he ſays, that there are certain ſtones which, when added to ſilver, copper, and iron ores, become fluid. For Cronſtadt was the firſt ſyſtematic writer (if I am not miſtaken) who mentioned fluate of lime as a particular genus.

That the ſtone called emerald, or heſ~~phorus, was really~~ our fluor ſpar, becomes obvious from what Kirchmair, profeſſor at Wittemberg, in Germany, remarks, who was probably the firſt who made known what kind of ſtone it was, which had the peculiar property of ſhining in the dark after having been heated ‡, and which by the ancients was ſought for with ſo much avidity. Neither need I to mention that the nitrous acid made uſe of in the receipt was equally capable of diſengaging the fluoric acid, though we uſually make uſe of the ſulphuric.

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* Breſlawer's Collections, l. c.

† Lapides ſunt gemmarum ſimiles, ſed minus duri, fluores liceat mihi verbum an e verbo exprimere, noſtri metallici appellant, nec meo judicio, inepte; ſiquidem ignis calore, ut glacies ſolis, liqueſcunt et fluunt. Varii autem et jucundi colores eis inſident.

BERGMANNII OPUSCUL, p. 466.

‡ Ante annos pacuos admodum inventus mineralis alicujus viſu et proprietatibus in quibuſdam ſimilis ſmaragdo, ab artificibus duobus mihi per amicè notis uſus eſt. Conficiendi phoſphori et repræſentandi modus levis atque brevis ille. Recipe q. v. mineralis viridis ſmaragdum pene referentis; contare in pulverem, aqua madefacta communi, pulvis inſtar ut fiat. Pencillo poſtea in lamina cuprea, magnitudine vel majoris plani alicujus manubrio inſtructa literas, quaſcunque voles, in lamina deſcribe craſſiuſculas. Ardentibus ſuper impone prunis vaſculo exceptis. Phœnomenon ſpectabis in obſcuro amæniſſimum ſine fume et odore lucens. Sed ut verum fatear, nec uſum, niſi curioſi animi explendi, artificium hoc, neque diuturnitatem habet. Pectus ergo avidum ſciendi meliora ſatiari nequit ſive Heſphorus vocetur ſive Veſperugo.

G. C. Kirchmaieri De Phoſphoris et natura lucis, nec non de igne, commentatio epiſtolica. Wittembergae, 1680, p. 7.

There remains, therefore, little doubt, that the art of etching on glaſs does not entirely belong to the moderns, or to deny that the ancients were altogether unacquainted with it, would be doing them an injuſtice. It ſeems rather, that this art belongs to the diſcoveries which were made in thoſe times, in which men were little inclined to tranſmit an account of their inventions to poſterity, and thus this art muſt have been forgotten or loſt. Scheele[*] diſcovered the fluoric acid, and re-invented the art of etching on glaſs in the year 1771. But had Swanhard been able to purſue properly what either accident, or ingenuity preſented to him, he might have enriched the arts, with a diſcovery which acquired great reputation to this Swediſh philoſopher one hundred years after.

FREDERIC ACCUM.

II.

A Deſcription of a new Inſtrument called the Blaſt Ventilator. Invented by J. W. BOSWELL.

IT would be ſuperfluous to offer any arguments to the cultivators of natural philoſophy, reſpecting the importance of any diſcovery in that ſcience. It is ſufficient to announce it to thoſe who ſo well know, that no operation of nature is unimportant; and that although the firſt perſon to whom a fact may occur, may not be able to point out all the advantages which may ariſe from it, yet others of more diſcernment coming after him, may improve upon his ideas, and perhaps uſe it for purpoſes of much higher importance.

What Dr. Hales has written of the great conſequence of the ſubject of ventilation to health, and the preſervation of the food of man, renders it unneceſſary to expatiate on the importance of any new diſcovery in this art; and to his excellent treatiſe on this matter, I beg leave to refer thoſe who have any doubts on the advantages ariſing from it.

The method of forcing air in any required direction, by a fall of water paſſing from a ſmaller into a larger tube, in an engine called the [†] Water Blaſt, is now well known, and frequently applied in mines and furnaces with great efficacy.

Some conſiderations on this machine led me to conjecture long ſince, that a current of air might be made to act inſtead of a fall of water, in an apparatus conſtructed on principles ſomewhat ſimilar to the above, and thus applied ſo as to draw off foul air or ſmoke from any places required, in ſituations where the other could not be uſed. It was only lately that I have been able to think of any method of putting this matter to the teſt of experiment, which would not be attended with too much inconvenience. This I have effected by the inſtrument of which I now ſend the draft and deſcription; and I found the reſult of the experiments made with it, fully to anſwer my expectations.

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[*] Tranſactions of the Swediſh Academy for the year 1771. C. xxxiii. p. 122.

[†] Dr. Lewis, in his very uſeful work on the Arts, has laid down the beſt conſtruction of this engine, from actual experiment.

In the draft the firſt figure repreſents the external appearance of this inſtrument, and the ſecond ſhews it in ſection. Plate 1. From A to B in fig. 2, there is a free open communication in the larger tubes. The part C E D is an open truncated cone, ending in a tube E F, one third of the diameter of that which ſurrounds it, and which reaches into the larger tube the length of two of the diameters of the latter.

There it a ſcale with the draft for fig. 1 and 2, which marks the proportion of the ſmall inſtrument I had firſt made. But as the ſize of any other muſt vary according to the uſes for which it is intended, I have only ſet down the number of proportional parts, and have not mentioned their value, which may be encreaſed at pleaſure.

In uſing this inſtrument, the baſe C D of the cone muſt be turned towards the wind, which when it blows even moderately, will then cauſe a very ſenſible current of air to paſs up from A to B through the larger tubes. The ſmall inſtrument, which I tried at a time when the wind was low, when placed as above directed, oppoſite the opening of the upper ſaſh of a window let down about two inches, acted with ſuch power, as to draw the flame of a candle conſiderably into the tube A, held horizontally at ſome diſtance from it, and made the tube B, by which I ſupported it, ſo hot in a ſhort time, that it was painful to hold it.

This experiment I repeated before many gentlemen always with the ſame reſult; and which at leaſt entitles the inſtrument to a trial on a larger ſcale for uſeful purpoſes, in which I have no doubt of its ſucceeding.

The uſes to which this inſtrument may be applied, are of no ſmall importance, even ſo far as they have occurred to me; others may yet be found out which I have not thought of, thoſe which I have thought of are as follows:

1ſt. It will do extremely well for extracting the unwholeſome air from mines, when it is made of a ſize ſufficiently large.

2d. It will alſo be particularly convenient in ſhips for changing the air in them, rendered unfit for reſpiration by the breath and perſpiration of a numerous crew, by the effluvia from proviſions, bilge water, &c.; and if its efficacy may be judged of in this reſpect, from experiments made on a ſmall ſcale, it would with a moderate wind entirely change the air in a large veſſel in the courſe of two or three hours, even when made of no great ſize.

3d. It will conſiderably encreaſe the force of the draft in a blaſt furnace, if placed on the top of its chimney as hereafter directed, for chimneys in general.

4th. It will ſerve as a ventilator for corn, ſtores, magazines, hoſpitals, and crowded rooms.

5th. It is particularly well calculated to prevent or cure the ſmoking of chimnies, (which inconvenience frequently renders houſes ſo uncomfortable, as to induce the owners to quit them; to ſay nothing of the injurious effects of ſmoke received into delicate lungs. One cauſe eſpecially of ſmoking chimneys it removes, for which no effectual cure has been before diſcovered.

Dr. Franklin, to whom the world is ſo much indebted for his many uſeful diſcoveries in natural philoſophy, and the arts, has given two very valuable papers on the ſubject of ſmoking chimnies, in the Tranſactions of the Philadelphia Society. In theſe he has recited nine cauſes, which occaſion chimneys to ſmoke, amongſt which that of high winds blowing the ſmoke down the funnel is a chief one: for all which he has propoſed effectual remedies; except for this laſt mentioned alone, for which, he acknowledges that there was no certain remedy diſcovered; we may therefore look on ſuch a remedy as an object the more valuable, as it compleats what the Doctor began: ſuch a remedy is the inſtrument deſcribed in this paper, which by removing this evil leaves no defect incident to chimneys that cannot be obviated; and it is ſo effectual for this purpoſe, that from the very principles of its conſtruction, the ſtronger the wind blows, the better will the chimney draw, thus changing what was formerly an evil into an actual benefit.

The manner in which the inſtrument ſhould be uſed for this purpoſe, is one with which we are already familiar. It ſhould be placed on the top of the chimney, in the ſame manner in which the contrivance, (intended for the ſame purpoſe, but very ineffectual) called the Boar's Head, is uſually put up: and ſhould like it be ſupported on a pivot, ſo as to turn round with its back to the wind, that the blaſt might fully enter its conical opening.

To prevent the external air from being drawn up between the turning tube and the fixed one, when placed on the chimney, ſome ſoft leather, or canvas, ſhould be faſtened round the outſide of the lower end of the turning tube, which ſurrounds the fixed tube; this leather or canvas, which ſhould hang down two or three inches below the interval between the tubes, will act as a ſpecies of valve to prevent the outer air from entering, and will thus cauſe the whole effect of the inſtrument to operate in increaſing the draft of the chimney.

There is another method of effecting this laſt purpoſe, more certain in its mode of acting, but which from its requiring a much greater expence, cannot be recommended for general uſe; this is repreſented in fig. 3. which figure is on a ſmaller ſcale than the other two, and is only intended to ſhew the beſt method of fixing the inſtrument on a chimney: it is drawn in ſection to exhibit the internal ſtructure. In it A repreſents the fixed part to be bedded in mortar on the top of the chimney in the uſual manner. B repreſents the moveable part, which turns with the wind, ſo that the conical opening G ſhall be always oppoſite to the blaſt. E is the upright rod on whoſe point B turns round; it is faſtened below to the two bars D D, which are fixed acroſs in the lower immoveable tube A, and paſſes through a collar in the bar H, which is faſtened at its extremities to the moveable tube B. By this means the rod preſerves its perpendicular poſition; and the part of the moveable tube, which ſurrounds the upper part of the fixed one, will revolve round it very cloſe without coming in contact in any part.

C C is an external tube, cloſely ſoldered or cemented to the fixed tube A, ſo that, with its upper part, it may form a deep groove of four or five inches, in which the lower extremity of the moveable tube may freely revolve: this groove ſhould be filled with mercury, high enough to cover the bottom of the moveable tube about half an inch; which contrivance will effectually prevent the outer air from being drawn in between the tubes, without impeding the motion of the upper one in the leaſt. F F is a ſmall projection faſtened to the moveable tube, to prevent the rain or duſt from falling into the groove.

The Blaſt Ventilator would be particularly ſerviceable to ſhip chimnies above all others. Any one who has ever been at ſea, muſt have obſerved the difficulty of preventing ſhip-chimnies from ſmoking when the wind riſes to any height, and what an advantage any contrivance would be that would prevent this inconvenience; and that this inſtrument would be quite effectual for this purpoſe, (there is every reaſon to believe from the experiments already made) and even make thoſe chimnies draw better as the wind blew harder.

The inſtrument repreſented in the two firſt figures would be the fitteſt for this purpoſe, any of the men on deck could eaſily ſhift it about as the wind changed; and as this way of uſing it might be made for a few ſhillings, the expence will be no object to prevent ſhip-owners from giving it a fair trial.

It is ſcarcely neceſſary to mention, that it would do equally well for the ſame purpoſe in canal paſſage-boats, which are often as much incommoded by ſmoke as ſhips are; a mode of conveyance to which in ſome ſituations, elegance is added to eaſe and cheapneſs, and which well deſerves farther extenſion on the Engliſh canals.

The manner in which the wind operates in this apparatus, to cauſe a current of air through the upright tube, is moſt probably the following:

When the conical opening C D, Fig. 2, is turned to the wind, the portion of the current of air which enters this opening, will be compreſſed againſt the ſides of the cone, as it paſſes on, more and more, till it comes to E; and after it has paſſed F, the extremity of the ſmall tube, it will begin to expand itſelf; but as it moves forward while thus expanding, the union of the two motions will cauſe it to form the figure of a cone or conoid, ſomewhat ſimilar to that by which it entered, which is marked by the dotted lines terminating at G and H.

This conical blaſt ſtriking obliquely the air before it in the tube B, will cauſe it to move forward only, its lateral motion being prevented by the ſides of the tube; and the preſſure of the atmoſphere forcing the air forward at A, to replace what has been removed at B by the conical blaſt, will of courſe cauſe the current of air from A through B already mentioned.

In ſituations where coals may be had cheap, ſteam may be made to operate on the blaſt ventilator inſtead of wind, for many uſeful purpoſes, by a contrivance ſomewhat ſimilar to the eolipile; with ſome alteration as to its form, but preſerving the ſame principle; particularly ſteam might be uſed with this inſtrument ſo as to force air on a blaſt furnace, and would do beſt for ſmelting iron, where ſome degree of dampneſs in the blaſt is rather beneficial.

To deſcribe the apparatus minutely for this purpoſe, would ſwell this paper to too great a bulk; but of its poſſibility I could eaſily convince any one, and would engage to conſtruct it, in ſuch a ſituation as before mentioned, at an expence comparatively trifling to any other mode of cauſing a blaſt, where a fall of water could not be procured.

Even where fuel is not very cheap, yet ſteam may be applied to the purpoſe of ventilation with this inſtrument, in the manner hinted at here, to great advantage, in thoſe ſituations where labour is difficult to be procured, or inconvenient to be uſed; or where habits and prejudice operate ſtrongly againſt working manual ventilators. This laſt is too frequently the caſe aboard men of war, and other ſhips, though in no other place is ventilation ſo neceſſary to preſerve health, and ſecure proviſions from decay.

An apparatus for ventilating ſhips in this manner might eaſily be fixed beſide the ſhip's boiler, to be uſed in calms, when the wind would not operate on the inſtrument before deſcribed, and which at other times might be applied to boil water for various uſes; and the ſhip's boiler itſelf might be ſo adjuſted as to ſerve for this purpoſe, without preventing or impeding its uſe in preparing proviſions.

To conclude, I will juſt mention a matter this ſubject has ſuggeſted of ſome importance, though ſomewhat differing from what has already been treated of.

From obſerving the effects of the conical opening in the blaſt ventilator, it occurs, that walls might be ſo conſtructed about a windmill, as to operate in a ſimilar manner, and that thus the wind might be conducted with increaſed force, to operate in any direction required on a fixed wind wheel, though it blew from various directions; and that a windmill thus conſtructed, would have the additional advantage of being capable to be regulated or ſtopped at pleaſure, in a manner ſomewhat ſimilar to a water-mill. This mode of conſtructing thoſe mills would prevent many fatal accidents; and would not coſt ſo much as might at firſt ſeem, as the granaries, and other buildings annexed to them, might be ſo arranged, as in a great meaſure to anſwer the purpoſe of the walls propoſed to be built *.

III.

Experiments on Whinſtone and Lava. By Sir JAMES HALL, *Bart. F. R. S. & F. A. S. Edin* †.

THE experiments deſcribed in this paper were ſuggeſted to me many years ago, when employed in ſtudying the *geological* ſyſtem of the late Dr. Hutton, by the following plauſible objection, to which it ſeems liable.

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* On the ſubject of this paper, ſee Phil. Journ. I. II. Art. Venturi. N.

† Read before the Royal Society of Edinburgh, and communicated by the author. An abridged account of the contents of this paper was given in our Journal II, 285.

Granite, porphyry, and baſaltes, are ſuppoſed, by Dr. Hutton, to have flowed in a ſtate of perfect fuſion into their preſent poſition; but their internal ſtructure, being univerſally rough and ſtony, appears to contradict this hypotheſis; for the reſult of the fuſion of earthy ſubſtances, hitherto obſerved in our experiments, either is glaſs, or poſſeſſes, in ſome degree, the vitreous character.

This objection, however, loſes much of its force, when we attend to the peculiar circumſtances under which, according to this theory, the action of heat was exerted. Theſe ſubſtances, when in fuſion, and long after their congelation, are ſuppoſed to have occupied a ſubterraneous poſition far below what was then the ſurface of the earth; and Dr. Hutton has aſcribed to the modification of heat, occaſioned by the preſſure of the ſuperincumbent maſs, many important phenomena of the mineral kingdom, which he has thus reconciled to his ſyſtem.

One neceſſary conſequence of the poſition of theſe bodies, ſeems, however, to have been overlooked by Dr. Hutton himſelf; I mean, that, after their fuſion, they muſt have cooled very ſlowly; and it appeared to me probable, on that account, that, during their congelation, a cryſtallization had taken place, with more or leſs regularity, producing the ſtony and cryſtallized ſtructure, common to all unſtratified ſubſtances, from the large grained granite, to the fine grained and almoſt homogeneous baſalt. This conjecture derived additional probability from an accident ſimilar to thoſe formerly obſerved by Mr. Keir, which had juſt happened at Leith: a large glaſs-houſe pot, filled with green bottle glaſs in fuſion, having cooled ſlowly, its contents had loſt every character of glaſs, and had completely aſſumed the ſtony ſtructure.

Theſe views made part of a paper which I had the honour of laying before this Society in 1790[*]; and about the ſame time I determined to ſubmit my opinions to the teſt of experiment. I communicated this intention to all my friends, and in particular to Dr. Hutton; from him, however, I received but little encouragement. He was impreſſed with the idea, that the heat to which the mineral kingdom has been expoſed was of ſuch intenſity, as to lie far beyond the reach of our imitation, and that the operations of nature were performed on ſo great a ſcale, compared to that of our experiments, that no inference could properly be drawn from the one to the other. He has ſince expreſſed the ſame ſentiments in one of his late publications, (*Theory of the Earth, vol. I. p.* 251.), where he cenſures thoſe who "judge of the great operations of the mineral kingdom, from having "kindled a fire, and looked into the bottom of a little crucible."

But, notwithſtanding my veneration for Dr. Hutton, I could not help differing from him on this occaſion: for, granting that theſe ſubſtances, when in fuſion, were acted upon by a heat of ever ſo great intenſity, it is certain, nevertheleſs, that many of them muſt have congealed in moderate temperatures, ſince many are eaſily fuſible in our furnaces; for it is impoſſible that a ſubſtance ſhould congeal at a higher point than that at which it may afterwards be melted. If, then, theſe phenomena depend upon the circumſtances of congelation, the imitation of the natural proceſs is an object which may be purſued with rational expectation of ſucceſs: and could we ſucceed in a few examples on a ſmall ſcale, and with eaſily fuſible ſubſtances, we ſhould be entitled to extend the theory, by analogy, to ſuch as, by their bulk, or by the refractory nature of their compoſition, could not be ſubjected to our experiments. It is thus that the aſtronomer, by obſerving the effects of gravitation on a little pendulum, is enabled to eſtimate the influence of that principle on the heavenly bodies, and thus to extend the range of accurate ſcience to the extreme limits of the ſolar ſyſtem.

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[*] Particular reaſons induced me not to publiſh this paper at full length; but, wiſhing to preſerve a record of ſome opinions peculiar to myſelf which it contained, I introduced a ſhort abſtract of it into the Hiſtory of the Tranſactions.

Encouraged by this reaſoning, I began my projected ſeries of experiments in the courſe of the ſame year (1790), with very promiſing appearances of ſucceſs. I found that I could command the reſult which had occurred accidentally at the glaſs-houſe; for, by means of ſlow cooling, I converted bottle glaſs, after fuſion, into a ſtony ſubſtance, which again, by the application of ſtrong heat, and ſubſequent rapid cooling, I reſtored to the ſtate of perfect glaſs. This operation I performed repeatedly with the ſame ſpecimen, ſo as to aſcertain that the character of the reſult was ſtony or vitreous, according to the mode of its cooling.

Some peculiar circumſtances interrupted the proſecution of theſe experiments till laſt winter, when I determined to reſume them. Deliberating on the ſubſtance moſt proper to ſubmit to experiment on this occaſion, I was decided by the advice of Dr. Hope *, well known by his diſcovery of the earth of ſtrontites, to give the preference to whinſtone.

The term whinſtone, as uſed in moſt parts of Scotland, denotes a numerous claſs of ſtones, diſtinguiſhed in other countries by the names of baſaltes, trap, wacken, grünſtein and porphyry. As they are, in my opinion, mere varieties of the ſame claſs, I conceive that they ought to be connected by ſome common name, and have made uſe of this, already familiar to us, and which ſeems liable to no objection, ſince it is not confined to any particular ſpecies †.

The following experiments were performed with various kinds of whinſtone, and have likewiſe been extended to lava. To inveſtigate the relation between theſe two claſſes of ſubſtances, ſeems, in the preſent ſtate of geology, an object of conſiderable importance; for they reſemble each other in ſo many reſpects, that we are naturally led to aſcribe the

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* In the courſe of laſt winter, when I firſt thought of reſuming my experiments, I propoſed to this gentleman, that, in imitation of a practice, common in the Academy of Sciences of Paris, we ſhould perform them in company. To this propoſal he cheerfully agreed; but, before any experiments had been begun, he found himſelf ſo much occupied by profeſſional duties, that he could not beſtow upon the ſubject the time which it neceſſarily required; and we gave up the idea of working in company.

† In characteriſing the particular ſpecimens, I have adopted, with ſcarcely any variation, deſcriptions drawn up by Dr. Kennedy, whoſe name I ſhall have occaſion frequently to mention in the courſe of this paper. In the employment of terms, we have profited by the advice of Mr. Deriabin, a gentleman well verſed in the language of the Wernerian ſchool.

formation of both to the ſame cauſe, and to believe that whinſtone, as well as lava, has been expoſed to the action of heat. In the courſe of the paper, I ſhall mention ſeveral accidental reſults, which, if conſidered ſeparately, might ſeem unworthy of notice, but which, by affording the means of compariſon between the two claſſes, are of great ſervice in the general inveſtigation.

The whinſtone firſt employed was taken from a quarry [*] near the Dean, on the water of Leith, in the neighbourhood of Edinburgh. This ſtone is an aggregate of black and greeniſh-black hornblend, intimately mixed with a pale reddiſh-brown matter, which has ſome reſemblance to felſpar, but is far more fuſible. Both ſubſtances are imperfectly and confuſedly cryſtallized in minute grains. The hornblend is in the greateſt proportion; and its fracture appears to be ſtriated, though in ſome parts foliated; that of the reddiſh-brown matter is foliated. The fracture of the ſtone *en maſſe* is uneven, and it abounds in ſmall facettes, which have ſome degree of luſtre. It may be ſcratched, though with difficulty, by a knife, and gives an earthy ſmell when breathed on. It frequently contains ſmall ſpecks of pyrites.

On the 17th of January 1798, I introduced a black lead crucible, filled with fragments of this ſtone, into the great reverberating furnace at Mr. Barker's iron foundery. In about a quarter of an hour, I found that the ſubſtance had entered into fuſion, and was agitated by a ſtrong ebullition. I removed the crucible, and allowed it to cool rapidly. The reſult was a black glaſs, with a tolerably clean fracture, interrupted however by ſome ſpecks.

In ſubſequent experiments, I endeavoured, by ſlow cooling after fuſion, to prevent the whinſtone from becoming vitreous, and to compel it to reſume its original character by cryſtallization. In this I ſo far ſucceeded as to obtain a ſubſtance, which was not glaſs, though it did not poſſeſs the properties of whinſtone. The production of this intermediate ſubſtance, which much reſembled the liver of an animal, is accompanied with ſome curious particulars, which I ſhall enumerate and explain in another part of this paper. On ſome occaſions, too, I obtained a vitreous maſs in which were a multitude of little ſpheres, having a dull or earthy fracture.

At laſt, on the 27th of January, I ſucceeded completely in the object I had in view. A crucible, containing a quantity of whinſtone, melted in the manner above deſcribed, being removed from the reverberatory, and conveyed rapidly to a large open fire, was immediately ſurrounded with burning coals, and the fire, after being maintained ſeveral hours, was allowed to go out. The crucible, when cold, was broken, and was found to contain a ſubſtance, differing in all reſpects from glaſs, and in texture completely reſembling whinſtone. Its fracture was rough, ſtony and cryſtalline; and a number of ſhining facettes were interſperſed through the whole maſs [†]. The cryſtallization was ſtill more apparent in cavities produced by air bubbles, the internal ſurface of which was lined with diſtinct cryſtals *.

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[*] Called Bell's Mills Quarry.

[†] It contained a number of ſmall globules like lead ſhot, which were found to conſiſt of regulus of iron, reduced from the oxide in the whinſtone, by means of the carbonic matter of the black lead crucible.

Having ſhewn this reſult to ſeveral of my friends, Dr. Hope regretted that the ſubſtance, previouſly to its artificial cryſtallization, had not been reduced to the ſtate of ſolid glaſs; ſince the adverſaries of the ſyſtem might alledge, that, during the action of heat, the original cryſtallized texture of the ſtone had never been completely deſtroyed. Being convinced of the propriety of this obſervation, I determined, in future, to reduce the ſtone firſt to glaſs, and to perform the cryſtallization after a ſecond fuſion.

For this purpoſe, with the aſſiſtance of Dr. Kennedy, to whoſe co-operation I am greatly indebted for the ſucceſs of all the following experiments, I reduced a quantity of the ſame whinſtone to moſt perfect black glaſs. A crucible, filled with fragments of this glaſs, being then expoſed to a heat, which, from previous trials, was judged to be more than ſufficient to reduce its contents to fuſion, the fire was very gradually lowered till all was cold. I thus expected to obtain a reſult ſimilar to that laſt mentioned, but found, to my great ſurpriſe, that the fragments had never been in complete fuſion, ſince they ſtill, in a great meaſure, retained their original ſhape. This extraordinary fact, which afterwards led to the diſcovery of ſome curious properties of whinſtone, will be fully accounted for in a ſubſequent part of the paper.

Another portion of the ſame glaſs being perfectly melted by a very ſtrong heat, the temperature was reduced to about 28 of Wedgwood, and was maintained at that pitch during ſix hours. The reſult was a perfectly ſolid maſs, cryſtallized to a certain depth from the outſide, though ſtill vitreous in the heart. In another experiment, performed like the laſt in all reſpects, except that the heat was maintained at 28 during twelve hours, I obtained a maſs entirely cryſtalline and ſtony throughout, with facettes appearing in the ſolid parts, and ſmall cryſtals ſhooting into ſome of the cavities.

Soon after I had communicated theſe reſults to Dr. Hope, he performed, with complete ſucceſs, an experiment ſimilar to the firſt, in which I had obtained a cryſtallized ſubſtance, by the gradual cooling of the melted ſtone. The ſame was likewiſe, ſoon afterwards, performed by Mr. Boſwell of Auchinleck.

My experiments, already deſcribed, were confined to one ſpecies of whinſtone; but have ſince been extended to ſix other varieties. They were all firſt reduced to glaſs by the application of a ſtrong heat, and ſubſequent rapid cooling. After a ſecond fuſion they

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By this reduction, as a portion of its iron was withdrawn from the maſs, and as the relative proportion of the component elements was thus varied, the nature of the original ſubſtance could not fail to be changed. I determined, on this account, to lay aſide black lead crucibles in future. The propriety of this has ſince been fully proved; for the firſt reſult is found to differ eſſentially from thoſe obtained in all my ſubſequent experiments, which were performed with Heſſian crucibles, or with others, which, like them, contain no ſenſible quantity of carbonic matter.

* I ſhowed this reſult at a meeting of the Society on 5th of February.

were cryſtallized, by being kept long in a ſtationary temperature, between 28 and 30. This laſt operation was beſt performed in a long and narrow muffle, wholly ſurrounded with burning coals, according to a practice long followed by Dr. Kennedy, by which the heat could be maintained with ſo great ſteadineſs as to render the reſult almoſt certain.

The fuſibilities were determined in an open muffle, in which a fragment of the ſubſtance under trial was placed contiguous to a pyrometer piece. As ſoon as the fragment, in conſequence of the gradual riſe of heat, had ſo far ſoftened as to yield to the touch of a bent iron rod, the pyrometer was removed and meaſured. The fuſibilities, thus obtained, in degrees of Wedgwood's ſcale *, have been ſtated in a table, to which I would be underſtood always to refer. I have diſtinguiſhed the cryſtallized ſubſtances, obtained from the glaſſes, by the name of *cryſtallite*, a term ſuggeſted by Dr. Hope. It may be obſerved in this table, that the original whins ſoften in a range from 38 to 55; the glaſſes from 15 to 24, and the artificial cryſtallites from 32 to 45.

No. 1. *Whin of Bell's Mills Quarry.*

This ſtone was the ſubject of all the foregoing experiments, which were frequently repeated with ſucceſs on a large ſcale.

In trying the fuſibility of the glaſs obtained from it, a curious circumſtance occurred, which accounts for the unexpected reſults already mentioned. I had placed in the muffle a long and ſlender fragment of this glaſs, with its extremities reſting on two ſupports of clay, and its middle unſupported. Having then increaſed the temperature by ſlow degrees, I expected to diſcover the loweſt point of emolleſcence, by obſerving when the fragment ſunk by its own weight. The muffle having attained a moderate heat, I obſerved the glaſs to loſe its ſhape a little. Wiſhing to ſee it completely melted, the ſame heat was continued, but no further change took place. The heat was then raiſed ſeveral degrees, but without effect. At laſt, being urged ſtill further, the glaſs ſunk down completely between its ſupports. The pyrometer being then withdrawn, denoted a temperature above 30.

It occurred to me, that, on this occaſion, the glaſs, by the firſt application of heat, had ſoftened, and then had cryſtallized, ſo as to become hard again; that, in cryſtallizing, it had acquired ſuch infuſibility as to yield to no heat under 30. I immediately confirmed this conjecture by the following experiment.

A piece of the ſame glaſs, placed in a cup of clay, was introduced into the muffle, heated to 21. In one minute it became quite ſoft, ſo as to yield readily to the preſſure of an iron rod. After a ſecond minute had elapſed, the fragment, being touched by the rod, was found to be quite hard, though the temperature had remained ſtationary. The ſubſtance, thus hardened, had undergone a change throughout; it had loſt the vitreous character; when broken, it exhibited a fracture like that of porcelain, with little luſtre; and its colour was changed from black to dark brown. Being expoſed to heat, it was found to be fuſible only at 31; that is, it was leſs fuſible than the glaſs by 13 or 14 degrees.

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* The meaſurement of the temperatures may be relied upon as accurate; they were determined by two ſets of pieces, one purchaſed by me during the lifetime of the late Mr. Wedgwood, and the other likewiſe made by him, belonging to Dr. Kennedy. The two ſets correſpond exactly; and Dr. Kennedy's had, at his requeſt, been carefully examined by the preſent Mr. Wedgwood, who found them true by his father's original ſtandard.

Numerous and varied experiments have ſince proved, in the cleareſt manner, that, in any temperature, from 21 to 28 incluſive, the glaſs of this whin paſſes from a ſoft, or liquid ſtate, to a ſolid one, in conſequence of cryſtallization; which is differently performed at different points of this range. In the lower points, as at 23, it is rapid and imperfect; in higher points, ſlower and more complete, every intermediate temperature affording an intermediate reſult. I likewiſe found, that cryſtallization takes place, not only when the heat is ſtationary, but likewiſe when riſing or ſinking, provided its progreſs through the range juſt mentioned is not too rapid. Thus, if the heat of the ſubſtance, after fuſion, exceeds one minute in paſſing from 21 to 23, or from 23 to 21, the maſs will infallibly cryſtallize, and loſe its vitreous character.

Theſe facts enabled me to account for the production of the ſubſtance reſembling the liver of an animal, which I obtained in my firſt attempts to cryſtallize the melted ſtone. Not being then aware of the temperature proper for complete cryſtallization, I had allowed it to be paſſed over rapidly by the deſcending heat, and I had begun the ſlow cooling in thoſe lower points, at which the formation of this intermediate ſubſtance takes place.

By the ſame means I was enabled to explain the other unexpected reſult, which I obtained in endeavouring to convert the glaſs of this ſtone into cryſtallite. The fire applied to the crucible, containing fragments of the glaſs, had been raiſed very ſlowly, which I know to have been the caſe by ſome circumſtances of the experiment. The glaſs had ſoftened by the firſt application of heat, but had cryſtallized again as the heat gradually roſe; ſo that the ſubſtance conſolidated, while ſtill ſo viſcid as to retain the original ſhape of the fragments; at the ſame time it acquired ſuch infuſibility as to reſiſt the application of higher degrees of heat during the reſt of the proceſs.

No. 2. *Whin of the Rock of Edinburgh Caſtle.*

This is a baſalt of a blackiſh blue colour. Its grain is fine, and its fracture uneven, partaking of the ſplintery. It is in general homogeneous, although, in ſome pieces, a very few minute cryſtals of hornblend are perceptible. It has ſome luſtre, from a number of ſmall ſhining facettes; has an earthy ſmell when breathed on; and gives fire ſlightly with ſteel.

The pure glaſs which this whin yielded, by rapid cooling after a moderate heat, was cryſtallized in three experiments, and produced maſſes greatly reſembling the original. In one of theſe, formed on a large ſcale in the glaſs-houſe, the reſemblance is ſo ſtrong, both as to colour and texture, that it would be difficult, or perhaps impoſſible, to diſtinguiſh them, But for a few minute air bubbles viſible in the artificial cryſtallite. The glaſs is leſs fuſible than that of No. 1. and ſeems not to poſſeſs the property of producing the liver cryſtallite.

No. 3. *Whin of the Baſaltic Columns on Arthur's Seat, near Edinburgh.*

Its baſis is a baſalt of a dark grey colour, and uneven fracture. It contains numerous laminar cryſtals of felſpar, which ſeem to be almoſt colourleſs, and have conſiderable luſtre and tranſparency. It alſo contains ſome black hornblend. It has an earthy ſmell when breathed on, and gives ſparks ſlightly with ſteel.

In the temperature of 100, or upwards, the whole was changed to pure black glaſs; but in a more moderate heat, (about 60), the felſpar remained unchanged, while the hornblend diſappeared, and formed a glaſs along with the baſis of the ſtone. Both kinds of glaſs yielded highly characteriſed cryſtallites; that laſt mentioned, having its felſpars entire, produced a ſubſtance like porphyry, in which the white felſpars were embedded in a black cryſtalline baſis. The cryſtals formed in this baſis are ſo complete in one example, that they are ſeen projecting into the cavities, and ſtanding erect on the external ſurface, ſo as to make it ſparkle all over. Theſe black cryſtals ſeem to be hornblend of new formation. We have found, by ſome late experiments, that they are conſiderably more refractory than the cryſtallite in which they lie, and are equally infuſible with ſome ſpecies of natural hornblend.

No. 4. *Whin from the Neighbourhood of Duddingſtone Loch.*

It has for its baſis a black baſalt of an uneven fracture. In it are embedded augit in numerous cryſtals, felſpar in a ſmaller proportion, and diſperſed grains of olivin. The felſpar ſeems to be greeniſh-white, with conſiderable luſtre and tranſparency. The ſtone gives fire with ſteel, and has a ſlight earthy ſmell when breathed on. Its glaſs yields a fine grained cryſtallite, like that of No. 1.

No. 5. *Whin of Saliſbury Craig near Edinburgh.*

This ſpecies is an aggregate of black hornblend, and of a greeniſh-white matter, both in minute grains. The greeniſh-white matter reſembles felſpar, but is much more fuſible. The general characters are nearly the ſame with thoſe of the ſpecimen already deſcribed, No. 1. It has conſiderable luſtre, chiefly from the hornblend; an earthy ſmell when breathed on; and gives ſome ſparks with ſteel. Its glaſs yielded a highly facetted cryſtallite, approaching to the ſtructure of the original whinſtone, No. 4.

No. 6. *Whin from the Water of Leith.*

It is found in great blocks in the bed of the river, and has been brought there no doubt from a maſs of the ſame kind in the mountains above. In conſiſts of black hornblend, and of a whitiſh matter reſembling felſpar, as in No. 1. and No. 5. Theſe two ſubſtances are nearly in equal proportion, and are confuſedly and imperfectly cryſtallized in minute maſſes. If the whitiſh ſubſtance were felſpar, this ſtone, as well as that laſt mentioned, would be the grünſtein of Werner; but this white ſubſtance is far more fuſible than felſpar, and melts at a lower heat than the hornblend, with which it is mixed. It has an earthy ſmell when breathed on, and may be ſcratched with difficulty by a knife.

In fuſion and cryſtallization it reſembled the other whins. A fragment ſimilar to this in all reſpects, which I found in the neighbourhood of Edinburgh, manifeſted ſo ſtrong a diſpoſition to cryſtallize, that, though cooled in the open air after fuſion, it was found ſtony in the heart, with a vitreous outſide. When cryſtallized, however, with every precaution, it yielded no remarkable reſult.

No. 7. *Whin of the Balſaltic Columns of Staffa.*

I received this ſpecimen from a gentleman who broke it from the original rock. It is baſalt of a bluiſh-black colour. It is fine grained and homogeneous; and its fracture is uneven. It has a ſmall degree of luſtre, from a number of minute ſhining points perceptible in a ſtrong light. It gives an earthy ſmell when breathed on, and may be ſcratched with difficulty by a knife. It yielded a perfect and very hard glaſs, which, in a regulated heat, produced a uniform ſtony cryſtallite, greatly reſembling the original.

It has thus been ſhown, that all the whins employed aſſume, after fuſion, a ſtony character, in conſequence of ſlow cooling; and the ſucceſs of theſe experiments, with ſo many varieties, entitles us to aſcribe the ſame property to the whole claſs. The arguments, therefore, againſt the ſubterraneous fuſion of whinſtone, derived from its ſtony character, ſeem now to be fully refuted.

Experiments on Lava.

In the inveſtigation of Dr. Hutton's ſyſtem, great advantage may be expected from an examination of lavas. They have undoubtedly flowed on the ſurface by means of heat; and whinſtone, according to his hypotheſis, having flowed in the bowels of the earth by the influence of the ſame agent, the two claſſes ought to poſſeſs many properties in common, by which the hiſtory of both may be illuſtrated.

I have been enabled to inſtitute a compariſon between them, by means of a cabinet of volcanic productions which I collected in 1785, in company with Dr. J. Home of this Society, on Veſuvious, Ætna, and the Lipari Iſles. On this occaſion we were greatly aſſiſted by the celebrated M. Dolomieu*, who accompanied us in part of our expedition. This author complains, in his writings, that travellers, in collecting volcanic productions, have brought away only the ſuperficial ſcoria of lavas, which nearly reſemble each other in all caſes, and convey no idea of the real character of the lava, which can only be ſeen in the interior parts of the currents. In forming our collection we ſcrupulouſly avoided this error, and choſe ſuch ſpecimens only as were the moſt compact and free from the ſcorified appearance of the ſurface.

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* Though I differ widely from this gentleman in many of his theoretical opinions, I cannot too ſtrongly expreſs my admiration of his merit as a natural hiſtorian. His deſcriptions of countries, as well as of minerals, preſent the moſt lively repreſentations to the mind of the reader, which, in the numerous inſtances I have witneſſed, are perfectly correct.

When theſe ſolid lavas are compared with our whinſtones, the reſemblance between the two claſſes is not only ſtriking at firſt ſight, but bears the cloſeſt examination. They both conſiſt of a ſtony baſis, which frequently contains detached cryſtals of various ſubſtances, ſuch as white felſpar and black hornblend. The analogy between the two claſſes ſeems to hold through all their varieties; and I am confident that there is not a lava of Mount Ætna to which a counterpart may not be produced rom the whinſtones of Scotland.

This reſemblance in external character is accompanied with an agreement no leſs complete in chemical properties. But before I mention the experiments which tend to prove this agreement, it will be neceſſary firſt to examine the opinion of two very celebrated authors concerning lavas. M. Dolomieu and Mr. Kirwan, though they differ widely in many reſpects, agree in believing, that lavas have never been acted upon by heat of ſufficient intenſity to produce complete fuſion; and endeavour, each by an hypotheſis peculiar to himſelf, to account for their fluidity. The opinion of theſe gentlemen is of ſuch importance in the preſent queſtion, and the arguments they have uſed are ſo extraordinary, that I muſt beg leave to quote their words at full length.

M. Dolomieu ſtates his opinion in the following paſſage, (*Iſles Ponces*, p. 7.): " Il eſt " eſſentiel de conſtater, par beaucoup d'exemples et d'obſervations, quelques vérités que " j'ai annoncées il y a pluſieurs années, ſavoir, que le feu des volcans ne dénature pas ordi- " nairement les pierres qu'il a miſes en état de fuſion; qu'il ne les altere pas au point de " ne pouvoir les reconnoître, de ne pas diſtinguer quelle a pu être la baſe des laves; que " ce feu agit différemment que le feu de nos fourneaux, tel que nous l'employons dans la " chimie et dans les arts; qu'il produit dans les laves une fluidité qui n'a aucun rapport " avec la fluidité vitreuſe, que nous opérons, lorſque nous traitons à grand feu les mêmes " matières qui leur ſervent de baſe, et lorſque nous voulons rendre aux laves elles mêmes " leur fluidité. Celui des volcans n'a point d'intenſité; il ne peut pas même vitrifier les " ſubſtances les plus fuſibles, tels que les ſchorls, qui ſe trouvent comme parties conſti- " tuantes dans l'intérieur des laves; il produit la fluidité par une eſpece de diſſolution par " une ſimple dilation, qui permet aux parties de gliſſer les unes ſur les autres, et peutêtre " encore par le concours d'une autre matière qui ſert de véhicule à la fluidité."

Mr. Kirwan cenſures this ſuppoſition as ſtrange and inconceivable; but in my opinion, that which he has brought forward is not leſs ſo. In the *Elements of Mineralogy, ſecond edition*, vol. I. p. 396. he ſays: "Now, there are but three ſorts of fuſion with which we are ac- " quainted: that which produces porcelain; that which produces enamels and ſemi-vitri- " fications; and that which produces glaſs. By inſpecting lava we ſhall find that very " little of it has been in any of theſe ſtates; ſince therefore it has flowed, it is plain it has " derived its liquifaction not from the fuſion of its own materials, but from that of ſome

" foreign ſubſtance mixed it. This fact is ſo plain, that it has even ſtruck M. Dolomieu, " in the midſt of his prepoſſeſſions in favour of ſome ſtrange inconceivable power, which " he attributes to volcanic fire, of melting earthy ſubſtances, without effecting an alteration " in their ſenſible qualities. " I hope," ſays he, " to prove, that lavas contain, in their " interior, a combuſtible matter, which burns and conſumes in the ſame manner as other " inflammables," *Iſles Ponces*, 10. Yet he neglects telling us what this matter is; though " it plainly appears to be no other than ſulphur and bitumen, of which an immenſe quan" tity is found in all volcanos, which liquefies in a low degree of heat, and cauſes all the " ſtony ſubſtances to flow that are immerſed in it."

(*To be continued.*)

IV.

On the Hydraulic Machine erected at the Mines of Schemnitz in Hungary.

To Mr. NICHOLSON.

SIR,

THE early inſertion of the extract which I ſent you from the Micrographia of Robert Hooke in your laſt Number, encourages me to requeſt you will give ſome account, with your opinion, of a machine for raiſing water at the mines in Hungary, upon the principle of the fountain of Hiero. It is ſlightly mentioned in many elementary books, but I cannot point out the work in which it was originally publiſhed, nor ſay thing more than that I believe it has not been accurately deſcribed in any Engliſh work.

I am, &c.

R. B.

London, Feb. 20, 1800.

The account was firſt given to the Royal Academy at Paris by their correſpondent, M. Jars, and inſerted in their Memoirs for the year 1768. The machine was executed in the year 1755, and uſed to raiſe the water of a ſhaft named *Amalie*, in the mines at Schemnitz. Fig. 1, Plate 2, is copied from the engraving of Jars. It is placed at the depth of forty toiſes beneath the ſurface of the ground above. The wooden trough A is a kind of reſervoir at the end of a channel which conveys the water by which the machine is worked. The pipes are not drawn in the proportion of their lengths, but contracted to the ſpace of the deſign. B B conveys the water from it into the reſervoir D, which is twenty-two toiſes lower than A. This pipe deſcends very nearly to the bottom of D, as is ſhewn by the dotted line, with the intention that the included air ſhall be forced to aſcend and paſs through the pipes H into another reſervoir I ſtill lower. The pipes N, through which the ſubterraneous waters are raiſed, are ſixteen toiſes in height. They begin very near the bottom of the veſſel I, in order that the air from that reſervoir may not eſcape until all the water has been raiſed.

The pipes B and N are four and a half (French) inches in diameter within; but thoſe H for the air are only three inches in diameter at iſſuing from the reſervoir D, and two and a half when they enter the reſervoir I, and this alſo is the diameter of the pipe P.

The wooden trough L is the termination of a trough or channel from another engine, which raiſes the waters from a yet greater depth. From this proceeds a pipe with a cock K, in order that the water from L may either be admitted into I, or intercepted at pleaſure.

The reſervoir D, which, as well as I, is made of an alloy of copper and tin, is of double the capacity of I. It is eight feet and a half high, and five feet diameter inſide meaſure, and its thickneſs is two inches. The lower reſervoir I is four feet in diameter, and ſix feet and a half high inſide meaſure, and its thickneſs one inch and a half. Each of theſe veſſels was caſt in three pieces, as the drawing ſhews, which are joined by flanches and ſcrews, a ring of lead and another of leather being placed between, to ſecure the joint, and prevent the tranſmiſſion of any fluid. M. Jars obſerves, that the pipes would have been better if connected in the ſame manner; but the practice is to drive the ends of the pipes into hollow cylinders of very dry wood, iron bound with three hoops. Theſe anſwer very well, and are of conſiderable durability.

The cocks C, E, K are ſcrewed in their places by caps or covers faſtened down with ſcrews.

When the machine is to be put into action, all the cocks are cloſed, and as the reſervoir A is always full, the pipe B is alſo full, as far as the cock C. The reſervoir L is alſo conſtantly full of water from the mine which is to be raiſed ſixteen toiſes to O. For this purpoſe the cock K is opened, and the water flows into I, the air being ſuffered to eſcape from that veſſel by turning the cock M. The veſſel is known to be full by the emiſſion of water through P; at which inſtant both the cocks K and M are cloſed. Immediately afterwards the cocks C and G are opened, and the water from A entering B, compreſſes the included air, and forces it through the pipes H, where it preſſes on the ſurface of the water contained in the inferior reſervoir I, and forces that fluid to aſcend through N to O, or the Adit through which it is diſcharged. This water being raiſed, and the reſervoir I being empty, the cocks C and G are ſhut, and E and F is then opened, the firſt to ſuffer the contents of D to flow off, and the ſecond to accelerate the diſcharge, by admitting the external air. Both theſe cocks are again cloſed as ſoon as the evacuation is completed. During this laſt operation, another man below opens the cocks K and M, by which the included air iſſues with great force through P, and the water from L again fills the reſervoir I as at firſt. The operations of cloſing K and M, and opening C and G, being therefore again repeated, the contents of I are again forced up to, and thus the engine may be kept continually at work.

Each alternation employs about three minutes, and raiſes between twenty-nine and thirty cubic feet of water, which, in the courſe of the natural day, amounts to twelve or thirteen thouſand cubic feet of water, raiſed by the fall of about double the quantity through a ſomewhat greater height. Two men * are required to attend it; but M. Jars ſeems to think, that on account of the ſimplicity and cheapneſs of its conſtruction, and the little wear and tear, together with the facility with which it may be made to work and ſtop for very ſhort periods of time, it would be of great value in ſuch places as afford the requiſite fall of ſuperior water, and do not require a higher ſingle lift than fifteen or twenty toiſes (or fathoms.)

The fall of twenty-two toiſes given to the ſuperior water is not neceſſary for raiſing the column of ſixteen, though it is profitable, by increaſing the velocity of aſcent. But it was at firſt intended to force the lower water as high as twenty-one toiſes, which could not be performed with the apparatus then ſet up. For at the very firſt introduction of water into the veſſel D, it burſt with an effect which ſufficiently ſhewed the power of ſuch a column of water compreſſing a maſs of air which, by its elaſticity, was diſpoſed to give a a vibratory or jerking motion to the agent.

When the machine is near the end of its action, that is to ſay, when nearly the whole of the water has been raiſed out of the reſervoir I, if the cock M be opened to give vent to the compreſſed air, and a hat, or miner's bonnet, be preſented to the aperture P, the aqueous vapours diſperſed through the compreſſed air, and perhaps alſo, ſays M. J. part of thoſe of the external air, are condenſed on the bonnet, in the form of very white and compact ice, very much reſembling hail, and not eaſily ſeparated from the bonnet: it ſoon melts, which is not to be wondered at, as the temperature of the place itſelf is not cold. Meſſrs. du Hamel and Jars remained in Hungary from January to July, 1758, and obſerved the ſame phenomenon at all ſeaſons; but as they had not a thermometer, they could not make a number of experiments, which might have been of value in the inveſtigation of the ſubject.

It is obſervable, that the air iſſues out with ſuch impetuoſity, that the workman could not hold the bonnet at the diſtance of a few inches from the aperture, as he does in this experiment, if he were not ſupported behind. The ice is much more compact if the cock be only in part opened.

On the above, which is tranſlated from the Memoir of M. Jars, with very little abridgement, I have ſcarcely any additional remark to make. The utility of this engine, under the circumſtances here ſtated, is ſufficiently clear, and its applications might become more extenſive, if the improvements ſuggeſted in the preceding note were added. In particular, where a ſtream flows with the poſſibility of carrying off part of the water by a deep

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* It ſeems very eaſy to connect the levers of the cocks above and below, ſo as to require only one man to work the whole ſet: and, indeed, there would be little difficulty in making the machine work itſelf ſafely without any attendant except to ſet it off at firſt, or ſtop it when requiſite.---N.

ſhaft or drain, it would be practicable by the fall of part to raiſe the remainder to the top of an houſe or building for domeſtic or other purpoſes. In this caſe it would be requiſite to place the firſt veſſel D at the bottom of the ſhaft, and the ſecond I at the level of the ſtream, and the operation of this inverted engine would force water nearly as high above that level as the depth of the ſhaft might be below it.

If the aſcending and deſcending columns were of equal length, they would be in equilibrio, and no more water would riſe than juſt to fill the lower tube. The velocity of aſcent, ſuppoſing ſufficient water way, and other circumſtances, ſuch as the inflexions of the pipes, to be properly diſpoſed, will depend on the exceſs of length in the deſcending column, and this exceſs is capable of a maximum relative to the quantity raiſed to a given height, or as engineers call it, the effect. The longer the lift, the leſs the quantity raiſed, ſuppoſing the upper reſervoir to be of a given magnitude, the denſities of air being inverſely as the preſſures, or length of the preſſing column, and the quantity raiſed being equal to that of the air in its compreſſed ſtate. Whence it follows, that the whole quantity raiſed will be leſs the fewer the lifts.

I do not underſtand the hypotheſis upon which this machine was conſtructed. The reſervoirs have their capacity as 2 to 1. But the air ſubjected to the re-action of a column of 16 toiſes, beſides the common preſſure, would ſuſtain the preſſure of four atmoſpheres, and therefore be condenſed into one-fourth the ſpace. If there be no fallacy in this plain remark, it muſt follow, that the lower veſſel I was only half emptied when the ſtream ceaſed to be afforded at O, and, conſequently, the effect was only half what is here ſtated.

That air condenſed to one-fourth ſhould take up and diſſolve more water than in its ordinary rare ſtate, and afterwards depoſit it when it recovered its original dimenſions, is conſentaneous with other well known facts: and the modern theoriſts will eaſily apply the doctrine of latent heat, or the increaſed capacity of expanded air, to account for the phenomenon of its robbing the water not only of the heat which maintained its ſtate of elaſtic ſolution, but even that which would have been requiſite to keep up the ſtate of common denſe fluidity.

W. N.

V.

An Account of the Pearl Fiſhery in the Gulph of Manar, in March and April 1797. By Henry J. Le Beck, *Eſq.*

(Concluded from Vol. III. page 547.)

THE diving ſtone is a piece of coarſe granite, a foot long, ſix inches thick, and of a pyramidical ſhape, rounded at the top and bottom. A large hair rope is put through a hole in the top. Some of the divers uſe another kind of ſtone ſhaped like a half moon, to bind.

bind round their belly, ſo that their feet may be free. At preſent theſe are articles of trade at *Condatchey*. The moſt common, or pyramidical ſtone, generally weighs about thirty pounds. If a boat has more than five of them, the crew are either corporally puniſhed or fined.

The diving, both at *Ceylon* and at *Tutucorin*, is not attended with ſo many difficulties as authors imagine. The divers, conſiſting of different caſts and religions, (though chiefly of *Parrawer*[*] and *Muſſelmans*,) neither make their bodies ſmooth with oil, nor do they ſtop their ears, mouths, or noſes with any thing, to prevent the entrance of ſalt water. They are ignorant of the utility of diving bells, bladders, and double flexible pipes. According to the injunctions of the ſhark conjurer they uſe no food while at work, nor till they return on ſhore, and have bathed themſelves in freſh water. Theſe *Indians*, accuſtomed to dive from their earlieſt infancy, fearleſsly deſcend to the bottom in a depth of, from five to ten fathoms in ſearch of treaſures. By two cords a diving ſtone and a net are connected with the boat. The diver putting the toes of his right foot on the hair rope of the diving ſtone, and thoſe of his left on the net, ſeizes the two cords with one hand, and ſhutting his noſtrils with the other, plunges into the water. On reaching the bottom, he hangs the net round his neck, and collects into it the pearl ſhells as faſt as poſſible, during the time he finds himſelf able to remain under water, which uſually is about two minutes. He then reſumes his former poſture, and making a ſignal, by pulling the cords, he is immediately lifted into the boat. On emerging from the ſea, he diſcharges a quantity of water from his mouth and noſe, and thoſe who have not been long enured to diving frequently diſcharge ſome blood; but this does not prevent them from diving again in their turn. When the firſt five divers come up and are reſpiring, the other five are going down with the ſame ſtones. Each brings up about one hundred oyſters in his net, and if not interrupted by any accident, may make fifty trips in a forenoon. They and the boat's crew get generally from the owner, inſtead of money, a fourth of the quantity which they bring on ſhore; but ſome are paid in caſh, according to agreement.

The moſt ſkilful divers come from *Colliſh*, on the coaſt of *Malabar*; ſome of them are ſo much exerciſed in the art, as to be able to perform it without the aſſiſtance of the uſual weight; and for a handſome reward will remain under water for the ſpace of ſeven minutes; this I ſaw performed by a *Caffry* boy, belonging to a citizen at *Karical*, who had often frequented the fiſheries of theſe banks. Though Dr. Halley deems this impoſſible, daily experience convinces us, that by long practice any man may bring himſelf to remain under water above a couple of minutes. How much the inhabitants of the South Sea iſlands diſtinguiſh themſelves in diving we learn from ſeveral accounts; and who will not be ſurpriſed at the wonderful *Sicilian* diver Nicholas, ſurnamed the Fiſh[†]?

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[*] Fiſhermen of the Catholic religion.

[†] According to Kircher, he fell a victim amongſt the *Polypes* in the gulph of *Charybdis*, on his plunging, for the ſecond time, in its dangerous whirlpool, both to ſatiſfy the curioſity of his king, Frederic, and his inclination for wealth. I will not pretend to determine, how far this account has been exaggerated.

Every one of the divers, and even the moſt expert, entertain a great dread of the ſharks, and will not, on any account, deſcend until the conjurer has performed his ceremonies. This prejudice is ſo deeply rooted in their minds, that the government was obliged to keep two ſuch conjurers always in their pay, to remove the fears of their divers. Thirteen of theſe men were now at the fiſhery from *Ceylon* and the coaſt, to profit by the ſuperſtitious folly of theſe deluded people. They are called in *Tamul*, *Pillăl Kadtăr*, which ſignifies one who binds the ſharks and prevents them from doing miſchief.

The manner of enchanting conſiſts in a number of prayers learned by heart, that nobody, probably not even the conjurer himſelf, underſtands, which he, ſtanding on the ſhore, continues muttering and grumbling from ſun riſe until the boats return; during this period, they are obliged to abſtain from food and ſleep, otherwiſe their prayers would have no avail; they are, however, allowed to drink, which privilege they indulge in a high degree, and are frequently ſo giddy, as to be rendered very unfit for devotion. Some of the conjurers accompany the divers in their boats, which pleaſes them very much, as they have their protectors near at hand. Nevertheleſs, I was told, that in one of the preceding fiſheries, a diver loſt his leg by a ſhark, and when the head conjurer was called to an account for the accident, he replied that an old witch had juſt come from the coaſt, who, from envy and malice, had cauſed this diſaſter, by a counter-conjuration, which made fruitleſs his ſkill, and of which he was informed too late; but he afterwards ſhewed his ſuperiority by enchanting the poor ſharks ſo effectually, that though they appeared in the midſt of the divers, they were unable to open their mouths. During my ſtay at *Condatchey*, no accident of this kind happened. If a ſhark is ſeen, the divers immediately make a ſignal, which, on perceiving, all the boats return inſtantly. A diver who trod upon a hammer oyſter, and was ſomewhat wounded, thought he was bit by a ſhark, conſequently made the uſual ſignal, which cauſed many boats to return; for which miſtake he was afterwards puniſhed.

The owners of the boats* ſometimes ſell their oyſters, and at other times open them on their own account. In the latter caſe ſome put them on mats in a ſquare, ſurrounded with a fence; others dig holes of almoſt a foot deep, and throw them in till the animal dies; after which they open the ſhells and take out the pearls with more eaſe. Even theſe ſquares and holes are ſold by auction after the fiſhery is finiſhed, as pearls often remain there, mixed with the ſand.

In ſpite of every care, tricks in picking out the pearls from the oyſters can hardly be prevented. In this the natives are extremely dexterous. The following is one mode they put in practice to effect their purpoſe: when a boat owner employs a number of hired people to collect pearls, he places over them an inſpector of his own, in whom he can confide; theſe hirelings previouſly agree that one of them ſhall play the part of a thief, and bear the puniſhment, to give his comrades an opportunity of pilfering. If one of the gang happens to meet with a large pearl, he makes a ſign to his accomplice, who inſtantly conveys away one of ſmall value, purpoſely, in ſuch a manner as to attract notice. On this the inſpector and the reſt of the men take the pearl from him: he is then puniſhed and turned out of their company. In the mean time, while he is making a dreadful uproar, the real thief ſecures the valuable pearl, and afterwards the booty is ſhared with him who ſuffered for them all. Beſides tricks like theſe the boat owners and purchaſers often loſe many of the beſt pearls, while the dony is returning from the bank; for, as long as the animal is alive and untouched, the ſhells are frequently open near an inch; and if any of them contain a large pearl, it is eaſily diſcovered and taken out by means of a ſmall piece of ſtiff graſs or bit of ſtick, without hurting the pearl fiſh. In this practice they are extremely expert. Some of them were diſcovered whilſt I was there, and received their due puniſhment.

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* Theſe are the individuals which farm one or more boats from the renter; and though they are in poſſeſſion of them only during the fiſhery, they are commonly called the owners of the boats.

Gmelin aſks if the animal of the *mytilus margaritiferus* is an *aſcidia?* See Linn. Syſt. Nat. tom. I. p. vi. 3350. This induces me to believe that it has never yet been accurately deſcribed; it does not reſemble the *aſcidia* of Linnæus, and may, perhaps, form a new genus. It is faſtened to the upper and lower ſhells by two white flat pieces of muſcular ſubſtance, which are called by *Houttuin* * *ears*, and extend about two inches from the thick part of the body, growing gradually thinner. The extremity of each *ear* lies looſe, and is ſurrounded by a double brown fringed line. Theſe lie almoſt the third of an inch from the outer part of the ſhell, and are continually moved by the animal. Next to theſe, above and below, are ſituated two other double fringed moveable ſubſtances, like the branchiæ of a fiſh. Theſe *ears* and *fringes* are joined to a cylindrical piece of fleſh; of the ſize of a man's thumb, which is harder and of a more muſcular nature than the reſt of the body. It lies about the centre of the ſhells, and is firmly attached to the middle of each. This, in fact, is that part of the pearl fiſh which ſerves to open and ſhut the ſhells. Where this column is faſtened, we find on the fleſh deep impreſſions, and on the ſhell various nodes of round or oblong forms, like imperfect pearls. Between this part, and the hinge (*cardo*), lies the principal body of the animal, ſeparated from the reſt, and ſhaped like a bag. The mouth is near the hinge of the ſhell, enveloped in a veil, and has a double flap or lip on each ſide; from thence we obſerve the throat, (œſophagus) deſcending like a thread to the ſtomach. Cloſe to the mouth there is a carved browniſh tongue, half an inch in length, with an obtuſe point; on the concave ſide of this deſcends a furrow, which the animal opens and ſhuts, and probably uſes to convey food to its mouth †. Near its middle are two bluiſh ſpots, which ſeem to be the eyes. In a pretty deep hole near the baſe of the tongue, lies the beard *(byſſus)*, faſtened by two fleſhy roots, and conſiſting of almoſt one hundred fibres, each an inch long, of a dark green colour, with a metallic luſtre; they are undivided, parallel, and flattened. In general the *byſſus* is more than three quarters of an inch, without the cleft *(rima)*; but if the animal is diſturbed, it contracts it conſiderably. The top of each of theſe threads terminates in a circular gland or head, like the *ſtygma* of many plants. With this *byſſus* they faſten themſelves to rocks, corals, and other ſolid bodies; by it the young pearl fiſh cling to the old ones, and with it the animal procures its food, by extending and contracting it at pleaſure. Small ſhell fiſh, on which they partly live, are often found clinging to the former. The ſtomach lies cloſe to the root of the beard, and has, on its lower ſide, a protracted obtuſe point. Above the ſtomach are two ſmall red bodies, like lungs; and from the ſtomach goes a long channel or gut, which takes a circuit round the muſcular column above-mentioned, and ends in the anus, which lies oppoſite to the mouth, and is covered with a ſmall thin leaf, like a flap. Though the natives pretend to diſtinguiſh the ſexes, by the appearance of the ſhell, I could not find any genitalia. The large flat ones they call males, and thoſe that are thick, concave, and vaulted, they call females, or *pedoo-chippy*; but, on a cloſe inſpection, I could not obſerve any viſible ſexual difference.

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* Vide Houtt. Nat. Hiſt. Vol. I. p. xv. p. 381, ſeq.

† The depth at which the pearl fiſh generally is to be found, hindered me from paying any attention to the locomotive power, which I have not the leaſt doubt it poſſeſſes, uſing for this purpoſe its tongue. This conjecture is ſtrengthened by the accurate obſervations made on *muſcles* by the celebrated Reaumur, in which he found that this body ſerves them as a leg or arm, to move from one place to another. Though the divers are very ignorant with regard to the œconomy of the pearl fiſh, this changing of habitation has been long ſince obſerved by them. They alledge, that it alters its abode when diſturbed by an enemy or in ſearch of food. In the former caſe they ſay it commonly deſcends from the ſummit of the bank to its declivity.

It is remarkable that ſome of theſe animals are as red as blood, and that the inſide of the ſhell has the ſame colour, with the uſual pearly luſtre, though my ſervants found a reddiſh pearl in an oyſter of this colour; yet ſuch an event is very rare. The divers attribute this redneſs to the ſickneſs of the pearl fiſh; though it is moſt probable that they had it from their firſt exiſtence. In the ſhade they will live twenty-four hours after being taken out of the water. This animal is eaten by the lower claſs of *Indians*, either freſh in their curries, or cured by drying; in which ſtate they are exported to the coaſt; though I do not think them by any means palatable.

Within a mother of pearl ſhell I found thirteen *murices nudati* (vide Chemnitz's New Syſtem, Cabt. vol. XI. tab. 192, f. 1851 and 1852), the largeſt of which was three quarters of an inch long; but as many of them were putrid, and the pearl fiſh itſelf dead, I could not aſcertain whether they had crept in as enemies, or were drawn in by the animal itſelf. At any rate turtles and crabs are inimical to the animals, and a ſmall living crab was found in one of them.

The pearls are only in the ſofter part of the animal, and never in that firm muſcular column above-mentioned. We find them in general near the earth, and on both ſides of the mouth. The natives entertain the ſame fooliſh opinion concerning the formation of the pearl which the ancients did. They ſuppoſe them formed from dew-drops in connection with ſun-beams. A *Brâhmen* informed me that it was recorded in one of his *Sanſcrit* books, that the pearls are formed in the month of *May* at the appearance of the *Sooatee* ſtar (one of their twenty-ſeven conſtellations) when the oyſters come up to the ſurface of the water, to catch the drops of rain. One of the moſt celebrated conchologiſts *, ſuppoſes that the pearl is formed by the oyſter in order to defend itſelf from the attacks of the *pholades* and *boreworms*. But we may be aſſured that in this ſuppoſition he is miſtaken, for although theſe animals often penetrate the outer layers of the pearl ſhell, and there occaſion hollow nodes, yet, on examination, it will be found, that they are never able to pierce the firm layer, with which the inſide of the ſhell is lined. How can the pearls be formed as a defence againſt exterior worms, when, even on ſhells that contain them, no worm-holes are to be ſeen? It is, therefore, more probable theſe worms take up their habitations in the nodes, in order to protect themſelves from the attacks of an enemy, than that they are capable of preying on an animal, ſo well defended as the pearl-fiſh is. It is unneceſſary to repeat the various opinions and hypotheſes of other modern authors; it is much eaſier to criticiſe them, than to ſubſtitute in their place a more rational theory. That of Reaumur, mentioned in the memoirs of the *French* Academy for 1712, is the moſt probable, viz. that the pearls are formed like bezoars and other ſtones in different animals, and are apparently the effects of a diſeaſe. In ſhort it is very evident, that the pearl is formed by an extravaſation of a glutinous juice either within the body, or on the ſurface of the animal: the former caſe is the moſt common. Between one and two hundred pearls have been found within one oyſter. Such extravaſations may be cauſed by heterogeneous bodies, ſuch as ſand, coming in with the food, which the animal, to prevent diſagreeable friction, covers with its glutinous matter, and which, as it is ſucceſſively ſecreted, forms many regular lamellæ, in the manner of the coats of an onion, or like different ſtrata of bezoars, only much thinner; this is probable, for if we cut through the centre of a pearl, we often find a foreign particle, which ought to be conſidered as the nucleus, or primary cauſe of its formation. The looſe pearls, may originally have been produced within the body, and on their encreaſe may have ſeparated and fallen into the cavity of the ſhell. Thoſe compact ones, fixed to the ſhells, ſeem to be produced by ſimilar extravaſation, occaſioned by the friction of ſome roughneſs on the inſide of the ſhell. Theſe and the pearl-like nodes have a different aſpect from the pearls, and are of a darker and bluer colour. In one of the former I found a pretty large, true oval pearl, of a very clear water; while the node itſelf was of a dark blueiſh colour. The yellow or gold coloured pearl, is the moſt eſteemed by the natives; ſome have a bright, red, luſtre; others are grey or blackiſh, without any ſhining appearance, and of no value. Sometimes when the grey lamella of a pearl is taken off, under it is found a beautiful genuine one, but it oftener happens that after having ſeparated the firſt coat you find a worthleſs impure pearl. I tried ſeveral of them, taking one lamella off after another, and found clear and impure by turns, and in an impure pearl I met with one of a clear water, though in the centre of all I found a foreign particle. The largeſt and moſt perfect pearl which I ſaw during my ſtay at *Condatchey*, was about the ſize of a ſmall piſtol bullet, though I have been told ſince my departure, many others of the ſame ſize have been found. The ſpotted and irregular ones are ſold cheap, and are chiefly uſed by the native phyſicians as an ingredient in their medicines.

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* The Rev. Mr. Chemnitz at *Copenhagen*.

We may judge with greater or leſſer probability by the appearance of the pearl-ſhell, whether they contain pearls or not. Thoſe that have a thick calcareous cruſt upon them, to which *ſerpulæ* (ſea tubes) *Tubuli marini irregulariter intorti*, *Criſta-gali Chamar lazuras*, *Lepas tintinabulum*, *Madreporee*, *Millipore*, *Cellipore*, *Gorgontæ*, *Spongiæ*, and other zoophytes are faſtened, have arrived at their full growth, and commonly contain the beſt pearls; but thoſe that appear ſmooth, contain either none, or ſmall ones only.

Were a naturaliſt to make an excurſion for a few months to *Manâr*, the ſmall iſland near *Jafna* and the adjacent coaſt, he would diſcover many natural curioſities, ſtill buried in obſcurity, or that have never been accurately deſcribed.

Indeed no place in the *Eaſt Indies* abounds more with rare ſhells, than theſe: for there they remain undiſturbed, by being ſheltered from turbulent ſeas, and the fury of the ſurf. I will juſt name a few of them; viz. *Tellina foliaca* Lynn[*], *Tell, Spenglerii*, *Arca culculata*[†], *Arca Noæ*, *ſolen anatinus* Linn. *Oſtrea Iſognomum*, *Terebullum*, *albidum*, *ſtriatum*, *Turbo ſcalaris*[‡] *Bula volva* Linn[§], *Vexillum ingritarum*, *&c.* Amongſt the beautiful conc ſhells: *conus thalaſſiarchus Anglicanus cullatus*[||], *amadis thaſſiarchus*, *con. generalcis* Linn. *c. capitaneus*[**], *c. miles*[††], *c. ſtercus muſcarum*[‡‡]. *c. reteaureum*, *c. glaucus*[§§], *c. cereola*, *regia corona murus lapedius*, *canda erminea ſocietas cordium*. There are many other beſides thoſe already mentioned, equally valuable and curious.

The great ſucceſs of the Rev. Doctor John in conchology when at *Tutucorin*, and aſſiſted by G. Angelbeck, with a boat and divers: and the capital collections made by his agents, whom he afterwards ſent there with the neceſſary inſtructions and apparatus, may be ſeen in Chemnitz's elegant cabinet of ſhells in 4to. (with illuminated plates), and how many new ſpecies of Zoophytes he diſcovered, we learn from another German work by Eſper at *Erlangen*, the third volume of which is nearly finiſhed.

[*] The golden tong.

[†] Mounkſcape.

[‡] Royal ſtaircaſe.

[§] Weaver's ſhuttle.

[||] Red *Engliſh* admiral.

[**] Green ſtamper.

[††] Garter ſtamper.

[‡‡] Great ſand ſtamper.

[§§] Capſ. Gottw.

VI.

Proceſs for extracting Sugar from the Beet, communicated in a Letter from Mr. Achard *to Citizen* Van Mons[*].

IT is the beta vulgaris L. which is proper for making of ſugar, though all the varieties are not equally ſo. That of which the internal part is white: the ſkin of a pale red, and the root of a long ſpindle ſhape, is the beſt. All the varieties of the beet yield ſugar, but this muſt be choſen when it is to be manufactured to advantage; beſides which, it is on the culture principally the quantity of ſugar produced by the root depends.

It is aſcertained from the operations of extracting ſugar from the beet root, which I have performed under the inſpection of a committee, nominated by the King of Pruſſia, that the beſt method is as follows. The root not peeled, but in the ſtate in which it is dug out of the ground, with no other preparation than that of carefully ſeparating the leaves and the heart, is to be boiled in water till it is ſoft enough to be pierced with a ſtraw. A ſhort time of ebullition is ſufficient to give it this degree of ſoftneſs, which is well known to the confectioners, who give it to ſome fruits before they preſerve them. The beet when cold is divided and reduced into ſlices by means of the machine uſed by farmers to ſlice potatoes for cattle. A deſcription of this machine is to be found in a publication of Buſch, entitled: *Ueberſicht der fortſchritte in wiſſenſchaften, kunſten, manufacturen und handwerken von Oſtern* 1796, *bis Oſtern* 1797. *Erfurt* 1798: and the engraving which renders the ſubject clearer is copied in the firſt plate of my work. This method of dividing the root is the beſt I have yet found. Two men with this machine can cut nearly 100 pounds into very fine ſlices in three minutes. To extract the juice from the roots when ſliced, they are ſubjected to the action of a preſs which ſhould act with force ſufficient to extract at firſt as much juice as poſſible. The pulp which remains in the preſs ſtill contains a conſiderable portion of ſugar, which is worth extracting. To effect this it is diluted in a ſufficient quantity of water for twelve hours, after which the fluid part is drawn out by preſſure. The ſaccharine matter, after this ſecond extraction, is ſtill ſufficiently abundant to afford by fermentation, brandy or vinegar, with profit.

The liquids thus obtained are afterwards mixed and ſtrained through a flannel, and reduced by conſtant boiling to about two thirds. It is then paſſed a ſecond time through a woollen cloth, or a piece of cloth, ſuch as is uſed in ſugar refineries; after which the liquor is boiled in a ſmaller veſſel than the firſt till it is reduced to one half. The liquor is again boiled in a ſtill ſmaller veſſel which gives it the conſiſtency of a liquid ſyrup. It is neceſſary to remark, that by endeavouring to give to the ſyrup a too ſtrong conſiſtency there will be danger of ſpoiling the whole,

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[*] Decade. An. VIII. p. 15. No. 15. p. 324.

This ſyrup poured into ſhallow earthen veſſels, which preſent a large ſurface to the air, is to be placed in a ſtove at the heat of 20 or 30 degrees, or if agreeable at 30 or 40 of Reaumur, in order to cryſtalize it. During this inſenſible condenſation of the ſyrup the cryſtalline incruſtation, which is formed at the ſurface, ſhould be occaſionally broken, in order that by favouring the evaporation the product of cryſtals may be haſtened. As ſoon as it is obſerved that, inſtead of the cryſtalline incruſtation, a thick gummy pellicle, not granulated, is formed on the ſurface of the ſyrup, it is a ſign that the matter does no longer cryſtalize, but begins to dry, and the evaporation ſhould then be ſtopped. This reſidue forms a mixture more or leſs thick, of a cryſtalline ſubſtance and a fluid viſcid matter. In order to ſeparate the cryſtalized ſugar from the gummy extract, they are put together into a ſack of wet cloth tied tight, and then by means of a preſs, gradually applied, the fluid part is to be paſſed through the cloth, and the ſugar remains in the bag. This ſugar after drying is a yellow muſcovado, compoſed of regular cryſtals, which when pulverized form a white powder of which the taſte is very good, being ſweet and clear, and may be applied to a number of uſes for which refined ſugar is employed. By the operation of refining, ſugar may be made from this muſcovado of whatever quality is agreeable, and by repeating it the fineſt ſugar may be had. The waſte in this manufacture, that is to ſay the reſidual pulp, the ſyrup or mucilage which paſſes through the cloth when ſubjected to the preſs, the ſyrup in which the ſugar has cryſtalized, the waſhings, &c. &c. all theſe are ſtill very uſeful, and a conſiderable quantity of rum or brandy may be obtained from them, which may be uſed in making up the fineſt compounds. The muſcovado, ſuch as is obtained by the firſt opperation, coſts about a groſs and a half of Pruſſia, without reckoning the watter which may be had by turning the reſidues to uſe. When we add this product, and when the manipulations ſhall be more perfect, to effect which I ſhall employ myſelf this winter, I am perſuaded that our European muſcovado will only coſt half the price, or 9 fennins; and in the countries, where fuel is dearer, at 1 groſs [*]. The manufacture of ſpirits from the waſte of the ſugar is of great importance, as by this means a great ſaving of corn will be made, and the manufacturing of beet ſugar, which delivers Europe from a deſtructive monopoly becomes ſtill more intereſting. I am at preſent employed in the attempt to diſcover a method of pouring the juice of the roots when ſufficiently condenſed into moulds or forms, in order that it may acquire the figure of a ſugar loaf, and afterwards by claying become very white at a ſingle operation. I have already found ſeveral methods of obtaining this object very ſpeedily. This new manipulation will facilitate the art of ſugar-making, and diminiſh the price ſtill further.

[*] The pound in uſe at Berlin, being 476, 28 grammes, and the value of the groſs, or of 12 fennins, being 17 centimes, French money, it follows that the hectogramme does not amount to more than about 5 or 6 centimes, (or the pound, poids de marc, from 28 to 50 centimes). Note of the French editor.---(about 2½ d. per lb. avoirdup.)

VII.

Letter to the EDITOR *from Mr.* WILLIAM HENRY, *on his Diſcovery of a new and improved Method of preparing the Pruſſiate of Pot-aſh; and on the diſputed Queſtion, Whether Pruſſiate of Pot-aſh be decompoſed by Muriate of Barytes?*

Mancheſter, March 23, 1800.

SIR,

IN conſequence of a paper, inſerted in your Philoſophical Journal, I have been favoured with the following letter from Mr. Kirwan. As the references, which it contains, to authorities on a diſputed point, may be intereſting to others as well as to myſelf, I take the liberty of ſending you a copy of it for publication; premiſing, that I have ſtated to Mr. K. my intention of making you this communication, and that he has not expreſſed any objection.

“ *To Mr.* WILLIAM HENRY.

“ DEAR SIR,

“ On looking over Mr. Nicholſon's valuable Philoſophical Journal, which, though of
“ ancient date, I had not until lately leiſure to inſpect, I find, in the 29th No. Vol. ii.
“ p. 170, that you ſeem to regret I had not mentioned on what grounds I denied the pre-
“ cipitation of barytic earth from muriatic acid by the Pruſſian alcali, and then ſuggeſt that the
“ relation of the Pruſſian alcali to this earth had not been an object of attention to Klaproth
“ or Pelletier, &c. I hope, therefore, it will be agreeable to you that I ſhould ſtate thoſe
“ grounds. That Pruſſian alcali, properly prepared, does not precipitate barytic ſolutions,
“ was firſt proved by that accurate chemiſt, Mr. Meyer, of Stetin, as you may ſee in 2
“ Crell's Chemical Annals for 1786, p. 142, 143. As I ſuppoſe you have that work, I
“ ſhall not trouble you with the detail of his experiments. You may, alſo, ſee that both
“ Pelletier and Klaproth did treat of this fact, Pelletier affirming the precipitation of this
“ earth by the Pruſſian alcali (21 Journal des Mines, p. 45); and Klaproth expreſsly deny-
“ ing it, and aſſerting that Pelletier was miſtaken. (See 2 Klaproth, p. 91.)

“ I am, dear Sir,

“ With high regard and reſpect, &c.

“ R. KIRWAN.”

“ *Dublin, Jan.* 20.

Unfortunately it is not in my power to conſult any one of the works, to which Mr. K. has kindly referred me; and highly as I reſpect the authorities, to which he has appealed, yet I did not feel diſpoſed to acknowledge myſelf in an error, till convinced by a circumſtantial detail of the experiment of Meſſrs. Meyer and Klaproth. I reſolved, therefore, to appeal to the deciſion of experiments conducted with all poſſible accuracy and attention.

The queſtion in diſpute is, whether or not a double elective affinity be exerted between pruſſiate of pot-aſh and muriate of barytes? If ſuch an affinity really ſubſiſts, and produces, on the admixture of the two ſolutions, two new compounds, one of theſe (the pruſſiate of barytes) muſt neceſſarily, from its known inſolubility, appear under the form of a precipitate. Now, on mixing the two ſolutions, a precipitation generally takes place; but this, it is affirmed by certain chemiſts, is owing to the impurity of the pruſſiate of pot-aſh, and is occaſioned by the decompoſition of ſulphate of pot-aſh by muriate of barytes. The truth of this explanation may be aſcertained, either by an attentive examination of the precipitate, or by employing a pruſſiate of pot-aſh perfectly free from ſuch impurity.

The former of theſe teſts, as I have ſtated in your Journal, II. 171, I employed ſeveral months ago. Having found that pruſſiated barytes is ſoluble in diluted muriatic acid, which exerts no action on the ſulphate of that earth, I digeſted a precipitate, thrown down from muriated barytes by pruſſiated pot-aſh, with diluted muriatic acid. Part of the precipitate was taken up; and the ſolution betrayed, to the proper teſts, deciſive characters of its containing pruſſiated barytes. Since the receipt of Mr. Kirwan's letter, I have repeated this experiment ſeveral times; occaſionally with the reſult, which has juſt now been ſtated; while, at other times, none of the precipitate was diſſolved by the acid. Theſe contradictory reſults will be explained by what follows; but not being able to deviſe the cauſe of them, at the time they occurred, I had recourſe to the remaining expedient, viz. the employment of a pruſſiate of pot-aſh, perfectly free from ſulphuric ſalts.

The difficulty of obtaining a pruſſiate of the required purity in the common mode, is hardly conceivable, except by thoſe who have made the attempt. In your Journal, II. 171. I have aſſerted that both carbonate and ſulphate of pot-aſh are decompoſed by pruſſiated barytes; and it has ſince occurred to me, that an excellent mode of preparing pruſſiate of pot-aſh might be founded on theſe facts. For the ſmall portion of ſulphate of pot-aſh, contained in all carbonates, may be expected to be decompoſed by the pruſſiated barytes, provided this laſt be employed in ſufficient quantity. On making the experiment, the reſult anſwered my expectation; and I ſhall, therefore, give the proceſs, which I have ſince frequently repeated with the ſame ſucceſs, under the form of a practical rule.

1. Calcine any quantity of carbonate of barytes in a ſtrong fire, a ſufficient length of time to expel its carbonic acid. Diſſolve the pure barytes in boiling water, and add by degrees pure Pruſſian blue in powder, till it ceaſes to be diſcoloured. Filtre the ſolution through paper, and if it ſhould become muddy during cooling, owing to the depoſit of oxyde of iron, filtre it again. After having ſtood a few hours, ſmall yellowiſh cryſtals will appear, which are the pruſſiated barytes. From the remaining ſolution a further quantity of cryſtals may be obtained by evaporation.

2. To a ſolution of carbonate of pot-aſh, gently heated, add by degrees the pruſſiate of barytes in powder, till the ſolution no longer reſtores the colour of reddened litmus paper.

Rather more of the pruſſiate of barytes ſhould be employed than is required to decompoſe the carbonate. After digeſting the mixture about half an hour, filtre the liquor, which, when gently evaporated, will ſhoot into beautiful cryſtals of pruſſiate of pot-aſh. Theſe cryſtals contained, in one inſtance, 24 per cent. of oxyde of iron; but of this oxyde a conſiderable part may be ſeparated, by digeſting the ſolution, before evaporating it, with a little acetic acid (radical vinegar.) The acetic has this advantage over all other acids, that its combination with pot-aſh affords a ſalt incapable of cryſtallizing; and which cannot, therefore, mix with the cryſtals of pruſſiated pot-aſh.

The carbonate of barytes, which is thus re-generated, may be reſerved; and may be again fitted by calcination for the ſame proceſs.

With the view of deciding the queſtion in diſpute, I added to a ſolution of pruſſiate of pot-aſh prepared, in the above manner, a ſolution of muriated barytes, No precipitation enſued; nor was the tranſparency of the mixture in the leaſt degree diſturbed. This experiment, therefore, in addition to thoſe before related, convinced me that I had been deceived; and that Meſſrs. Meyer and Klaproth were correct in denying the precipitation. Happening, however, to examine the jar, about half an hour afterwards, I obſerved that a number of ſmall cryſtals had formed on its inner ſurface; and theſe, on allowing the mixture to ſtand a few hours, increaſed conſiderably in number. I next mixed the two ſolutions in larger quantity, with the ſame reſult. No immediate effect was apparent; but after a few hours, an abundant crop of ſmall cryſtals had formed at the bottom and on the ſides of the jar. Theſe cryſtals had the following character:

1. They diſſolved very ſparingly in cold water, viz. in about the proportion of a quarter of a grain to each ounce.

2. Hot water diſſolved them more readily, but ſtill only in very ſmall proportions.

3. From theſe watery ſolutions ſulphate of pot-aſh, or ſulphuric acid, precipitated ſulphate of barytes; and on adding the ſulphate of iron to another portion of the ſolution, an abundant precipitation of Pruſſian blue occurred.

4. The cryſtals were totally diſſolved by dilute muriatic acid; and the ſolution gave decided marks, to the proper teſts, of containing pruſſiated barytes.

5. When heated ro redneſs in a ſilver veſſel, they grew black and loſt their form. On adding dilute muriatic acid to this coal, an effervefcence enſued, and the ſolution was muriated barytes.

The foregoing characters are peculiar to the ſalt termed pruſſiate of barytes, the formation of which, under the above circumſtances, proves, beyond all doubt, that a double elective affinity is exerted between muriate of barytes and pruſſiate of pot-aſh. In this reſpect, barytes differs from other earths, and approaches the metals.

The above experiments furniſhed a clue, which led to the explanation of the contradictory properties, obſerved, at different times, in the precipitate from muriated barytes by pruſſiate of pot-aſh. In trials, which I have ſince made, it has uniformly happened, that common pruſſiate of pot-aſh has precipitated muriate of barytes. When the ſupernatant liquor was decanted immediately, the precipitate, after having been waſhed, proved to be ſimply ſulphate of barytes; but when the two ſolutions were allowed to ſtand together ſome time, before ſeparating them from the precipitate, this laſt had conſtantly the properties of a mixture of ſulphate of barytes, with pruſſiate of barytes; and ſometimes cryſtals of the latter ſalt were perceivable in the mixture.

Before taking my leave of the ſubject, allow me to correct an erroneous ſtatement, which I have made in your Journal, II. 171. I have there ſtated that cauſtic barytes does not precipitate pruſſiate of pot-aſh. This experiment I have ſince repeated with pure pruſſiate deſcribed above. No immediate precipitation enſued (the circumſtance that before miſled me); but after a few hours ſmall cryſtals were depoſited, which had every character of pruſſiate of barytes. It follows, therefore, that in a table of the elective affinities of Pruſſian acid, barytes ſhould be placed above pot-aſh.

I am, Sir, very reſpectfully,
your obedient ſervant,
WILLIAM HENRY.

VIII.

On the Genuineneſs and Purity of Drugs and Medicines. By Mr. FRED. ACCUM.

(Continued from page 122. Vol. II.)

EXAMINATION of ALKALINE SUBSTANCES.

Prepared Kali. Pharm. Lond. *Salt of Tartar.*

THE ſalt of tartar of the ſhops, if prepared according to the London Pharmacopeia, cannot be obtained pure from pearl-aſhes by mere ſolutions, colature, evaporation, and cryſtallization; hence this article generally contains a conſiderable portion of *ſulphate* and *muriate of potaſh*, and is never perfectly free from *ſiliceous* and *calcareous earth.* In order to diſcover its purity, one part ought to be diſſolved in eight parts of diſtilled water, and then neutralized with pure nitric acid: if the mixture remain perfectly tranſparent when cold, the alkali is free from ſiliceous earth; whereas on the contrary, if ſiliceous earth be preſent the mixture will become turbid, the ſiliceous earth will be ſeparated, and its quantity may thus be aſcertained.

Sulphate of pot-aſh, the next article generally found in this ſalt, is beſt diſcovered by adding gradually to a ſimilar neutraliſed ſolution, a ſolution of muriate of barytes, or acetite of lead. If ſulphuric acid be preſent, a white precipitate will be produced, but no ſuch effect will take place if no ſulphuric combination be preſent. One hundred parts of the precipitate, occaſioned by the admixture of the muriate of barytes, indicate 26 per cent. of ſulphuric acid, and 100 parts of the acetite of lead indicate 30 per cent. of acid. The preſence of muriatic acid may be aſcertained by adding to a like ſaturated ſolution of this ſalt, a few grains of nitrate of ſilver. If the alkali did not contain any combination of muriatic acid, it will have no effect upon it; but if the minuteſt quantity of muriatic acid is preſent, a white precipitate will be produced, which after ſome time acquires a bluiſh hue; neither nitric nor acetic acid will rediſſolve it. Calcareous earth is manifeſted in a ſimilar manner, by adding drop by drop, a ſolution of oxalic acid. The quantity of the precipitate thus obtained, if calcareous earth be preſent, gives the proportion of this earth in a given quantity of the ſalt ſubmitted to the experiment.

Pure ſalt of tartar, which ſtands all theſe teſts, is of a ſnow whiteneſs, inodorous, perfectly dry, and ſoluble without *any* ſediment in an equal quantity of pure cold water, it then contains generally 70 parts of alkali, 23 carbonic acid, and 7 of water.

Water of Kali. Pham. Lond. *Solution of Salt of Tartar.*

When ſalt of tartar is expoſed in a ſhallow veſſel to the action of a moiſt atmoſphere, it ſoon deliqueſces, or attracts ſo much water, as is ſufficient to render it liquid. This fluid ought to contain at leaſt one part of alkali in four. If ſo, it feels extremely greaſy or ſlippery between the fingers, on account of it diſſolving the epidermis, and converting it into a kind of ſoap. If the alkali made uſe of has been pure, this fluid is *perfectly colourleſs*, clear, void of ſmell, and poſſeſſes all the properties of a pure ſalt of tartar. But as the alkali made uſe of in general, by our chemiſts, is far from being ſo, the article we meet with has generally a yellowiſh tinge, a diſagreeable, ſomewhat urinous, ſmell, and ſoon depoſits a conſiderble quantity of earthy inſoluble matter.

Water of Pure Kali. Ph. L. *Solution of Cauſtic Pot-aſh.*

As it is called in our Pharmacopeia, ought to be a ſolution of cauſtic alkali in water, of ſuch a ſtrength that an exact pint ſhall weigh juſt 16 oz. troy. When well prepared, it is as limpid as water, and inodorous. It ought not to occaſion any precipitate with lime water, neither ought it produce the leaſt efferveſcence with acids. A redundancy of lime is beſt known by blowing into this fluid, through a tobacco pipe, or other tube. If too much lime be preſent, the fluid will then turn turbid. Inſtead of obtaining this article in the ſhops, we take common ſoap-leys of the ſoap manufacturers, diluted with water, hence this article is always yellow, and hence the redundancy of lime, and other heterogeneous ſalts found in it.

Prepared Natron. Ph. L. *Mineral Alkali.*

As it is no eaſy taſk to obtain mineral alkali *perfectly pure*, we need not expect to find it ſo in commerce. This article, if prepared agreeably to the directions of the Royal College of Phyſicians, cannot but be contaminated with *ſulphate* and *muriate of ſoda*. But what is ſtill worſe, the fraudulent chemiſts mix ſulphate of ſoda with it intentionally. In order to diſcover the ſulphuric and muriate ſalt, a determinate quantity of the alkali is to be ſaturated with acetous acid, and then divided into three different glaſſes. By adding to the firſt gradually a ſolution of muriate of barytes, the quantity of ſulphuric acid may be diſcovered, in the ſame manner as mentioned before. In the ſecond glaſs, having let fall a few drops of nitrate of ſilver, the quantity of muriatic acid may be inveſtigated in a ſimilar manner; and by adding gradually a little tartareous acid to the third glaſs, an acidulous tartraite of pot-aſh will be ſeparated in the form of a white powder, 300 parts of which contain 107 per cent. of vegetable alkali, as 75$\frac{1}{3}$ per cent. pure alkali, free from carbonic acid.

The beſt mineral alkali, free from any heterogeneous admixture, cryſtallizes in oblong druneated rhomboidal cryſtals, as clear as glaſs. It is ſoluble in an equal quantity of cold water, and generally contains 80 parts of alkali, 64 carbonic acid, and 256 of water.

Water of Pure Ammonia. Ph. L. *Water impregnated with Ammoniac.*

The *perfectly cauſtic* ſpirit of ſal ammoniac, as it is uſually called, is one of thoſe articles we ſeldom meet with. It generally contains a conſiderable portion of carbonic acid, and is alſo not ſufficiently ſtrong (particularly for philoſophical purpoſes). If this liquid is conſiderably ſtrong, a phial containing exactly 224 grains of diſtilled water muſt compriſe no more than about 216 grains of aqua ammonia.

Water of Ammonia. Ph. L. *Solution of Carbonate of Pot-aſh.*

In order to judge of the relative ſtrength of this article, which is nothing elſe but a mere ſolution of carbonate of ammoniac in water; ſtrong ardent ſpirit may afford no mean criterion of the ſtrength or proper ſaturation, as well as of the purity of this fluid. If this fluid be fully ſaturated with carbonate of ammoniac, a quantity of ſtrong ardent ſpirit, poured ſlowly down the ſides of the glaſs, will ſeparate the alkaline ſalt, by uniting with the water, which kept it in ſolution, in conſequence of which an opake denſe coagulum will be formed on the ſurface where the fluids touch each other; and if on ſhaking them briſkly together, the whole mixture ſhould become converted into a nearly conſiſtent maſs, this article is very good. Whereas, on the contrary, if weak, the effect will be far leſs conſiderable, and the coagulum will conſtantly remain ſeparated into two diſtinct parts, viz. a ſolid and a fluid one.

Water of ammonia fully ſaturated with carbonate of ammoniac has the ſpecific gravity 1,150.

Prepared Ammonia. Ph. L. *Carbonate of Ammoniac.*

This ſalt ſhould be totally volatile if laid on an ignited ſubſtance. It is frequently offered for ſale by chemiſts in the form of a coarſe powder, which is nothing elſe than a mixture of muriate of ammoniac and pot-aſh; this compoſition of courſe is not entirely volatiliſed by heat The carbonate of ammoniac imported from abroad always contain veſtiges of iron, and ſometimes a conſiderable quantity of lime. In order to diſcover the preſence of iron, a little of the ſalt is to be ſaturated with acetic acid, a few drops of pruſſiate of pot-aſh is then to be added, which will impart to the ſolution a blue tinge if the minuteſt quantity of iron be preſent. The preſence of calcareous earth is manifeſted by means of oxalic acid, occaſioning a white precipitate of carbonate of ammoniac; that containing the leaſt quantity of iron has a yellowiſh tinge, whereas pure carbonate of ammoniac is of a ſnow whiteneſs, perfectly dry, ſomewhat ſonorous, volatile before 212° Fahr. and ſoluble in 2 parts of diſtilled water at 55° Fahr. Its ſpecific gravity is generally 1,4076, and its component parts are 40 ammoniac, 53 carbonic acid, and 7 water.

Salt of Hartſhorn. Ph. L. *Carbonate of Ammoniac ſoiled with Animal Oil.*

The genuineneſs of this article (which differs from the former, merely, in being impregnated with a ſmall quantity of animal oil) may be diſcovered in a ſimilar manner.

Solution of the above in an Aqueous Fluid. Volatile Liquor of Hartſhorn. Ph. L.

Is in general imitated by mixing a quantity of aqua ammoniae purae with the officinal ſpirit of hartſhorn, in order to make it ſtronger, and to give it the appearance of a ſpirit well ſaturated with volatile ſalt. The ſpirit thus becomes more pungent, ſo as to bear an addition of a conſiderable portion of water, without betraying the impoſition either by the taſte or ſmell. This fraud may be diſcovered by adding ſtrong ardent ſpirit to the ſophiſticated ſpirit; for if no conſiderable quantity of cryſtallized volatile ſalt becomes precipitated, the adulteration may be ſuſpected. It may even be diſcovered by the circumſtance of the ſpirit not efferveſcing briſkly with acids.

Spirit of hartſhorn carefully diſtilled is colourleſs. Its taſte is not very unpleaſant (ſaline not pungent), and its ſmell ſomewhat empyreumatic. By long keeping it contracts a yellowiſh tinge, and acquires a bitter taſte and nauſeous fœtid ſmell. If fully impregnated with volatile alkali, its ſpecific gravity is about 1,500.

IX.

A Memoir on the Ammoniuret of Cobalt, and upon a new Acid contained in the Grey Oxide of that Metal, known by the Name of Zaffre. By LOUIS BRUGNATELLI[*].

MY attention has been for ſome time directed to the ammoniacal combinations of the metals, or the ammoniurets. They have hitherto been little examined, excepting that of copper, though their characteriſtic properties diſtinguiſh them very prominently from other bodies. The ammoniuret of cobalt is that to which I directed my firſt reſearches [*], and I now ſend you an account of the peculiarities they have offered.

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[*] Tranſlated from the Italian manuſcript of the author, by Van Mons, into French in the annals de Chemie XXXIII, 114, whence the preſent tranſlation is made.

1. I had ſeveral times obſerved that the precipitate, formed by ammonia in the ſolution of the nitrate of cobalt, was rediſſolved in that alkali. I collected this precipitate on a filtre, and waſhed and dried it. Its colour was dark. Upon half an ounce of this precipitate I poured two ounces of liquid ammonia, after which I cloſed the bottle, and left the mixture at reſt; the temperature of the atmoſphere being 20° above the zero of Reaumur. At the expiration of 24 hours the alkali had acquired a deep red colour [†], and the precipitate was entirely diſſolved. I ſuppoſed I had formed a perfect ammoniuret; but nevertheleſs I endeavoured to procure a larger quantity by other means.

I attempted without effect to diſſolve ſmalt, or the blue (glaſs) of cobalt, in cauſtic ammonia, even with the aſſiſtance of long continued digeſtion; but ſolution took place without difficulty with the grey oxide of the ſame metal, commonly called Zaffre. This ſubſtance afforded me an eaſy means of procuring the ammoniuret in abundance, and to repeat and vary my experiments in ſeveral different manners.

3. I evaporated the ammoniuret obtained from zaffre to dryneſs. The concrete reſidue which I obtained was compoſed of two very diſtinct ſubſtances, one of which was of a deep red colour, and the other of a pale yellowiſh tinge.

4. I poured diſtilled water upon this reſidue, agitating the mixture with a ſpatula of glaſs. The red portion was totally diſſolved, and communicated a beautiful roſe-colour to the water. The yellowiſh matter remained undiſſolved.

5. The yellowiſh ſubſtance may be obtained by evaporating the liquid ammoniuret, from which it is ſeparated, the moment the fluid is reduced to half its original bulk. The red ſubſtance remained diſſolved in the laſt fourth part of the liquid [‡].

6. Ammonia therefore takes from zaffre in its ſolution two very diſtinct ſubſtances, of which the one ſoluble in water has a red colour, and the other inſoluble in the ſame liquid is yellowiſh. This laſt ſubſtance is the pure oxide of cobalt. We think we have diſcovered a peculiar acid in the former, diſtinct in its properties from every other known acid.

Concerning the pure Oxide of Cobalt.

7. The yellowiſh ſubſtance which is ſeparated by ſlow evaporation in the air, or in the ſun's light, may be conſidered as the pure oxide of cobalt. In fact it is inſipid, inodorous, inſoluble in water, and ſoluble in the mineral acids. The nitro-muriatic acid forms with it

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[*] I have ſince examined the ammoniurets of mercury, zink, copper, and arſenic. B.

[†] Bergman obſerved only that ammoniae aſſumed a red colour with cobalt. "Cobaltum," ſays he, "a niccolo differt quod omnibus acidis et alcali volatile ſolvatur colore rubro." This circumſtance has been repeated by chemiſts without addition. B.

[‡] A yellow matter is ſeparated from the ammoniuret of cobalt, if kept for a time, even in well cloſed bottles. B.

a yellowiſh ſolution, which loſes moſt of its colour by the addition of a ſmall portion of diſtilled water.

8. This ſolution may be uſed as a ſympathetic ink, like the common muriate of cobalt. Sometimes the acid refuſes to diſſolve the whole of the oxide; but the ſolution is completed by the addition of a ſmall quantity of water.

9. This ſolution is precipitated by the pruſſiate of pot-aſh, of a blueiſh green, which does not change. It is not precipitated by the gallic acid, but the ſolution becomes of a deeper colour. In other reſpects this ſolution has the ſame habitudes as the common ſolution of cobalt in the ſame acid.

10. The muriatic acid diſſolves the yellow oxide very well, and aſſumes a very deep green colour, which immediately diſappears with the nitrous acid, and alſo by a ſmall portion of water. But it is revived again by the addition of a ſmall quantity of well concentrated muriatic acid.

Of the pure Ammoniuret of Cobalt.

11. The yellow oxide is totally ſoluble in ammonia, and forms the pure ammoniuret. Its colour is yellow, and ſometimes roſe coloured; the acids do not decompoſe it; the muriatic acid diſcolours it: pruſſiate of pot-aſh gives it a grey colour, and afterwards throws down a precipitate of the ſame colour. The ſulphuret of pot-aſh cauſes it to aſſume a deep colour, inclining to black, and throws down ſulphuret of cobalt *.

Concerning the Acid obtained from the Ammoniuret of Cobalt, and its Properties.

12. The red ſubſtance of the preceding experiments was ſeparated from the yellow oxide. For this purpoſe I evaporated the liquid ammoniuret in the ſun, and when it was reduced to about one-fourth of its volume, and no longer afforded any yellow precipitate, I filtered it through paper. The fluid was of a deep red colour, like that of cochineal. This liquid emitted no ſmell, but its taſte was very ſharp. It was expoſed to the ſun's light till perfectly dry.

13. The remaining maſs was diſſolved in diſtilled water, which acquired a fine ruby colour. This ſolution exhibited unequivocal ſigns of acidity. It depoſited on cooling ſome ſmall brilliant cryſtals, which I found to be a combination of the acid with ammonia. This acid appearing to be diſtinct from every other, I have thought fit to call it the *cobaltic acid.*

In order to aſcertain whether heat was capable of raiſing the cobaltic acid, I ſubmitted one pound of the ammoniuret of cobalt to diſtillation in a retort. When three-fourths of the liquid had paſſed into the receiver, I ſtopped the diſtillation. The retort contained a liquid rendered turbid by the precipitated yellow oxide. I decanted the liquid after the precipitate had ſettled, and evaporated it to dryneſs. The reſidue was yellowiſh, and partly ſoluble in water, to which it communicated a yellow tinge. This ſolution exhibited all the acid characters of the red liquid, No. 15.

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* The ſulphuret of cobalt, dried in the air, reſembles zaffre in its colour. When rubbed on paper, it aſſumes the metallic brilliancy, as do moſt of the other metallic ſulphurets. It emits a ſulphureous ſmell when heated, and takes fire when thrown on ignited coals. B.

15. In another experiment, I put one pound of the ammoniuret of cobalt into a retort, and urged the diſtillation to dryneſs. The fixed matter which remained on the bottom of the retort was blue at the ſurface, but internally yellow. After ſome hours the blue colour totally diſappeared, and was changed into red.

16. A remarkable difference between the reſidue of the evaporation by the ſun's light, and that by the fire, was that the latter tranſmits the cobaltic acid nearly colourleſs ; ſo that the cold ſolution is nearly as limpid as water. I have beſides remarked, that by this laſt proceſs the acid contains little or no cobalt of ammonia.

17. I ſhall here preſent the principal characters which diſtinguiſh this new acid. They are

1. Its form is concrete, and it is not volatilized by fire.

2. In ſome inſtances it is red (13); in ſome pale yellow (14); and in ſome colourleſs (16).

3. It is without ſmell.

4. It has a ſharp, and not unpleaſant taſte.

5. It ſtrongly reddens the tincture of turnſole.

6. It is perfectly ſoluble in water.

7. It decompoſes all the ſulphurets of alcali, and precipitates the ſulphur.

8. It precipitates the ammoniuret of copper of a light green, and that of zink of a clear white.

9. It precipitates the ſulphuret of copper of the ſame colour as the ammoniuret of that metal.

10. It precipitates the nitrate of ſilver white.

11. The nitro muriate of tin the ſame.

12. The nitrate of mercury of a light ſtraw colour.

13. The acetite of lead white.

14. It does not ſenſibly affect the ſolutions of gold and platina.

15. It precipitates lime water in a white coagulum, inſoluble in water, and in exceſs of acid.

16. It precipitates the acetites and muriates of barytes.

17. It is ſeparable by alcohol from its ſolution in water.

18. When employed as a ſympathetic ink, it does not give a green or blue colour, like the ſolutions of cobalt ; but gives a brown and afterwards a black colour to paper when rather ſtrongly heated, as happens with other acids.

19. It affords, with new made tincture of galls, a yellow abundant precipitate.

20. With the ſaturated ſolution of ſoda, it affords an irregular tranſparent ſalt ſoluble in water, and not deliquieſcent.

21. With pot-aſh it affords a cryſtallizable ſalt in ſquare cryſtals, which are tranſparent and permanent in the air.

22. With ammoniac it forms a ſalt ſoluble in its acid.

23. With barytes it forms an opake ſalt, cryſtallizable with difficulty.

Supplement.

The preſence of an acid in zaffre, induced me to ſuſpect that it might probably be arſenical: but this doubt ſoon vaniſhed, by comparing the characters of each.

1. The arſenical acid does not precipitate the ſolutions of ſilver, as does the cobaltic acid. 2. The arſenical acid precipitates lime water; this arſeniate is re-diſſolved by the acid, as well as by a new quantity of lime water. The contrary happens with the acid of cobalt. 3. The arſenical acid does not decompoſe the muriate and acetite of barytes, as does the cobaltic acid. 4. The arſenical acid is ſoluble in alcohol, which precipitates the cobaltic in a concrete form.

It remained to be proved, whether the acid obtained from zaffre exiſted ready formed in that oxide, or whether it was produced by the action of the ammonia.

As the acid of cobalt is very ſoluble in water, I boiled ſix pounds of zaffre in eight pounds of water for a quarter of an hour, and filtered the liquid while yet warm. That which paſſed was tranſparent and colourleſs; but had a perceptible taſte. I evaporated the liquid, taking care to cover the veſſel with a piece of ſilk. When it was reduced to one half, it became turbid, but the matter which was ſeparated was not perceptibly coloured. I continued the evaporation till no more than one-third of the liquid remained, and then removed it from the fire. It depoſited a very white matter, which the contact of the air changed to a beautiful roſe colour. I ſeparated this matter, and collected it on the filtre.

The filtered fluid was of a bright yellow colour, and perfectly tranſparent. It had a very decided acid taſte; reddened the tincture of turnſole, ſpeedily decompoſed lime water, the ſalts of barytes and of ſilver, was precipitated by alcohol, &c. In a word, its habitudes were in every reſpect the ſame as thoſe of the cobaltic acid obtained by the foregoing proceſſes.

The red depoſition which remained on the filtre had no taſte, and coloured the muriatic acid of a very fine green. It was the pure oxide of cobalt. This oxide is largely ſoluble in its own acid, and is precipitated in proportion as the acid is concentrated.

The acid which the ammonia ſeparated from the zaffre, exiſts therefore ready formed in that ſubſtance. Its radical ſtill remains to be poſitively aſcertained. In the mean time I have thought it proper to deſignate it by the name of the *Cobaltic Acid.*

X. *The*

X.

The Apparatus of Woulfe improved, ſo as not to require luting. By Cit. GIRARD.*

AS the ingenious apparatus of Woulfe, which has been ſo highly uſeful in modern chemiſtry, always requires a conſiderable time for its arrangement in thoſe operations in which it is uſed, the attention of philoſophers has been long directed to the means of ſimplifying it. Various forms have been contrived to conſtruct this apparatus, hermetically cloſed, without the aſſiſtance of luting; but they are in general too difficult in the execution. That which I now offer to the Inſtitute, may be put together or diſmounted in a few ſeconds, and appears applicable in all the circumſtances of experiment.

It conſiſts ſimply in cauſing the veſſels to be made at the glaſs houſe, with the neck A, to receive the interior tube A B (Pl. II. Fig. 2.) which is welded in the neck itſelf by means of a projection in the ſide of the glaſs. The tube on the oppoſite ſide is firſt drawn to a point, or thin tube as uſual; but then inſtead of breaking this tube near the bottle, to make it agree with the ſubſequent fittings, it is left entire and bended, ſo that it terminates in a curve, which may be introduced into the tube A B. It is unneceſſary to remark, that the tube A B, and the neck into which it enters, ſhould have the ſame curvature, which is very eaſily effected by bending both on the ſame cylinder of earth or wood.

It may be eaſily underſtood, that the neck of the firſt bottle being introduced into the tube of the ſecond, beyond which it projects, and both tubes entirely immerſed in the water, no gas can eſcape from the firſt veſſel, but by paſſing through its neck, and through the liquid it contains.

Fig. 3. repreſents another apparatus of Woulfe's, which will ſupply the place of the other in caſe of diſtance from a glaſs houſe. It is compoſed of large necked bottles and tubes. Take a ſomewhat thick tube, of ſufficient bore to receive another. The upper part of this muſt be enlarged by heating and blowing, and it may afterwards be ground and made to fit in the form of a ſtopper.

Another tube ſhould be then taken, which can enter the firſt ſo looſely that it can be moved with eaſe. The extremity A of this tube is then ground in the ſame manner, in order that it may fit another bottle, after which the uſual curve A B C is given to it.

Laſtly, it is introduced into the tube E F, fig. 4. and the prominent part D is bended upwards. The veſſel may then be conſidered as hermetically cloſed, as ſoon as the lower aperture of the large tube is immerſed in the included water.

The curve of the tubes and necks of the bottles is only intended to direct the bubbles of gas which eſcape through the neck, that they may not again enter the tube out of which they are required to paſs. This curvature is not required, except in ſmall apparatus.

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* Annales de Chimie, xxxii. 283.

When a projection of an inch, or more, can be given to the neck beyond the lower opening of the tube, the bubbles muſt neceſſarily eſcape without that precaution.

Fig 5. exhibits another conſtruction of the ſame apparatus.

If an apparatus, compoſed of a very long ſeries of bottles were employed, it might be feared, that the gas contained in the firſt veſſel having a very ſtrong reſiſtance to conquer, would cauſe the fluid of the firſt bottle to expand and riſe in the tube; but it is very eaſy to remedy this inconvenience, either by uſing bottles of different ſizes, or by putting a ſmaller quantity of fluid in the firſt bottles, than in the following ones.

XI.

A Memoir on the Diſcovery of certain Bones of a Quadruped of the Clawed Kind in the Weſtern Parts of Virginia. By THOMAS JEFFERSON, *Eſq.*

IN a letter of July 3d, I informed our late moſt worthy preſident, that ſome bones of a very large animal of the clawed kind had been recently diſcovered within this ſtate, and promiſed a communication on the ſubject as ſoon as we could recover what were ſtill recoverable of them. It is well known that the ſubſtratum of the country beyond the Blue Ridge is a limeſtone, abounding with large caverns, the earthy floors of which are highly impregnated with nitre; and that the inhabitants are in the habit of extracting the nitre from them. In digging the floor of one of theſe caves, belonging to Frederic Cromer in the county of Greenbriar, the labourers at the depth of two or three feet, came to ſome bones, the ſize and form of which beſpoke an animal unknown to them. The nitrous impregnation of the earth, together with a ſmall degree of petrification, had probably been the means of their preſervation. The importance of the diſcovery was not known to thoſe who made it, yet it excited converſation in the neighbourhood, and led perſons of vague curioſity to ſeek and take away the bones. It was fortunate for ſcience that one of its zealous and well informed friends, Colonel John Stewart of that neighbourhood, heard of the diſcovery, and, ſenſible from their deſcription, that they were of an animal not known, took meaſures without delay for ſaving thoſe which ſtill remained. He was kind enough to inform me of the incident, and to forward me the bones from time to time as they were recovered. To theſe I was enabled accidentally to add ſome others by the kindneſs of a Mr. Hopkins of New York, who had viſited the cave. Theſe bones are,

1ſt. A ſmall fragment of the femur or thigh bone; being in fact only its lower extremity, ſeparated from the main bone at its epiphyſis, ſo as to give us only the two condyles, but theſe are nearly entire.

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* American Tranſactions, IV. 246.

2d. A radius, perfect.

3d. An ulna, or fore-arm, perfect, except that it is broken in two.

4th. Three claws, and half a dozen other bones of the foot; but whether of a fore or hinder foot, is not evident.

About a foot in length of the reſidue of the femur was found, it was ſplit through the middle, and in that ſtate was uſed as a ſupport for one of the ſalt petre vats; this piece was afterwards loſt, but its meaſures had been firſt taken, as will be ſtated hereafter.

(To be continued.)

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Letter from Dr. Beddoes; with a Propoſal for a Courſe of Lectures.

To Mr. NICHOLSON.

SIR,

IF you approve the ſpirit of the undertaking, announced in the encloſed paper, pray notice it in your Journal. I hope the example will be followed. Whether the number of perſons, aware of the paramount importance of the ſcience of animal nature, is at preſent ſufficient to enſure a reaſonable compenſation to thoſe who may attempt to explain it in lectures, I am not uncertain. But this æra is faſt approaching. I am perſuaded we ſhall ſee inſtitutions for this purpoſe, ſimilar to thoſe which have long exiſted, or been lately formed for important, but very inferior purpoſes.

The ſucceſs at firſt experienced in the treatment of palſy by the gas, before ſpoken of in your Journal, continues. I repeat my undoubting conviction, that the introduction of factitious air into medicine, will amply recompence the pains that have been taken to bring it about. I had engaged an able diſſector and draughtſman, for the purpoſe of co-operating with Mr. Davy and myſelf in phyſiological reſearches. But an opportunity offering for his advantage, I of courſe relinquiſhed my claims upon him; I am much in want of a ſucceſſor, and I wiſh ſome ingenious young man, accuſtomed to manage the ſcalpel and the pencil, may be induced, by this notification, to join us in an enterprize which muſt be congenial to the feelings of every ardent cultivator of medical philoſophy. I ſhould make the conditions as agreeable to him as it lies in my power to do.

I am, Sir,

Your's, with great eſteem,

March 2, 1800. THOMAS BEDDOES.

Lectures

Lectures on the Laws of Animal Nature, and on the Means of preſerving the Syſtem from Injury upon the moſt important Occaſions of common Life.

At ſome convenient place in *Briſtol*, Dr. Beddoes propoſes to attempt a popular expoſition of the principles of the animal œconomy, with their application to the purpoſes of individual and domeſtic welfare, upon a plan widely different from that of any exiſting publication. For his opinion on the advantage of diſſeminating phyſiological information, he may refer to his *Lecture introductory* to Meſſrs. Bowles & Smith's Courſe of Anatomy; and an exemplification of the manner in which he thinks the ſubject ought to be treated will be found in his *Eſſay on Conſumption.*

Heretofore an acquaintance with the cauſes of his perſonal condition has ſeldom been numbered among the *accompliſhments* of the ſcholar, or the *qualifications* with which the man of buſineſs is fitted out for ſucceſs in the world. Yet it will be confeſſed, that neither ſucceſs in buſineſs, nor proficiency in the ſciences, accounted *liberal*, are ſeparately ſufficient for rendering the condition of human life deſireable. And, in fact, to endeavour by any combination of theſe materials, to conſtruct a ſyſtem of perſonal happineſs, is to project an edifice which ſhall ſtand ſecure without a foundation.—Of a truth, ſo long and ſo generally neglected, a portion of the public, it is believed, begins to feel that degree of conviction which operates upon conduct. In this belief, the preſent opportunity of inſtruction is offered to thoſe who may be deſirous of it.

If it be allowed that the moral and phyſical attributes of human nature are inſeparable, perſons intereſted in the art of education will ſcarce require to be reminded of the value of that ſpecies of knowledge which the lectures, here announced, are intended to communicate.

They ought to prevent many of thoſe mortal bruiſes which travellers along the road of life give themſelves for want of knowing the quality and poſition of the objects in their way.

By preſenting a juſt eſtimate of *that art* to the operations of which almoſt every one is ſooner or later doomed to ſubmit, they ſhould afford ſome protection againſt groſs medical incapacity or fraud.

They ſhould reduce to their juſt value many of thoſe axioms that wander about the world concerning what is wholeſome or unwholeſome in diet or exerciſe:—axioms which the inſtinct of ſelf-preſervation impels men to take up; and upon which, however looſely adopted, they act with as full aſſurance as if they knew them to have the moſt ſolid foundation in phyſiological ſcience.

Numbers fall victims to their own impatience under illneſs, or to the wavering conduct of their friends. Frequently in the onſet of dangerous diſeaſes, people by ſuffering themſelves to be amuſed by trifling domeſtic expedients, loſe an opportunity which no medical ſkill can ever retrieve. Upon theſe evils the prevalence of juſter ideas would act as a check. Nor is it paradoxical to ſuppoſe that the mortality among infants would be ſmaller, and debility of conſtitution at all periods of life more rare, if parents (however inſtructed in other things) were not in common nearly upon a level with nurſes in that which it ſo much imports them to poſſeſs—an acquaintance with the powers that operate to the injury or advantage, the deſtruction or preſervation, of the objects of their affection.

The Author further hopes (if he may repeat his own words) to contribute towards preventing the "ignorant from tampering with the ſick; towards promoting the aſcendancy "of ſcience over intrigue; alluring curioſity from the pernicious frivolities of literature, "and elevating the conceptions of men to the level of their higheſt intereſts."

As the whole courſe will be connected, the tickets will not be transferable—The number of lectures cannot be determined beforehand—But that there may be little chance of excluſion by reaſon of narrow circumſtances, the ſubſcription is fixed at ONE GUINEA—The lectures will be calculated for both ſexes and different ages—They will be delivered in the evening, and commence ſometime in April next—probably near the middle of the month—provided fifty perſons ſhall have entered their names by the 31ſt of March. This condition is indiſpenſable. Without a tolerably numerous audience, the author preſumes he could beſtow his time in a manner more advantageous to the public.

Subſcriptions received by Mr. SHEPPARD, Bookſeller, *oppoſite the Exchange*, with whom conditions for printing a SYLLABUS may be ſeen.

RODNEY-PLACE, CLIFTON, *March* 3, 1800.

Extract of a Letter from M. Fabbroni of Florence, to Cit. Van Mons, on the Ethiops of Iron, the Formation of Alcohol and Fermentation. (*An. de Chim. XXX.* 220.)

Take a pound of iron filings, reduce them to a paſte with water, and put it in a capſcule, or what is ſtill better, a glaſs matraſs, kept in a water bath at about 50 or 60 degrees (Reaumur). Pour on it gradually one or two ounces of aqua fortis, rather diluted, or of nitrous acid very much diluted, and continue ſtirring it with a ſpatula. It is remarkable, that it undergoes a kind of effervefcence, after which the iron is changed into a very fine black powder, and oxided to the firſt degree of oxidation; that is to ſay, converted into martial ethiops. The operation is performed in leſs than half an hour. If the mixture be put in a cloſed veſſel, and not ſhaken, but left from evening till the next day, the ſurface of the ethiops is found to be covered with a kind of champignons extremely white, and ſeveral lines high, which are nothing but volatile alkali, or carbonate of ammoniac. The air in the veſſel is in this laſt caſe, compoſed in a large portion of oxigenated nitrous gas. A decompoſition here takes place of the water and nitrous acid, both at the ſame time, by the iron which ſeizes their oxygen, in order to become converted into the oxide; and the component parts of theſe two liquids, that is, the azote and hydrogen, which being diſengaged at the ſame time, meet whilſt they are yet in a ſtate of condenſation, or before they have taken the form of a gas, by combining are transformed into ammoniac. A portion

tion of oxigen appears alſo to be precipitated on the carbon of the iron, which affords the carbonic acid, and cryſtalliſes the ammoniac, by ſaturating it as it is formed.

Chemiſts in general, have believed and advanced, that alcohol is the produce of the vinous fermentation. I am aſſured that it is not all ſo, for it does not exiſt in wine. I think I can render this very evident, if I have a method by which I can recover an hundredth part of alcohol, when completely mixed with ſtrong wine, in which, by the ſame method, I could not before diſcover the ſmalleſt atom, although I could obtain 20 or 25 of brandy, from a hundred parts by diſtillation. Theſe were the ſimple means by which it was performed: take a glaſs tube of ſufficient ſize to introduce the finger, and divide it into an hundred equal parts; take freſh wine, with which you have mixed one hundredth part of alcohol, and to that put as much pot-aſh in powder, as is found by a previous experiment, to be neceſſary to precipitate the reſinous coloring reſidue. Then ſtrain the wine, and pour it into the tube, and add as much pot-aſh in powder as is required to ſaturate it. The centime of alcohol, which had been mixed with the wine, will then be ſeen to riſe, and ſwim diſtinctly on the ſurface of the alkaline ſolution. It will be found of the ſame degree of ſtrength, and in the ſame proportion that it had been added, if the operation is performed with ſufficient quickneſs to avoid loſing any thing by evaporation. The previous ſeparation of the coloring matter ſerves merely to render the reſult more perceptible. Now, if by this means I procure only the ſame quantity of alcohol from the wine that I knew it contained before, and not a drop more, it appears to me, I have a right to conclude, that that which I obtain from the ſame wine by diſtillation, did not exiſt in it before, but that it is the operation of diſtillation which forms it; that its formation is determined by the heat; and laſtly, that this liquid is a product, and not an educt of the diſtillation of the wine. The heat neceſſary to effect this is not therefore very conſiderable, for it is formed by the heat of the fermentation, to an heat of diſtillation of 14 degrees, ſuch as may be obtained during winter. It may be formed in bottles by the warmth of the atmoſphere, &c. It is for this reaſon, that I have recommended new wine to be uſed in my experiment. All this has been printed in my *Arte di fare il vino*, which was publiſhed at Florence in 1788; but Italian books ſeldom paſs the Alps. The anatomical examination of the grape precedes the chemical part in this work. The theory of fermentation is there found explained according to the ancient principles, though at that epocha I had, for a conſiderable time, renounced the phlogiſtic theory. The eſſential ingredients, the active and paſſive principles, that is to ſay, of fermentation, are there deduced from experiment; and the making of artificial wine is the confirmation of them. The work is concluded, by expoſing the chemical means neceſſary to diſcover the alteration which adulterated wines undergo, and a ſummary of all the theories of fermentation that have been preſented, to the time of its publication. You will there, perhaps, find a phenomenon which contradicts them all, and which is very remarkable. It is that the acceſs of air is not eſſential to fermentation; for I have excited it in the vacuum of Toricelli.

Principles of modern Chemiſtry, ſyſtematically arranged, by Dr. Frederic Charles Gren, late Profeſſor at Halle, in Saxony. Tranſlated from the German, with Notes and Additions concerning late Diſcoveries, by the Tranſlator, and ſome neceſſary Tables. Illuſtrated by Seven Plates. In two Volumes, Octavo, 964 Pages. Price 16s. Cadell and Davies.

The character of the late Dr. Gren as a chemical philoſopher is well known; as is likewiſe the value of the work from which the preſent tranſlation is made. I ſhall, therefore only give a ſhort account of the plan. After a conciſe hiſtorical introduction, the author enters upon his ſubject, which he divides into twelve chapters, each of which is again ſubdivided into heads or titles, and thoſe into numerous paragraphs or ſections. The firſt treats of preliminary or general objects, and the operations and inſtruments of chemiſtry. 2. The ſimple ſubſtances and proceſs of combuſtion. 3. Salts in general. 4. Earths. 5. Mineral Acids. 6. Conſtituent parts of vegetables. 7. Of animal ſubſtances. 8. Spontaneous changes in the mixture of organic bodies, fermentation, &c. 9. Bitumens. 10. Mineral coal. 11. Metals. To theſe the tranſlator, beſides many uſeful notes and additions through the work, has added an appendix, containing the doctrine and tables of chemical attraction, the chemical characters of Haſſenfratz and Adet, ſpecific gravities, compariſon of thermometers, of weights and meaſures, together with an index to the whole.

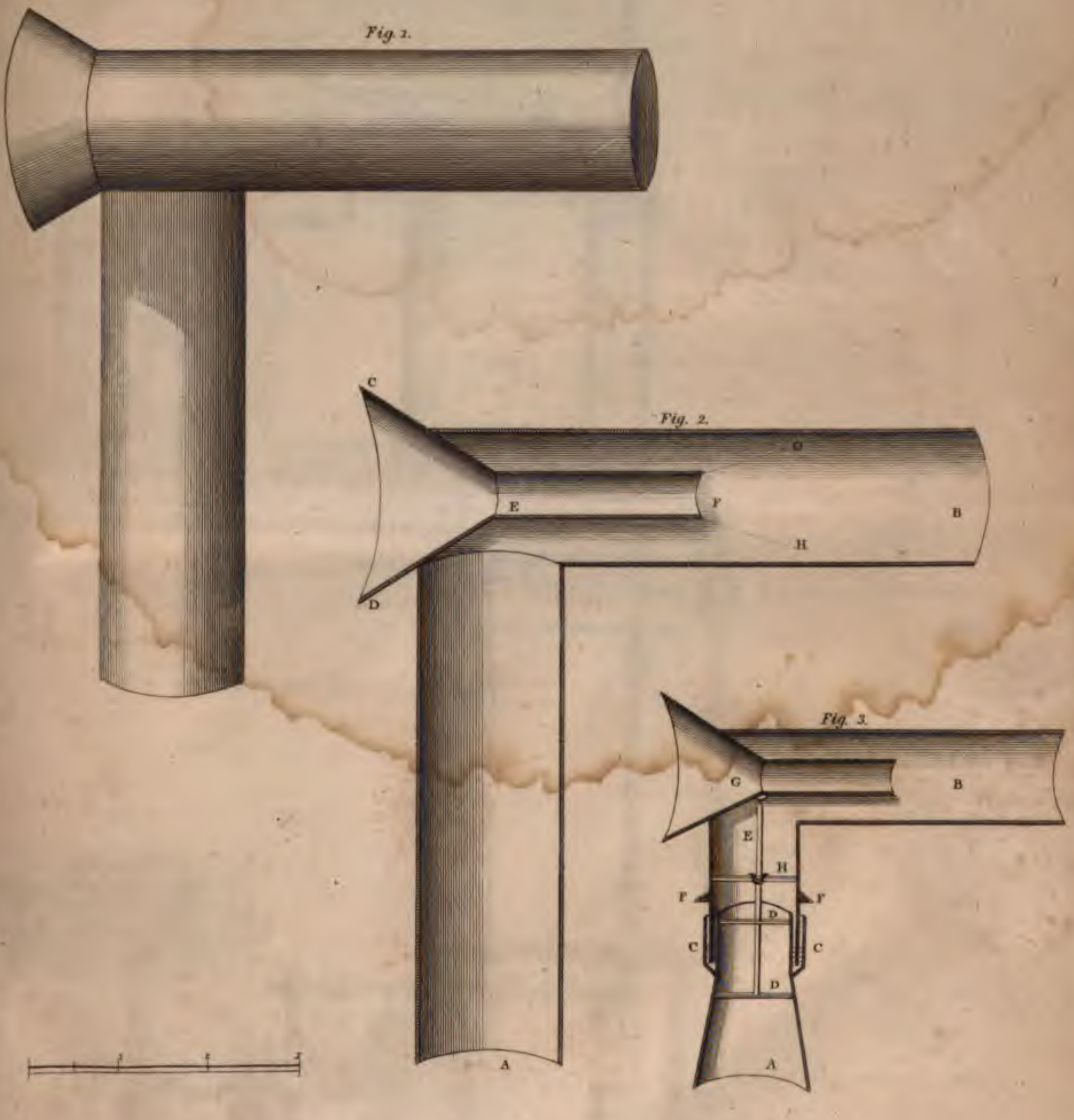
The Chemical Pocket Book, or Memoranda Chemica, arranged in a Compendium of Chemiſtry, according to the lateſt Diſcoveries, with Bergman's Table of ſingle elective Attractions, as improved by Dr. G. Pearſon. Calculated as well as for the occaſional Reference of the profeſſional Student as to ſupply others with a general Knowledge of Chemiſtry. By James Parkinſon. Small Twelves, 229 Pages. Price 5s. Symonds.

This is a very good compendium. By uſing the ſmall type, called burgeois, the author has contrived to give nearly the ſame quantity of matter in the pages of his little pocket volume as is uſually contained in an octavo page. The greater merit, however, conſiſts in the induſtry he has exerted in collecting the numerous particulars of modern chemiſtry from the moſt authentic ſources, and condenſing them with a degree of neatneſs and perſpicuity which will render his book equally uſeful as a manual of ſcience to the philoſopher and the ſtudent. He quotes his authorities much more frequently than is the modern cuſtom; but it will greatly add to the value of his book if in a future edition he ſhould refer in all places by the real title and page to every author of eminence to whom he might wiſh to refer his reader.

Royal Inſtitution of Great Britain:

A courſe of philoſophical and chemical lectures is at preſent delivered at the houſe of the Royal Inſtitution by Dr. Garnett, which are received with the moſt marked attention by an audience of the firſt reſpectability. The ordinances, bye laws, and regulations of this eſtabliſhment have likewiſe been printed. They conſiſt, for the moſt part, of a developement and organization of the ſubjects contained in their proſpectus of laſt year. The managers are charged to erect a laboratory and theatre for lectures, and to appoint profeſſors. They elect all the ſubſcribers. Proprietors are to be propoſed as candidates by a manager, and elected at the ſubſequent monthly meeting by a majority of two-thirds, and the ſum to be paid will be 60l. after May, 1800;—70l. after May, 1801;—80l. after May, 1802;—90l. after May, 1803; and 100l. after May, 1804. Subſcribers for life, or annual, muſt be propoſed by a manager, and elected at the ſubſequent weekly meeting. Ladies for theſe two claſſes are admitted by recommendation of certain ladies intruſted by the managers to hold books for that purpoſe. I do not find any ſpecific mention of the ſums to be paid by life and annual ſubſcribers; but upon enquiry at the houſe, I am informed, that on Friday the 7th of this month, the ſum for the former claſs was raiſed from ten to twenty guineas, and for the latter from two to three guineas. Whether the managers of this great eſtabliſhment have ſhewn a *laudable* partiality to their own claſs by giving them five regular notices of the advances they are to pay, while the the reſt of the public is called upon without ceremony for augmentation of fifty and one hundred per cent. may perhaps be queſtioned.

Mr. Boswell's Blast Ventilator.



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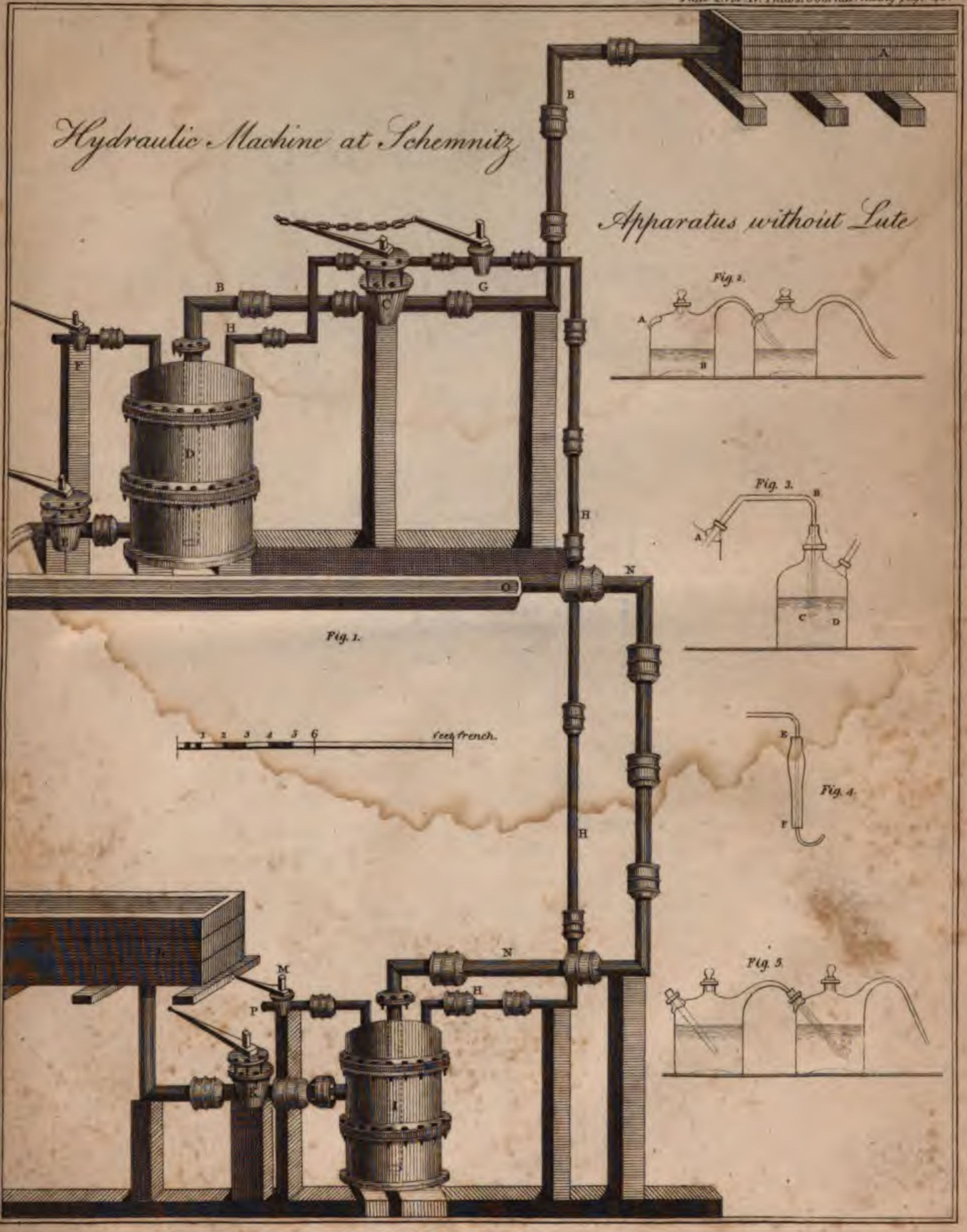

Hydraulic Machine at Schemnitz

Apparatus without Lute

Fig. 1.

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MAY 1800.

ARTICLE I.

On the different Effects of the Alkalis in the Production of Alum. By Professor HILDEBRANDT, *of Erlangen*[*].

It is a known fact, in the uſual preparation or extraction of alum, that pot-aſh, or urine, is added to the firſt ſolution, which is procured by lixiviating the roaſted and weathered aluminous ores. *Bergman*[†], being of opinion that theſe additions tended only to ſaturate the ſuperabundant acid of the lye by which the cryſtallization of the ſalt is impeded, has adviſed to add pure aluminous earth, or argil, inſtead of the preceding ſubſtances, on the ſuppoſition that thoſe additions do contaminate the alum with foreign ſalts; while, on the contrary, pure alumine would not only ſaturate the ſuperfluous acid, but increaſe the quantity of the ſulphate of alumine. *Profeſſor Lampadius*[‡], who adopted the ſame opinion, tried by experiments, according to Bergman's idea, whether he could not render the addition of pot-aſh or urine unneceſſary, and with this view he had, in the alum-works of Lederbur at Weiſgrun, added to the crude lye ſome white argil, dug up near the village Chotta.

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[*] Tranſlated from Dr. Scherer's (now Profeſſor at Halle in Saxony) Chemical Journal, Vol. II. pag. 419.

[†] *Bergman, de confectione aluminis, § II. Opuſc. Chem. et Phyſ. I. Lipſ.* 1788. *8vo. pag.* 308. Quum autem argilla pura baſin aluminis conſtituat, excedens lixivii acidum nullo alio modo melius, quam ea donatur, quæ ſuperfluum acidum non tantum aufert, ſed etiam ſalis quæſiti copiam auget.

[‡] *Lampadius*, pract. chemiſche Abhandlungen I. Dreſden 1795, § 14.

The ſuperabundant acid here ſpoken of, is not that portion which is eſſential to the alum, the acid in this ſalt being never fully ſaturated with argil; but that quantity only is meant, which may happen to be preſent in the lye as foreign, or not belonging to the compoſition of alum, as is the caſe, when the aluminous ore is too abundant in ſulphur, or when a ſufficient portion of its argillaceous part has not been combined with the ſulphuric acid generated in the proceſs.

If we admit that the crude lixivium of alum does really contain an exceſs of acid, and that it is this only which impedes the cryſtallization of the ſalt, the addition of argil ſeems much more proper, than that of either pot-aſh or urine; becauſe more alum muſt be obtained by the former, while the latter affords ſulphate of pot-aſh or ammoniac, which diminiſh the purity of the product.

But *Marggraf*[*] has long ago obſerved, that the ſulphate of alumine cannot be generated from argil and ſulphuric acid alone, and that an alkaline ſalt is one of its eſſential ingredients. To prove this fact, I have ſeveral times diſſolved ſome aluminous earth (that had been precipitated from Roman alum by means of pot-aſh, and properly edulcorated) in ſulphuric acid carefully rectified, and attempted to cryſtallize the clear filtered lye, by evaporation and cooling. But I never obtained the leaſt appearance of cryſtallized alum. The mixture conſiſting of argil and ſulphuric acid, rather in exceſs, is more ſoluble in water than true alum: ſo that two parts of water at a temperature of 50° Fahrenheit, are ſufficient to diſſolve one part of that maſs, merely dried without ignition; whereas the alum requires at the ſame temperature, 35, or more parts of water, to diſſolve it. Even after that lye had been reduced by evaporation to a much ſmaller compaſs, than a lixivium of true alum would admit of without cryſtallizing, it continued liquid, though expoſed to a freezing cold. On evaporating it ſtill farther, and by ſubſequent cooling, it affords indeed a compact maſs, but without any determinate figure, which occupies the bottom of the veſſel in the form of a dried jelly. It was but ſeldom that, in a certain degree of the evaporation, I could perceive a kind of cryſtallization in groups; but there was no appearance of ſuch hard octahedral cryſtals, as preſent themſelves in a ſolution of true alum, containing upwards of twenty parts of water to one of ſalt diſſolved in it[†].

If, on the contrary, to the above ſolution of argil in ſulphuric acid, a ſmall quantity of vegetable alkali only be added, and it be then evaporated and left to cool, regular cryſtals of alum will be obtained at each ſubſequent refrigeration.

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[*] *Marggraf*, Chim. Schriften. I. Berlin, 1768. S. 196.

[†] *Marggraf*, however, obſerved that when the argil was ſtrongly calcined, it afforded ſome cryſtals with the ſulphuric acid, which reſembled thoſe of alum, but were not ſo fine as thoſe obtained by the addition of an alkaline lye. I wiſh he had ſtated this more accurately. But, as my preſent object was to aſcertain, which of the alkalis may be moſt ſuitable to the generation of alum, I have not yet repeated that experiment.

In order to diſcover the proportion of the alumine to the ſulphuric acid, nothing more is required, than to combine as much of the earth with the acid, as it will diſſolve in the common temperature (from 50° to 60° Fahrenheit). An exceſs of the acid will, in that caſe always remain, as ſhewn by the teſt of Litmus. For at that temperature the acid does not take up ſo much of the argil as to become neutralized. The ſolution muſt, however, be left ſtanding long enough (24 hours) upon the remaining undiſſolved part of the earth, becauſe the argil is diſſolved much more ſlowly by this acid, than the calcareous and magneſian earths.

In order to diſcover the due proportion of pot-aſh in this proceſs, *Marggraf* directs us to continue adding gradually of the alkaline lye by drops, till a light pulverulent precipitate (that is to ſay, precipitated argil) appears. In the following experiments I added ſo much alkali, whether pot-aſh, or either of the other two, that (the quantity of diſſolved pure alumine being half an ounce) there remained a ſufficiently perceptible quantity of precipitated earth, though very ſmall in proportion to the whole. For, it is to be obſerved, that the very firſt drops of the alkaline lixivium, if neither itſelf nor the ſolution of the earth be too weak, produce a turbidneſs; but this firſt precipitate is re-diſſolved in the exceſs of the acid preſent. I am much inclined to ſuppoſe, that it may be immaterial to determine the preciſe quantity of alkali, becauſe I have conſtantly obtained alum by the addition of pot-aſh, notwithſtanding that in ſome inſtances I added more of it, and in others leſs.

To aſcertain the regular form of the cryſtals of alum, one or more threads ſhould be hung into the lye, in order that perfect, or nearly perfect octahedrons may be formed round them; for thoſe cryſtals which are formed at the bottom, or on the ſides of the veſſel, are always defective in one or more of their angles.

Having decided from theſe experiments, that a ſmall portion of alkali is eſſentially requiſite to the production of ſulphate of alumine, I mentioned the fact in the advertiſement of *Prof. Lampadius's Chemical Eſſays*. It cannot be denied, but that in one of his experiments [*] he has obtained alum, from mere argil and ſulphuric acid, without the addition of either pot-aſh or urine: but not to mention, that the quantity of alum obtained by the ſaid proceſs was indeed exceeding ſmall, in proportion to what has been produced on adding pot-aſh or urine, it is probable, that as much vegetable alkali as was ſufficient for the formation of that ſmall portion of alum may have entered into the crude lixivium, from the aſhes of the wood employed in the roaſting of the aluminous ore, and of which a greater or leſs portion is unavoidably mixed, with the roaſted mineral. Or that alkali may actually have pre-exiſted in the ore; (or if not, at leaſt the nitrogen and hydrogen, from which the alkali was generated, may have been preſent.) *Profeſſ. Klaproth* [†] has diſcovered pot-aſh in

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[*] *Lampadius*, loc. cit. pag. 16.

[†] *Klaproth* diſcovered in the *native alum* from *Miſeno* ſo large a proportion of pot-aſh, that 1000 grains of it afforded 470 grains of cryſtallized alum, and in theſe 27 grains of ſulphate of pot-aſh. (*Beytrage zur Kentniſs der Mineral Koerper*. I. Berlin, 1795. deile 313.)

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the *native alum* from *Miſeno.* Only one part of its aqueous ſolution would cryſtallize into genuine alum, in which alſo he traced out the alkali; the remaining portion of the lye yielded no alum ſpontaneouſly, but only when a little pot-aſh had been ſuperadded. *Bergman* * likewiſe, found that alkali in the artificial alum, even in the Roman; and Prof. *Lampadius* † himſelf conſiders it at preſent as an eſſential principle of alum, in conſequence of having found it in four different ſpecies of factitious alum: namely, thoſe from *Commotin* and *Weiſsgrün* in Bohemia, and thoſe from *Reichenbach,* and from the *Schwemſal* in Saxony.

On conſidering the ſhare, which the vegetable alkali has in the production of alum, the queſtion naturally ariſes: whether *pot-aſh* alone, or *ſoda* likewiſe, as well as *ammoniac,* are capable of combining with ſulphuric acid and argillaceous earth into alum ‡?

The aptitude of volatile alkali to enter into ſuch union, ſeems to be deducible from the circumſtance, that, in ſome alum-works, urine only and no pot-aſh is employed. However, as I have already obſerved in another reſpect, it is not impoſſible but that a quantity of vegetable alkali may be afforded by aſhes formed in the roaſting of the alum-ore, ſufficient to complete the alum.

The alkaline portion of the alum may be aſcertained in three different ways by analyſis.

1. Let the argil be thrown down from a watery ſolution of alum by means of *ammoniac*; cauſe then the ſalt contained in the clear decanted lixivium to cryſtallize, and ſubſequent to this expoſe it to a ſubliming heat. In this proceſs, the *ſulphate of ammoniac* ſublimes, but the *fixed ſulphate of pot-aſh,* if any has been in the alum, remains behind it in a compact form. This method diſcovers the preſence of fixed alkali only.

2. To a boiling hot aqueous ſolution of alum, add carbonate of lime. By this management the ſulphate of argil is decompoſed, and the lime is converted into gypſum; at the ſame time that the ſulphate of pot-aſh, if any was preſent, remains unaltered. This laſt would likewiſe be decompoſed by cauſtic or pure lime; but it is well known that the crude calcareous earth, on account of its ſtrong attraction for carbonic acid, has no ſuch power on ſulphate of pot-aſh. What quantity of crude lime may be neceſſary to accompliſh that decompoſition, may in ſome manner be determined, by the known proportions of argil to ſulphuric acid in alum, as well as by the proportion of calcareous earth to the ſame acid in gypſum, and laſtly that of crude lime to carbonic acid in carbonate of lime. However, as this determination is ſubject to ſome difficulties, it will be ſufficient to be attentive to the efferveſcence of the lime, and according to this effect, to add rather too much than too little, becauſe the redundant calcareous earth falls down, unchanged, together with the

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* *Bergman,* cit. § 77, page 307. Yet he ſays: *haud raro.*

† *Lampadius,* Pract. Chim. Abhandlungen II. 1797. S. 228.

‡ *Marggraf* (cit. loc. page 196) ſays only, that the addition of fixed alkali is neceſſary.---*Lampadius* (cit. p. 230) has not yet given any reſults of his experiments, made with the mineral and volatile alkali.---*Bergman* (cit. 307.) ſays: *cryſtallizationis obſtaculum alcali volatili æque tollitur, non vero alcali minerali et calce*; yet without relating any experiment.

precipitated argillaceous earth, from which it may be ſeparated by the known methods, if it ſhould be required to aſcertain the quantity of the argil. A certain portion of the ſulphate of lime, thus produced, is likewiſe precipitated at the ſame time, and in greater quantity, the leſs water there is in the ſolution. The remainder of the gypſum continues diſſolved in that fluid. Let the liquor be carefully poured off from the precipitate, and let this laſt be edulcorated with a ſufficient quantity of boiling water; let the waſhings be added to the fluid at firſt decanted; and the whole of the fluid be gradually evaporated, and left to cool, by alternations. By this management the gypſum will by degrees be ſeparated, and afterwards the ſulphate of alkali, which was an ingredient into the alum, whether it has been pot-aſh, or ammoniac. The ſulphate of lime will fall firſt, becauſe moſt difficultly ſoluble; the ſulphate of pot-aſh will be the next in order; and, laſtly, the ammoniac, being the moſt ſoluble of all. If at the ſame time the alum under examination has contained a portion of ſulphate of ſoda *, it will be more difficult to ſeparate this from the ſulphate of ammoniac, becauſe it is alſo very ſoluble. But it will preſently be ſhewn, that no ſoda is contained in alum.

3. Pour into a ſaturated ſolution of alum, in ſix times its quantity of water (heated to about 120° Farenh.) a ſaturated ſolution of acetate of lead. Sulphate of lead will be produced, which falls down; and alſo, the acetate of alumine as well as the acetate of pot-aſh (or of ammoniac), which, being readily ſoluble, remain diſſolved in the water. The ſupernatant fluid muſt then be decanted from the ſulphate of lead, and ſubjected to evaporation, until it beomes turbid; for by this means a little of the metallic ſulphate, that was ſtill held in ſolution by the water, is thrown down. When the fluid has been a ſecond time filtered, it muſt be evaporated to dryneſs. The dry reſiduum conſiſts of acetated argil and acetated pot-aſh. And if it be then ignited, the acetic acid, together with the ammoniac, if preſent, will be volatilized. Water muſt now be poured upon the reſidue; which, if only ammoniac has entered into that compound, extracts nothing from it, by reaſon of the volatilization of this alkali, and of the inſolubility of aluminous earth in water. But if a portion of vegetable alkali was contained in it, it is on account of its fixity, left behind in the argil, and will be diſſolved in the water. The alkali receives ſome carbonic acid from the deſtruction of the acetic, but is ſtill far from being ſaturated with it; and hence a part of the argil will be diſſolved along with it by the water. The earth muſt, in that caſe, be precipitated, by ſaturating the ſolution with carbonic acid, or letting it ſtand for a long time in open veſſels, ſlightly covered.

Analytical experiments alone are, nevertheleſs, inſufficient to decide with certainty, whether all the three alkalis, or only one, or two, of them are by their acceſſion capable of producing alum; and therefore our arguments cannot yet reſt on a complete induction. For, ſuppoſing I had decompoſed artificial as well as native alum in a thouſand different ways, and had found no ſoda in it, I cannot fairly conclude that the mineral alkali is

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* I need not here obſerve, that ſalts of more difficult ſolubility are, for the moſt part, (though not perfectly) ſeparated by the gradual and ſucceſſive diminution of the water, and cryſtallization.

unfit for the production of alum. Not to mention the few ſpcies of alum, which I have examined, I ſhould not chooſe to draw ſuch an inference from the reſults I did obtain from them.

Synthetical experiments, on the contrary, I am perſuaded, ought to decide in this caſe. And by theſe, (although *Marggraf* mentions, in a general way only, the pot-aſh as a neceſſary requiſite to the generation of ſulphate of alumine, without excluding the ſoda) I have until the preſent moment been convinced, that the combination of ſoda with ſulphuric acid and argil does *not* yield *any alum*, I mean that very ſalt, which is known by that name; but that it is generated by the addition of pot-aſh.

I employed, in my experiments, native ſoda (natrum) from Hungary, purified in a high degree. It was this purity that induced me to inſtitute thoſe experiments. And to aſſure myſelf that no portion of pot-aſh did interfere, I have precipitated the argil from Roman alum, not by means of pot-aſh, but by ſoda. If, therefore, ſomewhat of the neutral ſulphate, generated by the precipitation, had remained in the earth after it was edulcorated, it muſt neceſſarily have been ſulphate of ſoda.

The precipitated alumine was then properly waſhed; and I thought it needleſs to free it, according to *Richter*, from the adhering ſulphuric acid, the preſence of which was not detrimental in this inſtance. I then diſſolved it in purified ſulphuric acid, in exceſs, ſo that the ſolution reddened litmus-paper dipped in it. As I have already obſerved, the due proportion of the earth to the acid is found of itſelf, by permitting the acid, moderately diluted, to diſſolve as much of the earth as it can in a low temperature. When I afterwards added to the filtered ſolution ſucceſſive portions of diſſolved carbonate of ſoda, till part of the alumine itſelf began to precipitate, I filtered the ſolution once more, and began to attempt the cryſtallization of the ſalt, by gradually repeated evaporation and alternate cooling.

But in none of theſe inſtances did I obtain alum; but rather a *very ſoluble ſalt*, altogether *different* in its *form*. This combination is of ſo eaſy ſolution, that I might at firſt have repeatedly ſuſpected an accidental tranſpoſition of my evaporating diſhes, if I had not been ſure of the contrary, by the ſmall quantity of the reſidual liquor, and its refuſing to cryſtallize. That great ſolubility agrees very well with that of Glauber's ſalt (ſulphate of ſoda) as *vice verſâ*, the difficult ſolution of alum correſponds with that ſulphate of pot-aſh. When, in one of my experiments, I added juſt ſo much of ſoda, that the lixivium began to acquire a permanent cloudineſs (owing to the argillaceous particles firſt precipitated, and which are rediſſolved by the redundant acid, if the fluid be agitated, and thus the earthy particles be brought into contact [*] with its not yet alkalized part), there reſulted from its farther evaporation and cooling ſome minute, ſingle, ſhort needled cryſtals, reſembling thoſe of gypſum. Theſe, however, merely contained aluminous, and no calcareous earth; for they readily diſſolved in water; the oxalic acid precipitated nothing from their ſolution; pure

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[*] The earthy particles newly precipitated are, in general, more ſoluble, than thoſe of a longer ſtanding and deſiccated; becauſe the former are ſubtilely divided, and moiſtened through the whole of their maſs.

(cauſtic) ammoniac, on the contrary, threw down the argillaceous earth they contained. And when the lye was ſtill farther evaporated, it coaleſced upon refrigeration into a coherent ſaline maſs, in which, however, the needle-ſhaped texture could be diſtinguiſhed. In another experiment, in which I added a little more of (the ſame) ſoda in ſuch proportion that the quantity of precipitated argil became ſomewhat more conſiderable, I obtained by evaporating the filtered lixivium to a ſmall remainder, and as it cooled, longiſh, flat, tabular ſix-ſided cryſtals; (that is, very low hexagonal priſms, with broad ends and very low lateral facets, the two oppoſite ones of which were longer, and the other four ſhorter.) After the mother-water, which in proportion to the cloſely grouped cryſtals was but ſmall in quantity, had been decanted, and the cryſtals waſhed with a little cold water, I attempted to take them out of the veſſel by means of a ſilver ſpoon; but they were too ſoft, and were immediately compreſſed. Cold water (at 60° Farenh.) eaſily and quickly diſſolved them; I have not yet determined the quantity required for that purpoſe; but certainly no more than *three* parts are neceſſary for *one* of the ſalt. The ſolution taſted like that of alum; it gave to paper, ſtained with litmus, of a faint red; (the undiminiſhed ſolution, before the ſhooting of the cryſtals, afforded a ſtronger red); whence it follows, that there was a ſmaller proportion of the acid in the cryſtallized ſalt than in the whole fluid.

My attempts to produce alum by the acid of ſoda, being thus fruſtrated, I doubted very much whether *ammoniac*, the properties of which is far leſs analogous to thoſe of pot-aſh, than the properties of ſoda, would ſerve to produce alum, by combination with ſulphuric acid and argillaceous earth. However, experience teaches that in this as well as in various other caſes, reaſonings from analogy prove very often incorrect, when reſorted to in our enquiries into nature. Precipitate the argil from an aqueous ſolution of Roman alum, by means of ammoniac, waſh the precipitate well; diſſolve it then in pure ſulphuric acid, in the manner pointed out above; and then continue adding ammoniac (either carbonated or cauſtic) by degrees, until the ſolution begins to aſſume a permanent turbidneſs. Let the whole then be reduced by evaporation ſo much, that for *one* part of the diſſolved ſalt (the quantity of which may be aſcertained by the quantity of alum employed at the beginning of the proceſs) there ſhall remain from ſix to eight parts of water. As this lye cools, a ſalt will cryſtallize, which, with reſpect to its octahedral form, (in the beſt way to be perceived around the threads hung into the liquid) to the ſolidity and hardneſs of its cryſtals, and to its difficult ſolubility, &c. perfectly agrees with the alum, which is formed by means of pot-aſh *.

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* On this ſubject ſee this Journal, I. 318.

Experiments

II.

Experiments on Whinſtone and Lava. By Sir James Hall, Bart. F.R.S. & F.A.S. Edin*.

(Concluded from page 18.)

THE ſuppoſitions which theſe gentlemen have thus advanced, and have ſeriouſly maintained in various parts of their works, have ariſen in both from the belief, that, in our fires, nothing but glaſs can be produced from a lava after complete fuſion. This being taken for granted, it would certainly be very difficult to explain the phenomena of actual eruptions, by means of the known agents of nature. Recourſe has therefore been had, by one of theſe gentlemen, to a hypothetical modification of theſe agents; and by the other to the influence of ſubſtances, which have left behind them no trace of their exiſtence †, and which, had they been preſent, could not have produced the effects aſcribed to them.

According to both ſuppoſitions, the heat of volcanos is conceived to be of very little intenſity; but the few obſervations I had occaſion to make, which are confirmed by innumerable facts related by travellers, convince me that it muſt far exceed what is requiſite for the moſt perfect fuſion of the lavas, and of all the ſubſtances contained in them ‡; and the experiments already deſcribed ſuperſede the neceſſity of ſuppoſing any thing different from the common courſe of nature; for they afford, analogically, an eaſy ſolution of the difficulty, by ſhowing that glaſs is not the only reſult of fuſion, and that whin, a ſubſtance like lava, when cooled ſlowly after fuſion, reſumes its ſtony character. But, not content with analogy alone, I reſolved to aſcertain the truth of theſe concluſions in a direct manner, and performed the following experiments with ſpecimens of ſix different lavas, four of which, to my certain knowledge, had made part of external volcanic currents. In the preſent ſtate of geology, too much pains cannot be beſtowed in aſcertaining that the ſpecimens collected are really lavas, ſince this circumſtance has been frequently overlooked, as I ſhall endeavour to ſhew, when I ſpeak of the differences between them and whinſtone.

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* It ought to have been noticed in the laſt Number that this paper is inſerted in the Edinburgh Tranſactions, though, by ſome culpable neglect, the copies of that work have not yet reached London. N.

† None of the lavas I have ſeen contained the ſmalleſt veſtige of petroleum; nor did I meet with any ſulphur but what was evidently produced by the condenſation of vapours, riſing through crevices, long after the eruptions had ceaſed.

‡ I conceive, therefore, that the formation of the inſulated ſubſtances contained in lavas, as well as the other peculiarities of internal ſtructure, poſſeſſed by lavas in common with granite and baſaltes, muſt be aſcribed in all of them to cryſtallization during ſlow cooling after fuſion, as I had ſtated formerly in Spring 1790, (*Tranſ. Edin. vol. III.*) The year following, Dr. Beddoes preſented to the Royal Society of London a paper, in which he alſo explains the character of granite and baſaltes by cryſtallization, in conſequence of ſlow cooling.

No. 1. *Lava of Catania.*

This is the celebrated lava, which, in 1669, laid waſte great part of the town of Catania. The interior part of the current (accurately deſcribed by M. Dolomieu, *Iſles Ponces*, p. 256 *), from which the ſubject of our experiment was taken, conſiſts of a light grey baſis, interſperſed with cryſtals of felſpar and of ſchorl, (augit). It bears a general reſemblance to the rock of the baſaltic columns on Arthur's Seat, and exhibited the ſame phenomena in our experiments. After ſtrong heat, the whole was reduced, by rapid cooling, to pure black glaſs; but when the heat applied was moderate, the felſpars remained unchanged. Being maintained, after a ſecond fuſion, in a temperature of 28, both theſe glaſſes yielded ſtony and cryſtallized ſubſtances, ſomewhat leſs fuſible than the original; and when expoſed to a temperature of 22, they cryſtallized rapidly, like moſt of the whins, into the liver cryſtallite. This laſt property is common to all the lavas.

No. 2. *Lava of S^ta^ Venere.*

This current has flowed in the neighbourhood of a little chapel, called S^ta^ Venere, above the village of Piedimonte, on the north ſide of Mount Ætna. Owing to the ſtrong reſemblance which it bears to ſtones ſuppoſed not volcanic, we took care that our ſpecimens ſhould be broken from the actual current; and to one of them, though moſtly compact, is attached a ſcorified maſs, which had made part of the external ſurface. The ſolid part is of a black, or rather dark blue, colour; very fine grained and homogeneous, having a multitude of minute and ſhining facettes viſible in the ſun; in this, and in other circumſtances, it greatly reſembles the rock of Edinburgh caſtle. This lava is the ſecond in M. Dolomieu's *Catalogue*, and is well deſcribed, p. 186 †.

The pure black glaſs formed from this lava yielded, in the regulated heat, the moſt highly cryſtallized maſs we obtained from any lava or whin.

No. 3. *Lava of La Motta di Catania.*

This is likewiſe compact and homogeneous, but for a number of ſmall yellow grains of chryſolite ſcattered through it, (deſcribed by M. Dolomieu, p. 191 ‡). It has been thrown up by a partial eruption burſting through the ſandſtone hills which ſurround Mount Ætna. The ſituation of this maſs is ſingular: it reſts upon a little hill, formed of looſe ſcoria, the ſummit and ſides of which are covered by the ſtony maſs, ſo that no crater is viſible. It ſtruck me on ſeeing it, and I found M. Dolomieu had formed the ſame opinion, that the lava had riſen up in a perpendicular direction, and had flowed over on all ſides. Its great thickneſs, and ſmall extent, ſeem to favour a conjecture which this naturaliſt has formed with regard to ſeveral lavas, that they were erupted at the bottom of an ocean which once covered Sicily, and, being quickly cooled by the contact of water, had been prevented from flowing far. The conjecture ſeems plauſible enough*; and, having no proof that this ſubſtance made part of an external current, as I have with reſpect to the firſt two mentioned, I do not exhibit it as a lava with the ſame confidence. Whatever be its hiſtory, however, it poſſeſſes the chemical properties common to whin and lavas.

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* "Elle eſt formée d'une pâte de roche de corne griſe, à grains fins, mêlée d'écailles, et de criſtaux de "feld-ſpath de même couleur; elle contient un très grand nombre de criſtaux de ſchorl noir, et de grains de "cryſolites jaunes, les uns et les autres quelquefois chatoyans, de différentes couleurs dans leurs fractures. " . . . . . : Cette lave a une caſſure ſeche, et un grain rude, ſurtout dans le centre des courans; c'eſt là "où elle a toujours conſervé une couleur plus claire, qui doit être celle de ſa baſe; ſur les bords et les ſurfaces elle s'eſt fort noircie; elle y a acquis une aſſez forte action ſur l'aiguille amantée que celle du centre "n'a preſque point."

† "Lave homogene noire: ſon grain eſt fin et ſerré, il eſt un peu brillant, comme micacé lorſqu'on le "préſente au ſoleil; ſa caſſure nette et ſeche eſt conchéide comme celle du ſilex."

‡ It belongs to the fifth variety of his compact lavas.

Its glaſs yielded a dark grey cryſtallite of uniform texture. Beſide it in the drawer, now on the table, I have placed a cryſtallite, formed from the whin No. 1. which reſembles it in every reſpect.

No. 4. *Lava of Iceland.*

I received the ſpecimen from a perſon who found it on the ſpot; but not being acquainted with the circumſtances of its original poſition, I cannot be certain that it is a lava. It has however every appearance of being ſuch.

It is a blue homogeneous ſubſtance, having ſome chryſolites ſcattered irregularly through it. Nearly half its bulk is occupied by large air holes, which do not appear to have contained any extraneous matter.

It produced a very fuſible glaſs, from which was formed a cryſtallite much more refractory than the original.

No. 5. *Lava of Torre del Greco.*

This lava, which flowed from Veſuvius to the ſea in the middle ages, has been an object of much attention, on account of its conſpicuous baſaltic form. It conſiſts of a grey baſis, the fracture of which is coarſe and rough, and in which are embedded large and well characterized cryſtals of ſchorl, (augit), with a few chryſolites, (olivins).

It was found to be leſs fuſible than any of the others, yet its glaſs cryſtallized in a lower temperature.

No. 6. *Lava of Veſuvius, eruption 1785.*

From the circumſtances in which the above five lavas have been ſeen to cryſtallize after fuſion, it can ſcarcely be doubted that the ſame proceſs takes place in a volcanic ſtream, which in conſequence of its bulk, muſt cool with conſiderable ſlowneſs, and that a vitreous character would be aſſumed by the whole maſs, were it cooled with ſufficient rapidity.

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* M. Dolomieu aſcribes the formation of part of Mount Ætna itſelf to a ſimilar cauſe. I ſhall have occaſion, in another part of this paper, to conſider that opinion.

The truth of this laſt opinion is demonſtrated by ſome facts which I accidentally obſerved, long before my preſent views had occurred, when, in ſpring 1785, I had an opportunity of examining a ſtream of lava, which flowed from Veſuvius. The eruption was comparatively ſo gentle, that I was able, though not without inconvenience, to approach and examine the fiery ſtream on three different days. It was in general concealed by a thin white ſmoke, which the wind blew aſide occaſionally, ſo that I could diſtinctly ſee the lava as it burſt from the ſide of the hill. It was then of a bright white heat, and flowed with the agility and rapidity of water, in all reſpects reſembling melted iron running from the furnace. The liquid, at its firſt emergence, manifeſted a ſtrong efferveſcence, which ſubſiding as the heat abated, ſhewed itſelf at laſt only in the burſting of ſome very large bubbles, accompanied with a white ſmoke. Where I approached the ſtream, it was ſtill of a ſtrong red heat, and had the conſiſtence of honey. I thruſt a ſtick into it with eaſe, to the end of which ſome of the lava adhering, by its viſcidity, allowed itſelf to be drawn out into threads, and was found, when cold, to have a ſhining ſurface, and a vitreous fracture.

Being thus convinced that I had met with a lava of glaſs, I prepared ſome moulds of ſtucco, in which I meant to take caſts with that rare ſubſtance; and with this view returned to the mountain. I found the ſtream was not ſo liquid as at firſt, but I was able, by means of a ladle fixed on the end of a pole, to lift the ſpecimen now before us in a ſtate like dough. I then preſſed it with a ſeal, by which means, though too coarſe to receive an accurate impreſſion, it took the ſhape it now bears, which is that of the ladle. It is very porous, one-third of it nearly being occupied by air holes. It contains a great number of ſmall white cryſtals of Veſuvian garnet, embedded in a black ſubſtance, which completely reſembles the glaſs obtained in our experiments from lava by rapid cooling after fuſion. Beſides all their other properties, it poſſeſſes the fuſibility of the glaſſes, ſince it ſoftens completely at 18, that is, 14 or 15 degrees below the ſoftening point of any of the ſtony lavas. Being expoſed to the proceſs of regulated cooling, it gave the ſame reſult as all the other lava glaſſes. In the lower points it yielded a liver cryſtallite infuſible under 30, and in the higher a ſtony ſubſtance like a common lava or whin, and fuſible only at 35.

What has been ſaid is applicable to the interior parts of lavas; but I was at a loſs to underſtand the ſtate of their external ſurface, which, cooling much more rapidly, might be expected to poſſeſs a vitreous character; yet glaſs is not found on the ſurface of lavas, except in a very few caſes, and has occurred only in a ſingle ſpot on Ætna. This difficulty was removed, however, by the following conſideration: though the ſurface of a lava cools with far more rapidity than the reſt of the maſs, yet, owing to the contact of the fiery ſtream, that rapidity can never be very great; and we muſt ſuppoſe that the temperature of the ſurface employs more than a minute or two in deſcending from 23 to 21. Where this happens, we have ſhown that the ſubſtance conſolidates into the liver cryſtallite, which completely reſembles the ſcoria of a lava. A ſmall fragment of the maſs, which I took from the running ſtream, being placed in the temperature of 22, loſt its vitreous character in two minutes, as already ſtated; and had the maſs itſelf been allowed to remain but a very little longer in the ſtream, it would certainly have acquired, as well as the reſt of the ſurface, the dull character of ſcoria.

The ſame property accounts for the cruſt which is formed on the ſurface of flowing lavas, and which conſtitutes ſo remarkable a feature in their hiſtory. Were lava to congeal after the manner of pitch or wax, by an uniform and gradual increaſe of viſcidity throughout, no cruſt would be formed, or if, by the action of cold air, the upper ſurface were to harden a little, it might he ſoftened again by an influx of freſh matter a very little hotter than itſelf. In lavas, however, as we have proved, when the ſurface cools down to 21, it rapidly congeals to a hard ſubſtance, capable of reſiſting any heat under 30. The cruſt thus formed ſerves as a pipe, within which the flowing lava is confined. In ſeveral places on Ætna we meet with vaſt galleries, along which, and out of which, the lava has flowed, leaving the cruſt entire *.

The irregular manner in which a lava flows, when not extremely heated, may likewiſe be referred to the ſame cauſe. On the lower part of the running ſtream a cruſt is formed, ſo ſtrong as to retard its progreſs during a certain time, but the liquid behind, accumulating by degrees, at laſt acquires ſufficient ſtrength to force open the cruſt; the lava then flows out with rapidity, and continues its courſe till it is again retarded by the formation of a new cruſt.

Theſe experiments ſeem to eſtabliſh, in a direct manner, what I had deduced, analogically, from the properties of whinſtone, namely, that the ſtony character of a lava is fully accoumted for by ſlow cooling after the moſt perfect fuſion; and, conſequently, that no argument againſt the intenſity of volcanic fire can be founded upon that character. We are therefore juſtified in believing, as numberleſs facts indicate, that volcanic heat has often been of exceſſive intenſity.

In the compariſon inſtituted between whin and lava, the two claſſes are found to agree ſo exactly in all their properties which we have examined, as to lead to a belief of their abſolute idenity. This idenity has been fully eſtabliſhed by Dr. Kennedy, who has performed an exact analyſis † of ſeveral of the very ſpecimens of whinſtone and lava mentioned in this paper; by which he diſcovered, that the elements of the two claſſes are the ſame: above all, that they both contain 4 or 5 *per cent.* of ſoda. Their agreement in this eſſential circumſtance ſeems to account for their common properties, whilſt the varieties of proportion, among their component elements correſpond to the ſlight differences of reſult we have obſerved between the individuals of the ſame claſs ‡.

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* As at Malpertui above Piedimonte.

† An account of Dr. Kennedy's analyſis is publiſhed in this volume. (Edin. Tranſ.)

‡ Though chemiſts have hitherto overlooked, in their experiments, the mode in which bodies were cooled after being reduced to a ſtate of fuſion; yet many reſults, which we are now entitled to aſcribe to ſlow cooling, have been occaſionally obſerved. The ſlag of a furnace bears a ſtrong reſemblance to what we have

So cloſe a reſemblance affords a very ſtrong preſumption in favour of Dr. Hutton's ſyſtem, according to which both claſſes are ſuppoſed to have flowed by the action of heat; but the circumſtances under which they were expoſed to this action being materially different, we have reaſon to look for indications of that difference. Such are not wanting.

Calcareous ſpar frequently occurs in whinſtone, either in veins or in detached nodules, but is never found in lava, and could not exiſt in a volcanic ſtream; for heat, in ſuch circumſtances, would infallibly drive off the carbonic acid, and compel the lime to unite with the other component elements of the maſs. In whinſtone, which Dr. Hutton ſuppoſes to have flowed, at ſome remote period, in crevices of the earth, at a great depth below what was then its ſurface, the weight and ſtrength of the ſuperincumbent maſs of ſtrata* has been ſufficient to reſiſt the expanſion of the carbonic acid, and to conſtrain it, upon the principle of Papins digeſter, to continue in combination with the lime. This compound

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have called the liver cryſtallite, and is probably formed in the ſame manner. I have ſeen a maſs poſſeſſing, in a great meaſure, the ſtony character of whins and lavas, which was produced in a lime-kiln by the fuſion of an impure limeſtone; and Dr. Beddoes has obſerved a cryſtallized texture in the flags of ſome iron furnaces. I am informed, that the celebrated Mr. Klaproth has deſcribed ſome ſtriking examples of cryſtallization after fuſion, which he obtained in expoſing various ſubſtances to the heat of the porcelain furnace at Berlin.

* It may be aſked, what has become of this ſuperincumbent maſs; and by what means it has been removed. Dr. Hutton anſwers, that it has been gradually worn away during an immenſe courſe of ages, by the action of thoſe cauſes which continue, under our eyes, to corrode the ſurface of the globe: that the ſolid parts, being conveyed to the bottom of the ocean, are there depoſited in beds of ſand and gravel, which, in ſome future revolution, being expoſed to heat, may be again converted into ſtony ſtrata.

The whole of this ſyſtem appears to me well founded, except in what regards the removal of the ſuperincumbent maſs, which has been performed, I conceive, in a very different manner. I am inclined to agree on this point with M. Pallas, M. de Sauſſure, and M. Dolomieu, and to believe that, at ſome period very remote with reſpect to our hiſtories, though ſubſequent to the induration of the mineral kingdom, the ſurface of the globe has been ſwept by vaſt torrents, flowing with great rapidity, and ſo deep as to overtop the mountains; that theſe torrents, by removing and undermining the ſtrata in ſome places, and by forming in others immenſe depoſits, have produced the broken and motley ſtructure, which the looſe and external part of our globe every where exhibits.

In the Alps and in Sicily I have witneſſed ſeveral of thoſe curious facts, upon which M. de Sauſſure and M. Dolomieu found their opinion, and which ſeem to juſtify their concluſions. I have likewiſe obſerved, in this country, many phenomena which denote the influence of ſimilar agents. Lord Daar, who joins me in agreeing with Dr. Hutton in almoſt every article but this, has added great weight to the argument by ſome general obſervations on lakes, and by ſome very intereſting facts which he has obſerved in the Highlands of Scotland. We propoſe to purſue this ſubject, and to lay the reſult of our inquiries before the ſociety. Dr. Hutton, in the ſecond volume of his *Theory of the Earth*, has taken great pains to refute all that has been ſaid about theſe torrents; but, in my opinion, their exiſtence is not only quite conſiſtent with his general views, but ſeems deducible from his ſuppoſitions, almoſt as a neceſſary conſequence. When the ſtrata, according to his ſyſtem, were elevated from the bottom of the ſea, the removal of ſo much water, if not performed with unaccountable ſlowneſs, muſt have produced torrents, in all directions, of exceſſive magnitude, and fully adequate to the effects I have thus aſcribed to them.

ſeems to have entered readily into fuſion, along with the whinſtone, but to have kept ſeparate from it, as oil ſeparates from water through which it has been diffuſed, thus giving riſe to the ſpherical form, which the nodules of calcareous ſpar generally exhibit with more or leſs regularity *.

This circumſtance accounts for an appearance which has miſled ſome of the early obſervers of our minerals. Many whinſtone rocks externally reſemble very porous lavas, but when broken are always found to be quite compact internally, and to contain numerous round nodules of calcareous ſpar. Near the ſurface, the nodules, being waſhed out by rain, have left the cavities which have given riſe to this deception. The ſpherical form of the air holes in lavas, and of the nodules of calcareous ſpar in whins, ſeems to have been produced by a cauſe common to both, the mutual repulſion of two fluids intermixed, but not diſpoſed to unite.

It muſt be owned, that this theory of calcareous ſpar is as yet hypothetical; but it is ſupported by ſtrong analogy, and promiſes to be of ſervice, by leading to deciſive experiments and obſervations. I cannot help believing, that, by a careful examination of the volcanic countries, facts may yet be diſcovered which will throw light on this ſubject. In order to promote and direct ſuch reſearches, I ſhall beg leave to ſtate ſome obſervations which I made in thoſe countries in 1785, before I was attached to any ſyſtem of geology.

It is generally ſuppoſed, that ſome lavas of Ætna contain calcareous ſpar and zeolite; but this I conceive to be a miſtake. It is true, as I have ſeen, that many rocks of Ætna contain theſe ſubſtances in abundance; but in my opinion theſe rocks are no lavas, but have flowed ſubterraneouſly like our whins, and are the ſame with them in every reſpect. A particular diſtrict of Ætna, comprehending the Cyclopian Iſles, and the country round La Trezza, and the caſtle of Jaci, is decidedly of this deſcription; and veſtiges of the ſame kind occur in other parts of the mountain. In one place foſſile coal has been found, and in another we ſaw marine ſhells. In the neighbourhood of Bronte we obſerved a high ridge formed of ſtrata of ſandſtone and limeſtone, partly overflowed and concealed by recent lavas, but ſo placed as to render it evident that its continuation formed no inconſiderable part of the mountain. Thus, Ætna being compoſed, partly of the ſubterranean, and partly of the external productions of fire, may be expected to afford numberleſs opportunities of purſuing the compariſon between theſe two claſſes †.

A moſt intereſting ſcene for ſuch a compariſon occurs likewiſe on Veſuvius. The hiſtory of this volcano is ſimpler than that of Ætna, for it has been evidently formed, with all its appendages, by the continued action of external eruptions, which have raiſed it, at ſome remote period, from the bottom of a ſea, occupying all the Campi Phlegræi, and waſhing the ſurrounding Appenines. The whole volcano ſeems once to have conſiſted of

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* The modifications of the action of heat, occaſioned by preſſure, which have been taken into account by no geologiſt but Dr. Hutton, diſtinguiſh his theory from all other igneous theories.

† M. Dolomieu has obſerved this diſtinction; but ſuppoſes that the maſſes which we conceive to have flowed ſubterraneouſly were erupted at the bottom of the ſea.

a ſingle large cone, the greateſt part of which has ſunk during ſome violent eruption, probably that which took place in the time of Pliny, leaving a fragment of its baſis, now called the mountain of Somma. This fragment retains its original ſhape; and on the ſide fronting the towns of Somma and Otajano, the external conical ſurface, along which the ancient lavas had flowed, is ſtill entire. Fronting the centre of the cone, Somma breaks off abruptly, and preſents a vertical cragg, ſome hundred feet in height, which is concave inwards. From the gulf, produced by the ruin of the ancient mountain, though not exactly from its centre, have ariſen the exploſions which, by repeated accumulation, have formed the preſent cone of Veſuvius. Next the ſea, this cone has extended itſelf ſo as completely to cover all remains of the ancient one, forming a continued ſlope from the crater to the foot of the mountain. On the oppoſite ſide it meets the baſe of the craggs of Somma, and forms an angle, into which many ſucceſſive ſtreams of lava have flowed, producing a narrow horizontal valley, in the form of a creſcent, called the *Atrio del Cavallo*. From this valley the craggs of Somma preſent a complete view of the internal ſtructure of the ancient mountain, correſponding, in moſt things, to what might have been ſuppoſed.

The various ſubſtances, depoſited ſucceſſively on the external ſurface of the ancient cone, being cut vertically in this cragg, their ſucceſſion is diſtinctly ſeen, the ſection of each ſtratum preſenting to the view part of a horizontal circle; the whole conſiſts of alternate layers of thin ſtreams of lava, and very thick beds of looſe frothy *rapilli*, which laſt being thrown into the air in a ſoft ſtate, had fallen in ſhowers on the ſides of the mountain.

In various places the regularity of this arrangement is interrupted by certain vertical lavas, from two feet to ten or twelve in thickneſs, which croſs the ſtrata juſt deſcribed in an irregular manner, and paſs upward, without diſtinction, through the ſolid beds, and through the looſe ones. It immediately occurred to us[*], that theſe lavas muſt have flowed in fiſſures of the ancient mountain; and we accounted for them by ſuppoſing, that a melted ſtream, flowing along the external ſurface, had met in its courſe with one of thoſe crevices which are formed in all great eruptions, and had flowed into it ſo as to return again into the heart of the mountain. This conjecture very nearly agrees with thoſe advanced by M. Dolomieu, and by M. Breiſlack, who both mention theſe vertical lavas of Somma[†].

I have ſince been induced to conſider this phenomenon, which formerly ſeemed to preſent only an amuſing variety in the hiſtory of volcanic eruptions, as of the utmoſt conſequence in geology, by ſupplying an intermediate link between the external and the ſubterraneous productions of heat. I now think, that, though we judged rightly in believing thoſe lavas to have flowed in crevices, we were miſtaken as to their direction; for inſtead of flowing downwards, I am convinced they have flowed upwards, and that the crevices have performed the office of pipes, through which lateral exploſions have found a vent. This will appear in the higheſt degree probable, when we attend to the known hiſtory of volcanic eruptions. It generally happens, that the lava begins to flow from the ſummit, in conſequence of the crater being filled with liquid matter up to the brim. At that moment the baſis of the mountain muſt be preſſed outwards by a very great hydroſtatical force, equal to the weight of a column of liquid lava as high as the mountain itſelf. It is natural then to expect, that this preſſure, aſſiſted by ſtrong percuſſions of exploſion, ſhould lacerate the body of the hill, and form great rents. The lava, urged upwards by the ſame preſſure, would flow through theſe rents, and emerge at the ſurface with violence. The diſcharge would continue through this channel till the propelling force had ceaſed, when the rents would be left full of lava; which, cooling in that poſition, would produce vertical lavas, ſuch as thoſe of Somma. This ſuppoſition is confirmed by various phenomena: the lava ceaſes to flow from the crater as ſoon as a lateral eruption has begun; when it ruſhes with ſuch violence from the ſide of the mountain as to fly to a great height into the air, like a *jet d'eau;* and it often makes its appearance, in the ſame inſtant, at various mouths, which are not ſcattered at random, but placed in one continued line, indicating the diſcharge from a rent. Some circumſtances likewiſe, which I obſerved on a cloſe examination of the vertical lavas, indicate that the crevices had performed the office of pipes. Frequently the ſubſtance at the middle differs from that on each ſide, whilſt the ſides reſemble each other exactly. I explain this, by ſuppoſing that the lava, which had firſt flowed through the pipe, and had coated its ſides with ſolid matter, had been followed by a ſtream ſomewhat different, which had remained there on the cooling of the whole. In one caſe, I found lava on each ſide, and in the middle tuffa, which is generally ſuppoſed to have been erupted in the ſtate of watery mud. In another, the ſubſtance in immediate contact with the mountain is vitreous, the reſt being common lava. This is fully explained by our experiments, if we ſuppoſe the ſtream to have flowed into a cold crevice.

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[*] I ſaw this place in company with Dr. J. Home in 1785.

[†] M. Dolomieu conceives theſe lavas to have flowed over the lips of the crater, (*Iſles Ponces, p.* 100.); M. Breiſlack, that they had firſt filled the open cavity of the crater, and from thence had flowed into crevices formed in its ſides, "che una lava avendo riempita la cavita del cratere ſi foſſe inſinuata per queſte "fenditure," (*Topografia Fiſica della Campania, p.* 115.) This laſt mentioned work, publiſhed in 1798, contains many intereſting and accurate deſcriptions. Should the circumſtances of the times permit, the author will have it in his power to follow out, with every advantage, the hints I have ſuggeſted.

To apply theſe obſervations to the general hiſtory of the globe: it is evident that the vertical lavas bear the cloſeſt reſemblance, in point of poſition, to veins of every deſcription, which, in all parts of the world, are found penetrating the ſtrata, and which, according to Dr. Hutton, have flowed by means of ſubterraneous heat. The veins, or dikes, (as they are called), of whinſtone, which ſo commonly occur in this country, differ from them in no circumſtance which I had the means of obſerving. It is therefore natural to expect that, if examined with particular care, their agreement will be found complete. Of this, however, we muſt not form too ſanguine expectations; for though the vertical lavas of Somma have undoubtedly ſuſtained the preſſure of a great ſuperincumbent maſs, we have no proof that this force was ſufficiently ſtrong, as Dr. Hutton ſuppoſes was the caſe in whinſtone, to repreſs the volatility of carbonic acid. On the other hand, as we are yet entirely ignorant of the degree of force requiſite for this purpoſe, we have no proof that it has been too weak. All the veins of Somma I examined were abſolutely compact, except one which was full of pores. I am unable to determine whether this was the real poroſity of a lava, or whether, as in our whins, it aroſe from the removal, near the ſurface, of nodules of calcareous ſpar. Granting that theſe pores were real air holes, the circumſtance was peculiar to that ſingle ſtream, and may have been owing to an inferior degree of preſſure; for, in this reſpect, we have reaſon to look for the greateſt diverſity. Some vertical ſtreams muſt have flowed while the mountain was yet low; others may have found vent at a low level; in both which caſes, the preſſure would be feeble: whereas other ſtreams, communicating with elevated lateral eruptions, would ſuſtain, in their lower parts, the full re-action of deep columns of liquid lava, and may be expected to exhibit the effects of great preſſure. Should any future traveller be fortunate enough to meet with a nodule of calcareous ſpar in a lava, occupying the crevice of a mountain formed by undoubted external eruptions, all that has been ſaid of the effects of preſſure would ceaſe to be hypothetical, and this fundamental article of Dr. Hutton's theory would be eſtabliſhed beyond diſpute.

I have now examined the relation between whinſtone and lava in various points of view; and the reſult of the inveſtigation, by ſhowing the intimate connection between the two claſſes, tends ſtrongly to confirm the ideas of Dr. Hutton. I flatter myſelf, likewiſe, that the experiments, independently of the general views of geology, are of ſome value, by accounting for the ſtony character of lavas, and thus enabling us to diſpenſe with the various myſtical ſuppoſitions which have of late perplexed the hiſtory of volcanic phenomena.

TABLE OF FUSIBILITIES.

	Subſtances.	*Original ſoftened.*	*Glaſs ſoftened.*	*Cryſtallite ſoftened.*
No. 1.	Whin of Bell's Mills Quarry - - - - -	40	15	32
2.	Whin of Caſtle Rock - - - -	45	22	35
3.	Whin of Baſaltic Column, Arthur's Seat - - -	55	18	35
4.	Whin near Duddingſtone Loch - - -	43	24	38
5.	Whin of Saliſbury Craig - - - - - -	55	24	38
6.	Whin from the Water of Leith - - -	55	16	37
7.	Whin of Staffa - - - - - - - -	38	14½	35
No. 1.	Lava of Catania - - - - -	33	18	38
2.	Lava of S^ta^ Venere, Piedimonte - - - - -	32	18	36
3.	Lava of La Motta - - - - -	36	18	36
4.	Lava of Iceland - - - - - - -	35	15	43
5.	Lava of Torre del Greco - - - -	40	18	28
6.	Lava of Veſuvius, 1785 - - - -	18	18	35

III.

A Memoir on the Diſcovery of certain Bones of a Quadruped of the Clawed Kind in the Weſtern Parts of Virginia. By THOMAS JEFFERSON, *Eſq.*

(Concluded from page 43.*)*

THESE bones only enable us to claſs the animal with the unquiculated quadrupeds; and of theſe the lion being neareſt to him in ſize, we will compare him with that animal, of whoſe anatomy Monſieur Daubenton has furniſhed very accurate meaſures in his tables at the end of Buffon's Natural Hiſtory of the Lion. Theſe meaſures were taken, as he* informs us, from "a large lion of Africa," in which quarter the largeſt † are ſaid to be produced. I ſhall ſelect from his meaſures only thoſe where we have the correſponding bones, converting them into our own inch and its fractions, that the compariſon may be more obvious: and to avoid the embarraſſment of deſignating our animal always by circumlocution and deſcription, I will venture to refer to him by the name of the Great-Claw or Megalonyx, to which he ſeems ſufficiently entitled by the diſtinguiſhed ſize of that member.

	Megalonyx Inches.	Lion. Inches.
Length of the ulna, or fore-arm - - -	20.1	13.7
Height of the olecranum - - - -	3.5	1.85
Breadth of the ulna, from the point of the coronoide apophyſis to the extremity of the olecranum - - -	9.55	
Breadth of the ulna at its middle - - -	3.8	
Thickneſs at the ſame place - - - -	1.14	
Circumference at the ſame place - - -	6.7	
Length of the radius - - - - -	17.75	12.37
Breadth of the radius at its head - - -	2.65	1.38
Circumference at its middle - - -	7.4	3.62
Breadth at its lower extremity - - - -	4.05	1.18
Diameter of the lower extremity of the femur at the baſe of the two condyles - - - -	4.2	2.65
Tranſverſe diameter of the larger condyle at its baſe -	3	
Circumference of both condyles at their baſe - -	11.65	
Diameter of the middle of the femur - - -	4.25	1.15
Hollow of the femur at the ſame place - -	1.25	
Thickneſs of the bone ſurrounding the hollow - -	1.5	
Length of the longeſt claw - - - -	7.5	1.41
Length of the ſecond phalanx of the ſame - -	3.2	1.41

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* Buffon, XVIII. 38, Paris edition in 31 vols. 12mo.

† 2. De Manet, 217.

The dimenſions of the largeſt of the foot bones are as follow:

	Inches.
Its greateſt diameter, or breadth at the joint	2.45
Its ſmalleſt diameter, or thickneſs at the ſame place	2.28
Its circumference at the ſame place	7.1
Its circumference at the middle	5.3

	Of longeſt toe.	Middle ſized toe.	Shorteſt toe.
2d. Phalanx. Its length	3.2	2.95	
Greateſt diameter at its head or upper joint	1.84	2.05	
Smalleſt diameter at the ſame place	1.4	1.54	
Circumference at the ſame place	5.25	5.8	
3d. Phalanx. Its length	[*]7.5	[†]5.9	3.5
Greateſt diameter at its head or upper joint	2.7	2.	1.45
Smalleſt diameter at the ſame place	.95	.9	.55
Circumference at the ſame place	6.45	4.8	

Were we to eſtimate the ſize of our animal by a compariſon with that of the lion on the principle of *ex pede Herculem*, by taking the longeſt claw of each as the module of their meaſure, it would give us as a being out of the limits of nature. It is fortunate therefore that we have ſome of the larger bones of the limbs which may furniſh a more certain eſtimate of his ſtature. Let us ſuppoſe then that his dimenſions of height, length and thickneſs, and of the principal members compoſing theſe, were of the ſame proportions with thoſe of the lion. In the table of M. Daubenton an ulna of 13.78 inches belonged to a lion 42½ inches high over the ſhoulders: then an ulna of 20.1 inches beſpeaks a megalonyx of 5 feet 1.75 inches height, and as animals who have the ſame proportions of height, length, and thickneſs have their bulk or weights proportioned to the cubes[‡] of any one of their dimenſions, the cube of 42.5 inches is to 262 lb. the height and weight of M. Daubenton's lion as the cube of 61.75 inches to 803 lb. the height and weight of the megalonyx; which would prove him a little more than three times the ſize of the lion, I ſuppoſe that we ſhould be ſafe in conſidering, on the authority of M. Daubenton, his lion as a large one. But let it paſs as one only of the ordinary ſize, and that the megalonyx, whoſe bones happen to have been found, was alſo of the ordinary ſize. It does [§] appear that there was diſſected for the academy of ſciences at Paris, a lion of 4 feet 9⅜ inches height. This individual would weigh 644 lb. and would be in his ſpecies, what a man of eight feet height would be in ours. Such men have exiſted. A megalonyx equally monſtrous would be 7 feet high, and would weigh 2000 lb. but the ordinary race, and not the monſters of it, are the object of our preſent enquiry.

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[*] It is actually 6¾ inches long, but about ¾ inch appear to have been broken off.

[†] Actually 5.65 but about ¼ inch is broken off.

[‡] Buffon xxii. 121.

[§] Buffon xviii. 15.

I have uſed the height alone of this animal to deduce his bulk, on the ſuppoſition that he might have been formed in the proportions of the lion. But theſe were not his proportions, he was much thicker than the lion in proportion to his height, in his limbs certainly, and probably therefore in his body. The diameter of his radius, at its upper end, is near twice as great as that of the lion, and, at its lower end, more than thrice as great, which gives a mean proportion of 2½ for 1. The femur of the lion was leſs than 1¼ inch diameter. That of the megalonyx is 4¼ inches, which is more than three for one. And as bodies of the ſame length and ſubſtance have their weights proportioned to the ſquares of their diameters, this exceſs of caliber compounded with the height, would greatly aggravate the bulk of this animal. But when our ſubject has already carried us beyond the limits of nature hitherto known, it is ſafeſt to ſtop at the moſt moderate concluſions, and not to follow appearances through all the conjectures they would furniſh, but leave theſe to be corroborated or corrected by future diſcoveries. Let us only ſay then, what we may ſafely ſay, that he was *more* than three times as large as the lion: that he ſtood as preeminently at the head of the column of clawed animals as the mammoth ſtood at that of the elephant, rhinoceros, and hippopotamus: and that he may have been as formidable an antagoniſt to the mammoth as the lion to the elephant.

A difficult queſtion now preſents itſelf. What is become of the great-claw? Some light may be thrown on this by aſking another queſtion. Do the wild animals of the firſt magnitude in any inſtance fix their dwellings in a thickly inhabited country? ſuch, I mean, as the elephant, the rhinoceros, the lion, the tyger? as far as my reading and recollection ſerve me, I think they do not: but I hazard the opinion doubtingly, becauſe it is not the reſult of full enquiry. Africa is chiefly inhabited along the margin of its ſeas and rivers. The interior deſart is the domain of the elephant, the rhinoceros, the lion, the tyger. Such individuals as have their haunts neareſt the inhabited frontier, enter it occaſionally, and commit depredations when preſſed by hunger: but the maſs of their nation (if I may uſe the term) never approach the habitation of man, nor are within reach of it. When our anceſtors arrived here, the Indian population, below the falls of the rivers, was about the twentieth part of what it now is. In this ſtate of things, an animal reſembling the lion ſeems to have been known even in the lower country. Moſt of the accounts given by the earlier adventurers to this part of America make a lion one of the animals of our foreſts. Sir John Hawkins * mentions this in 1564. Thomas Harriot, a man of learning, and of diſtinguiſhed candor, who reſided in Virginia in 1587 † does the ſame, ſo alſo does Bullock in his account of Virginia, ‡ written about 1627, he ſays he drew his information from Pierce, Willoughby, Claiborne, and others who had been here, and from his own father who had lived here twelve years. It does not appear whether the fact is ſtated on their own view, or on information from the Indians, probably the latter. The progreſs of the new population would ſoon drive off the larger animals, and the largeſt firſt. In the preſent interior of our continent there is ſurely ſpace and range enough for elephants and lions, if in that climate they could ſubſiſt; and for mammoths and megalonyxes who may ſubſiſt there. Our entire ignorance of the immenſe country to the weſt and north-weſt, and of its contents, does not authoriſe us to ſay what it does not contain.

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* Hakluyt, 541. edition of 1589.

† Ibid. 757, and Smith's Hiſtory of Virginia, 10.

‡ Bullock, page 5.

Moreover it is a fact well known, and always ſuſceptible of verification, that on a rock on the bank of the Kanhawa, near its confluence with the Ohio, there are carvings of many animals of that country, and among theſe one which has always been conſidered as a perfect figure of a lion. And theſe are ſo rudely done as to leave no room to ſuſpect a foreign hand. This could not have been of the ſmaller and maneleſs lion of Mexico and Peru, known alſo in Africa both in * ancient and † modern times, though denied by ‡ M. de Buffon: becauſe like the greater African lion, he is a tropical animal; and his want of a mane would not ſatisfy the figure. This figure then muſt have been taken from ſome other prototype, and that prototype muſt have reſembled the lion ſufficiently to ſatisfy the figure, and was probably the animal the deſcription of which by the Indians made Hawkins, Harriot, and others conclude there were lions here. May we not preſume that prototype to have been the great-claw?

Many traditions are in poſſeſſion of our upper inhabitants, which themſelves have heretofore conſidered as fables, but which have regained credit ſince the diſcovery of theſe bones. There has always been a ſtory current that the firſt company of adventurers who went to ſeek an eſtabliſhment in the county of Greenbriar, the night of their arrival were alarmed at their camp by the terrible roarings of ſome animal unknown to them: that he went round and round their camp, that at times they ſaw his eyes like two balls of fire, that their horſes were ſo agoniſed with fear that they couched down on the earth, and their dogs crept in among them, not daring to bark. Their fires, it was thought, protected them, and the next morning they abandoned the country. This was little more than 30 years ago.—In the year 1765, George Wilſon and John Davies, having gone to hunt on Cheat river, a branch of the Monongahela, heard one night, at a diſtance from their camp, a tremendous roaring, which became louder and louder as it approached, till they thought it reſembled thunder, and even made the earth tremble under them. The animal prowled round their camp a conſiderable time, during which their dogs, though on all other occaſions fierce, crept to their feet, could not be excited from their camp, nor even encouraged to bark. About day-light they heard the ſame ſound repeated from the knob of a mountain about a mile off, and within a minute it was anſwered by a ſimilar voice from a neighbouring knob. Colonel John Stewart had this account from Wilſon in the year 1769, who was afterwards Lieutenant Colonel of a Pennſylvania regiment in the revolution-war; and ſome years after from Davies, who is now living in Kentucky.

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* Ariſtot. Animal, 9. 4. Pliny, 8. 16.

† Kolbe.

‡ Buffon, xviii. 18.

Theſe circumſtances multiply the points of reſemblance between this animal and the lion. M. de la Harpe of the French Academy, in his abridgment of the General Hiſtory of Voyages, ſpeaking of the Moors, ſays*, "it is remarkable that when, during their huntings, they meet with lions, their horſes, though famous for ſwiftneſs, are ſeized with ſuch terror that they become motionleſs, and their dogs equally frightened, creep to the feet of their maſter, or of his horſe." Mr. Sparrman in his voyage to the Cape of Good Hope, chap. 11. ſays, "we could plainly diſcover by our animals when the lions, whether they roared or not, were obſerving us at a ſmall diſtance. For in that caſe the hounds did not venture to bark, but crept quite cloſe to the Hottentots; and our oxen and horſes ſighed deeply, frequently hanging back, and pulling ſlowly with all their might at the ſtrong ſtraps with which they were tied to the waggon. They alſo laid themſelves down on the ground, and ſtood up alternately, as if they did not know what to do with themſelves, and even as if they were in the agonies of death." He adds that "when the lion roars, he puts his mouth to the ground, ſo that the ſound is equally diffuſed to every quarter." M. de Buffon (xviii. 31.) deſcribes the roaring of the lion as, by its echoes reſembling thunder: and Sparrman c. 12. mentions that the eyes of the lion can be ſeen a conſiderable diſtance in the dark, and that the Hottentots watch for his eyes for their government. The phoſphoric appearance of the eye in the dark ſeems common to all animals of the cat kind.

The terror excited by theſe animals is not confined to brutes alone. A perſon of the name of Draper had gone in the year 1770, to hunt on the Kanhawa. He had turned his horſe looſe with a bell on, and had not yet gone out of hearing when his attention was recalled by the rapid ringing of the bell. Suſpecting that Indians might be attempting to take off his horſe, he immediately returned to him, but before he arrived he was half eaten up. His dog ſcenting the trace of a wild beaſt, he followed him on it, and ſoon came in ſight of an animal of ſuch enormous ſize, that though one of our moſt daring hunters and beſt markſmen, he withdrew inſtantly, and as ſilently as poſſible, checking and bringing off his dog. He could recollect no more of the animal than his terrific bulk, and that his general outlines were thoſe of the cat kind. He was familiar with our animal miſcalled the panther, with our wolves and wild beaſts generally, and would not have miſtaken nor ſhrunk from them.

In fine, the bones exiſt: therefore the animal has exiſted. The movements of nature are in a never ending circle. The animal ſpecies which has once been put into a train of motion, is ſtill probably moving in that train. For if one link in nature's chain might be loſt, another and another might be loſt, till this whole ſyſtem of things ſhould evaniſh by piece-meal; a concluſion not warranted by the local diſappearance of one or two ſpecies of animals, and oppoſed by the thouſands and thouſands of inſtances of the renovating power conſtantly exerciſed by nature for the reproduction of all her ſubjects, animal, vegetable, and mineral. If this animal then has once exiſted, it is probable on this general view of the movements on nature that he ſtill exiſts, and rendered ſtill more probable by the relations of honeſt men applicable to him and to him alone. It would indeed be but conformable to the ordinary economy of nature to conjecture that ſhe had oppoſed ſufficient barriers to the too great multiplication of ſo powerful a deſtroyer. If lions and tygers multiplied as rabbits do, or eagles as pigeons, all other animal nature would have been long ago deſtroyed, and themſelves would have ultimately extinguiſhed after eating out their paſture. It is probable then that the great-claw has at all times been the rareſt of animals. Hence ſo little is known, and ſo little remains of him. His exiſtence however being at length diſcovered, enquiry will be excited, and further information of him will probably be obtained.

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* Gentleman's, and London Magazines, for 1783.

The Coſmogony of M. de Buffon ſuppoſes that the earth, and all the other planets primary and ſecondary, have been maſſes of melted matter ſtruck off from the ſun by the incidence of a comet on it: that theſe have been cooling by degrees, firſt at the poles, and afterwards more and more towards their equators: conſequently that on our earth there has been a time when the temperature of the poles ſuited the conſtitution of the elephant, the rhinoceros, and hippopotamus; and in proportion as the remoter zones became ſucceſſively too cold, theſe animals have retired more and more towards the equatorial regions, till now that they are reduced to the torrid zone as the ultimate ſtage of their exiſtence. To ſupport this theory, he * aſſumes the tuſks of the mammoth to have been thoſe of an elephant, ſome of his teeth to have belonged to the hippopotamus, and his largeſt grinders to an animal much greater than either, and to have been depoſited on the Miſſouri, the Ohio, the Holſton, when thoſe latitudes were not yet too cold for the conſtitutions of theſe animals. Should the bones of our animal, which may hereafter be found, differ only in ſize from thoſe of the lion, they may on this hypotheſis be claimed for the lion, now alſo reduced to the torrid zone, and its vicinities, and may be conſidered as an additional proof of this ſyſtem; and that there has been a time when our latitudes ſuited the lion as well as the other animals of that temperament. This is not the place to diſcuſs theories of the earth, nor to queſtion the gratuitous allotment to different animals of teeth not differing in any circumſtance. But let us for a moment grant this with his former poſtulata, and aſk how they will conſiſt with another theory of his " qu'il y a dans la combinaiſon des elemens et des autres cauſes phyſiques, quelque choſe de contraire a l'aggrandiſement de la nature vivante *dans ce nouveau monde*; qu'il y a des obſtacles au developpement et peutetre a la formation des grands germes †." He ſays that the mammoth was an elephant, yet ‡ two or three times as large as the elephants of Aſia and Africa: that ſome of his teeth were thoſe of a hippopotamus, yet of a hippopotamus § four times as large as thoſe of Africa: that the mammoth himſelf, for he ſtill conſiders him as a diſtinct animal ‖, " was of a ſize ſuperior to that of the largeſt elephants. That he was the primary and greateſt of all terreſtrial animals." If the bones of the megalonyx be aſcribed to the lion, they muſt certainly have been of a lion of more than three times the volume of the African. I delivered to M. de Buffon the ſkeleton of our palmated elk, called orignal o r mooſe, feet high over the ſhoulders, he is often conſiderably higher. I cannot find that the European elk is more than two-thirds of that height: conſequently not one-third of the bulk of the American. He * acknowledges the palmated deer (daim) of America to be larger and ſtronger than that of the Old World. He † conſiders the round horned deer of theſe States and of Louiſiana as the roe, and admits they are of three times his ſize. Are we then from all this to draw a concluſion, the reverſe of that of M. de Buffon. That nature, has formed the larger animals of America, like its lakes, its rivers, and mountains, on a greater and prouder ſcale than in the other hemiſphere? Not at all, we are to conclude that ſhe has formed ſome things large and ſome things ſmall, on both ſides of the earth, for reaſons which ſhe has not enabled us to penetrate; and that we ought not to ſhut our eyes upon one half of her facts, and build ſyſtems on the other half.

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* Buffon, Epoq. 2. 233, 234.

† Buffon, xviii. 145.

‡ 2. Epoq. 223.

§ 1. Epoq. 246. 2. Epoq. 232.

‖ 2 Epoq. 234, 235.

To return to our great-claw; I depoſit his bones with the Philoſophical Society, as well in evidence of their exiſtence and of their dimenſions, as for their ſafe-keeping; and I ſhall think it my duty to do the ſame by ſuch others as I may be fortunate enough to obtain the recovery of hereafter.

TH: JEFFERSON.

Monticello. Feb. 10*th*, 1797.

P. S. *March* 10*th*, 1797. After the preceding communication was ready to be delivered in to the Society, in a ‡ periodical publication from London I met with an account and drawing of the ſkeleton of an animal dug up near the river La Plata in Paraguay, and now mounted in the cabinet of Natural Hiſtory of Madrid. The figure is not ſo done as to be relied on, and the account is only an abſtract from that of Cuvier and Roume. This ſkeleton is alſo of the clawed-kind, and having only four teeth on each ſide above and below, all grinders, is in this account claſſed in the family of unquiculated quadrupeds deſtitute of cutting teeth, and receives the new denomination of megatherium, having nothing of our animal but the leg and foot bones, we have few points for a compariſon between them. They reſemble in their ſtature, that being 12 feet 9 inches long, and 6 feet 4½ inches high, and ours by computation 5 feet 1.75 inches high: they are alike in the coloſſal thickneſs of the thigh and leg bones alſo. They reſemble too in having claws: but thoſe of the figure appear very ſmall, and the verbal deſcription does not ſatisfy us whether the claw-bone, or only its horny cover be large. They agree too in the circumſtance of the two bones of the fore-arm being diſtinct and moveable on each other; which however is believed to be ſo uſual as to form no mark of diſtinction. They differ in the following circumſtances, if our relations are to be truſted. The megatherium is not of the cat form, as are the lion, tyger, and panther, but is ſaid to have ſtriking relations in all parts of its body with the bradypus, daſypus, pangolin, &c. According to analogy then, it probably was not carnivorous, had not the phoſphoric eye, nor leonine roar. But to ſolve ſatisfactorily the queſtion of identity, the diſcovery of fore-teeth, or of a jaw bone ſhewing it had, or had not, ſuch teeth, muſt be waited for, and hoped with patience. It may be better, in the mean time, to keep up the difference of name.

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* Buffon, xxix. 245. † Ibid. xii. 91. 92. xxix. 245. Vide Suppl. 201.

‡ Monthly Magazine, Sep. 1796.

IV.

A Letter from Mr. John Heckewelder, *to Dr.* Barton, *giving ſome account of the remarkable Inſtinct of a Bird called the Nine-Killer*[*].

Bethlehem, December 18th, 1795.

HAVING an opportunity by a friend of mine to Philadelphia, I muſt mention to you a curious fact, that came to my knowledge but yeſterday.

I went to a farm, about eleven miles and a half from this place, to view a young orchard, which had been planted, about five weeks ago, under my direction, where on viewing the trees, I found, to my great aſtoniſhment, almoſt on every one of them, one and on ſome two and three graſshoppers, ſtuck down on the ſharp thorny branches, which were not pruned when the trees were planted. I immediately called the tenant, and aſked the reaſon and his opinion of this. He was much ſurpriſed at my ignorance about the matter, and informed me, "that theſe graſshoppers were ſtuck up by a ſmall bird of prey, which the Germans called *Neun-toedter* (in Engliſh, Nine-killer); that this bird had a practice of catching and ſticking up nine graſshoppers a day, and that as he well knew they did not devour the graſshoppers, nor any other inſects, he thought they muſt do it for pleaſure. I aſked him for a deſcription of this bird, and was perfectly ſatisfied that it lived entirely on ſmall animals, ſuch as ſmall birds, mice, &c. for I had paid attention to this bird as early as the year 1761, when, in the winter, one of the ſame ſpecies took a favourite little bird out of my cage at the window, from which time I have watched them more cloſely, and have found them more numerous in the weſtern-country than here. Not being ſatisfied with what the tenant had told me reſpecting the intention of the bird's doing all this (viz. for diverſion ſake), and particularly obſerving each and every one of theſe graſshoppers ſtuck up ſo regularly, and in their natural poſition as when on the ground, not one of them having its back downwards, I began to conjecture what might be the real intention which the bird had in this, and my determined opinion was, that this little bird-hawk, by inſtinct, made uſe of this art, in order to decoy the ſmaller birds, which feed on inſects, and by theſe means have a fair opportunity of catching them. All this I communicated to my friends, on my return home, and they were not leſs aſtoniſhed at what I had related to them, than I had been on diſcovering the fact. It being agreed that one or more gentlemen of learning and obſervation ſhould more minutely examine into this matter, the proprietor of this farm, with another gentleman and myſelf, went this day out for the purpoſe, and viewing the graſshoppers on a number of theſe ſmall trees (ſome of which we cut off, and took home), we returned to the tenant, who not only himſelf but alſo his father and ſiſter gave us the beſt aſſurances, that they had, long ſince, and from time to time, obſerved this bird catching graſshoppers and ſticking them up in the manner already related, and that ſometimes they had obſerved, in places where this ſpecies of bird keeps, numbers of graſshoppers ſtuck up on a thorn-buſh in like manner. The Reverend Mr. V. Vleck is perfectly ſatiſfied that this bird-hawk is the Lanius Canadenſis (in Bartram [*]), and has obligingly communicated the following account of this little bird-hawk to me: it is extracted from a German publication printed at Goettingen, in 1778, under the title of "Natural Hiſtory for Children, by M. George Chriſtian Paff," who after giving a deſcription of the different ſpecies of this bird, concludes thus: "Why is this bird of prey called the nine-killer? Becauſe it is ſaid to have the habit of ſticking beetles or other inſects, and perhaps ſometimes nine of them in ſucceſſion, upon thorns, that they may not eſcape until he has leiſure to devour them all at once. And for the ſame reaſon, it is ſometimes called the thorn-ſticker." Now by the above account, we ſee that it is known in Europe that this ſame ſpecies of birds actually does ſtick up inſects of different kinds on thorns, &c. but it is ſuppoſed they eat them immediately after being ſtuck up. Here the caſe is quite otherwiſe. They remain ſtuck up, for we muſt ſuppoſe theſe to have been ſtuck up at leaſt ſome weeks ago, and before the hard froſts ſet in. The very birds (as we ſuppoſe) that ſtuck them up are now on the ſame ground, watching the ſmaller birds that come out to feed, and have been ſeen catching the latter but a few days ago. If it were true, that this little hawk had ſtuck them up for himſelf; how long would he be feeding on one or two hundred graſshoppers? But if it be intended to ſeduce the ſmaller birds to feed on theſe inſects, in order to have an opportunity of catching them, that number, or even one half, or leſs, may be a good bait all winter: and all of us, who have conſidered theſe circumſtances, are firmly of opinion, that theſe inſects thus ſtuck up, are to ſerve as a bait, &c. through the courſe of the winter.

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[*] Addreſſed to Dr. Benj. Smith Barton, and inſerted in the Tranſactions of the American Philoſ. Society, IV. 124.

You will readily excuſe my being ſo lengthy on this ſubject. The matter appeared to me of too much conſequence to paſs over haſtily. I ſhall be glad to hear your opinion on this ſubject.

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[*] I do not find that Mr. Bartram has mentioned, in any part of his *Travels*, a Lanius Canadenſis. Since the date of this letter, Mr. Heckewelder has favoured me with a well-preſerved ſpecimen of the bird-hawk. It proves to be the Lanius Excubitor of Linnæus, the great-ſhrike of Mr. Pennant. B. S. B.

I ſend you a few of theſe graſshoppers, as I cut them from the trees. They being hard and dry, moſt of their legs broke off in taking them home.

I am, with great reſpect,
Dear Sir,
Your moſt obedient and
Humble Servant,
JOHN HECKEWELDER.

V.

Letter from Dr. Beddoes *on the Experiments made at the Royal Inſtitution with the Nitrous Oxide.*

To Mr. NICHOLSON.

SIR,

Clifton, 17th April.

THOUGH I muſt be too well aware of the cauſes of variation in medical and phyſiological teſtimony, to be much moved by groundleſs contradiction, yet I own I did expect that competent experimenters would nearly agree in their reports of the effect, produced by reſpiring the *nitrous oxide.*

A few days ago, however, I was well informed, that on trial at a reſpectable inſtitution in London, the gas had fallen far ſhort of what Mr. Davy and myſelf had taught the public to expect. The gentleman who brought this intelligence being at Clifton, it was eaſy to give him perſonal proof of our fidelity.

Yeſterday my friend Mr. William Reynolds, of Ketley, in Shropſhire, aſſured me that he had himſelf reſpired the gas at the *inſtitution* in London to no purpoſe; and that moſt others had done the ſame. The agency of the nitrous oxide is much too diſtinct and certain to leave a doubt, but that there muſt be ſome ſtrange miſtake here. On ſeeing a paralytic patient take it, Mr. W. R. was at once aware of the reaſon (at leaſt, of one ſufficient reaſon) for the failures in town, and on reſpiring a very ſmall quantity indeed, he felt effects exactly the ſame in kind, as the agreeable ones deſcribed in my *notice.* He had previouſly inhaled atmoſpheric air without knowing it to be ſuch. He expatiated with ſatisfaction on the ſenſations *now* excited by the nitrous oxide, and ſaid he ſhould like another doſe. On being aſked, if he had felt any ſuch deſire after the experiment in town, he anſwered, *no - not the leaſt.* We are conſtantly accuſtomed to the eager expreſſion of this deſire.

In our ample experience, we have not found the experiment to fail above once in twenty times. With a given degree of accuracy and perſeverance, it would, I apprehend, *never* fail. I do not mean that the reſult would be always agreeable.

The gas being procured genuine, the lungs ſhould be emptied by a ſtrong expiration, the noſe held tight, and the lips exactly cloſed on the mouth-piece. From 5 to 8 quarts breathed backwards and forwards into the reſervoir (a proceeding of which Mr. Davy's reſearches will ſhew the propriety) have proved an average doſe. The feelings will determine the time. To the hyſterical and the exquiſitely ſenſible, the gas ſhould not be adminiſtered.

It is very well to know from actual trial, what the London experiments prove, that interrupted reſpirations, during which the gas is probably much diluted with reſiduary air in the lungs, can ſeldom have effect. This, however, an obſerver of any ſagacity could have predicted.

A laborious analyſis, and the reſult of a vaſt variety of careful obſervations, will ere long appear, to correct the miſconceptions which may have ariſen from looſe caſual trials. Meanwhile, I beg you to inſert this explanation in your valuable Journal. I think it of no ſmall importance to mankind, that there ſhould exiſt nothing to prevent this great agent from being employed, whenever direct experience or circumſpect analogy warrants its uſe.

I am, Sir,

Reſpectfully your's,

THOMAS BEDDOES.

VI.

On the colouring Matter of Dog's Mercury (Mercurialis perennis Lin.) By a Correſpondent.

To Mr. NICHOLSON.

SIR,

RESEARCHES concerning the colouring principles of vegetable ſubſtances, have been very general and extenſive; but I have never read any thing concerning the colour which reſides in the juice of dog's mercury, and therefore conclude, that the properties of this vegetable are not very generally known.

The herb grows plentifully in woods and hedges. Its medical virtues are at preſent neglected; and it is never removed from its ſolitary abode for any uſeful purpoſe, but the inſtruction of the botaniſt. Accident may, perhaps, have diſcloſed this fact to him, that its juice affords a blue ſtain or colour: any one, however, may aſcertain its truth by the following eaſy experiment:

Take a ſtalk of the herb; rub the broken end upon a piece of fine white paper; and ſqueeze out ſome of the juice. No difference will be perceived at firſt, where the juice has touched the paper, but ſome hours afterwards it will be ſtained blue.

This colour is in the greateſt perfection, when the herb is young, and contains the moſt juice, the thinneſt and moſt colourleſs part of which produces the beſt blue: and it will be found by the experiment, that when the ſtalk is very forcibly ſqueezed, the paper is coloured rather green at firſt, and never becomes of a good colour. On the contrary, the place touched with the cleareſt and thinneſt part of the juice, diſcovers no colour for many hours, but at laſt exhibits the fineſt blue contained in the vegetable. This colour is not very durable, but after ſome time turns to a red, which will continue for years.

A friend of mine, Mr. R. P. to whom I communicated theſe properties of the juice, and requeſted him to deviſe ſome experiments, introduced ſome of the leaves and ſtalks of the herb, into a ſmall phial containing a ſolution of prepared ammonia, and in a little time the ſolution was coloured blue.

The herb, when dry, exibits no particular appearance; but the roots, which are long, ſlender, and white, when dried, diſcover in ſome places a fine blue tinge upon the outer ſkin; and if a quantity be ſteeped in water, it becomes of a blue colour, which afterwards turns red.

A ſolution of ammonia extracts the colour much ſooner than water alone, and, I think, improves it.

Some years ago, I diſcovered that the juice would ſtain a blue colour when laid upon a piece of white paper or fine white cloth; previous to that, however, I underſtood, that certain individuals had endeavoured to procure a body of colour from this vegetable; but of their ſucceſs, or the methods of conducting their proceſſes, I could learn nothing with certainty.

The properties of this vegetable appear ſingular to me, and, if they are not generally known, may perhaps deſerve the attention of your philoſophical readers, as the elucidation of the ſubject ſeems likely to be of ſome advantage to the arts.

It is proper to be obſerved, that as the plant is of a poiſonous nature, it muſt not be careleſsly uſed.

I am, Sir,

Your humble ſervant,

C.

April 19th, 1800.

Account

VII.

Account of a Work entitled "The Obſervations of NEWTON *concerning the Inflections of Light; accompanied by other Obſervations differing from his; and appearing to lead to a change of his Theory of Light and Colours."* *By a Correſpondent.*

THIS anonymous work conſiſts of the obſervations of Newton concerning the inflections of light contained in his third Book of Optics, reprinted, and accompanied by new facts and obſervations * made concerning the ſame phænomena, together with inferences, which differ altogether from thoſe of Newton.

In his firſt obſervation, Newton, repeating an experiment of Grimaldi, perceived as he had done, that in a beam of ſolar light paſſing through a ſmall hole $\frac{1}{42}$ of an inch wide, the breadth of the ſhadow of a hair obſerved at a diſtance from the hole, was conſiderably greater than that of the hair itſelf. This he conceived could only be effected by a *repulſive* power in the hair upon the *parallel* rays of the beam, turning the light aſide *from* the ſhadow, and rendering the ſhadow conſiderably *broader than it ought to be*, if the rays paſſed by in right lines. By actual meaſurement he further aſcertained, that the ſhadow was broader in proportion to the diſtance from the hair when nearer, than when farther from it; and this he imagined to depend upon the diminution of the repulſive power upon the more diſtant rays of light, in conſequence of which they were leſs turned aſide from, and therefore in their progreſs invaded and reduced the dimenſions of the ſhadow.

This of Newton is followed by an obſervation marked *α*, of the ſame appearances. In which the firſt thing noticed is, that the beam of light after paſſage through the ſmall hole, is no longer a beam of parallel rays, as Newton ſuppoſed, and has drawn it in his figure, but conſiſts of divergent rays. This is proved to be the caſe by actual meaſurement, due allowance being made for the penumbral light of the ſun, and is ſtated to appear more clearly by immediate inſpection of the various changes produced in the beam. This divergence is aſcribed to the well eſtabliſhed principle of an attractive force exerciſed by bodies upon light, in conſequence of which when the hole is very ſmall, its edges act upon the whole of the paſſing light, and render it altogether divergent.

In ſuch divergent light, therefore, the ſhadows of bodies ought to be of dimenſions conſiderably greater than the bodies themſelves, in proportion to the divergence of the rays, and to the diſtance of obſervation. This proportion, however, is not exactly preſerved, becauſe the rays in paſſing by the hair, undergo from the *attraction* of the hair a change of direction *towards* the ſhadow, in conſequence of which the ſhadow is really *leſs broad*, than it would have been if the rays had paſſed in right lines, and this proportion of the ſhadow is greater at ſmaller, than at larger diſtances. Theſe things are clearly ſhewn by a figure, and the principle of attraction is ſo well eſtabliſhed, as to leave no doubt of its affording the proper explanation of theſe appearances.

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* The obſervations of the author are deſignated by the letters *α*, *β*, *γ*, *δ*, &c. of the Greek alphabet.---N.

In oppoſition to the authority of Newton, he is quoted againſt himſelf. Five paſſages are produced from his works, which prove, that on different occaſions he reaſoned inconſiſtently and contradictorily concerning theſe phenomena.

This obſervation is concluded with the curious remark, that ſtrictly ſpeaking, all this reaſoning has been applied, to what in fact is not a ſhadow, formed by the interception of light by the body, as appears in a ſubſequent obſervation γ, but a dark interval of the nature of thoſe obſerved between the fringes of colours afterwards mentioned. So little have theſe phenomena been underſtood!

The ſecond and third obſervations of Newton next follow, and are ſucceeded by the author's obſervation β, in which, inſtead of only three noticed by Newton at the termination of the ſhadow of the hair, an uninterrupted ſucceſſion of fringes are deſcribed, the colours of which are ſtated to vary ſomewhat from thoſe, the fringes themſelves admitting of various changes, and this new arrangement being made with a view to analogies, that are ſtated to exiſt between theſe and other ſimilar appearances, and in order that they may generally accord therewith, and with other obſervations of Newton made elſewhere.

Newton's fourth obſervation deſcribes the ſucceſſive appearance of the three fringes, with their dark intervals at different diſtances from the hair, and concludes from meaſures taken, that the ſquares of the breadths of the fringes, and their intervals together, are in the continual progreſſion $1, \frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \frac{1}{5}$ or thereabouts. Obſervation γ, ſtates, that at a very ſmall diſtance from the hair, the ſhadow appears diſtinct, well-defined, and intenſely black; that at a greater diſtance it is divided through its whole length by a line of light, and changed to a double ſhadow, reſembling the ſhadows of two hairs, but not ſo intenſely black as before; and that at yet greater diſtances the ſhadow increaſes in breadth, and diminiſhes in blackneſs, whilſt the line of light putting on colours, and becoming more and more dilute, ſpreads over and diſappears within the ſhadow, which again loſes its double appearance. The firſt only is a proper ſhadow, which is quickly invaded and deſtroyed by the inflected lights of the two neareſt fringes on both ſides of the hair, which meet together at that diſtance therefrom, and the apparent double ſhadow conſiſts of the two ſucceeding dark intervals, which become ſingle as the intermediate light becomes dilute, and ceaſes to appear. This is what has been called a ſhadow by Grimaldi and Newton, and has been conſidered and meaſured as ſuch.

The fringes and their intervals, as they are formed under various circumſtances, and their various dimenſions are then ſhortly noticed, and it is affirmed, contrary to the obſervation of Newton, that the breadths of the fringes and intervals, ſo far from obſerving generally the proportions aſſigned by him, do vary indefinitely through almoſt all degrees of their poſſible extenſion.

According to the fifth obſervation of Newton, a beam of light paſſing through a hole $\frac{1}{4}$ of an inch wide by the edge of a knife, on the blade of which part of the light fell, exhibited *two* ſtreams of faint light ſhooting out both ways from the beam into the ſhadow like the tails of comets, at an angle of about ten or twelve degrees, and ſometimes farther; at the ſame time that a line of light appeared at the edge of the knife, viſible even out of the direction of the general light from parts beyond either the point or handle of the knife. This light, Newton obſerves, was *contiguous* to the edge, and narrower than the light of the *innermoſt* fringe, and paſſed *between* it and the knife. The author, in his obſervation δ, remarks, that the preceding circumſtances conſtitute only one of the many caſes of the inflexions of light in which no fringes are formed, but only a general derivation and divergence of the rays is produced, which is owing to the paralleliſm of the rays of light not being changed by their paſſage through the large hole, but wherever the diverging of a beam of light is ſuch that the inflexions of a body can give them the due arrangement, fringes are formed of endleſs varieties as theſe circumſtances vary. Three caſes of fringes, making, with the former, four general caſes of inflexions, are then ſtated as including theſe varieties of appearance as the diſperſion of the beam from its greateſt, is diminiſhed to its ſmalleſt ſtate of divergency and to paralleliſm, and it is explained and ſhown by figures how the formation of fringes depends upon the divergency, and is deſtroyed by the paralleliſm of a beam. The Newtonian experiment is then repeated with a beam of light paſſed through a hole quarter of an inch wide, in a plate of lead, the *two* ſtreams of light obſerved by him concerning the origin and exiſtence of which much doubt and difficulty had ariſen among philoſophers, are ſhown to be owing the one to the edge of the lead of the hole, the other to the edge of the knife, and to be produced in the parallel beam of light. The line of light obſerved by Newton is noticed as indeed worthy of particular attention, being in a very remarkable manner derived out of the whole body of the light paſſing by the edge, and not merely from that *contiguous* to it, and not being narrower than the *inmoſt* fringe, or paſſing *within* it, becauſe in this caſe no fringes are or can be formed, paſſing, however, at thoſe very points near the body at which, under other circumſtances, the *many* neareſt fringes are formed.

According to obſervations the ſixth and ſeventh, by approaching the parallel edge of another knife to that of the former, at the diſtance of the 400th part of an inch, juſt before they touch, the whole paſſing light is divided into two ſtreams, each bent towards the neareſt edge with a dark ſhadow between them, which ſtreams vaniſh upon contact of the knives; their parts *fartheſt* from the direct light vaniſhing *laſt*. As the knives, however, approached each other, and before the ſhadow appeared, the three fringes themſelves appeared on the inner ends of the ſtreams on either ſide of the direct light; and grew diſtincter and larger until they *all* vaniſhed, the outmoſt firſt, the middlemoſt next, and inmoſt laſt, leaving the ſtreams of light deſcribed in the fifth and ſixth obſervations. Theſe ſtreams, therefore, Newton concluded paſſed by the edges at *leſs* diſtances than any of the fringes, and the fringes at different diſtances according to the order of their vaniſhing.

Obſervation ε diſtinctly ſtates all the phænomena that occur at the parallel edges of two knives in a beam of parallel light approaching towards each other in the ſame plane. At firſt appear two borders of inflected light, one at each edge, ſimilar each to the one before obſerved at the ſingle edge. In theſe borders, and of this light, are made two ſets of fringes, one at each edge, which beginning in the form of ſlender white ſtreaks and dark intervals on the edges of the borders neareſt to the knives become broader, and coloured, and by degrees occupy the whole light between the edges, forming two complete ſets of fringes, each compoſed of the colours before deſcribed in obſervation β, and beginning in the centre of the paſſing light. Theſe fringes grow broader, then vaniſh by pairs, until at laſt only one pair, with a dark interval between them, remains, which decreaſe and vaniſh upon the contact of the knives. From this ſtatement it appears, that Newton was miſtaken in conſidering this laſt pair of fringes as ſtreams of light diſtinct from the fringes, and as being the very borders themſelves of inflected light obſerved at the edge of a ſingle knife, out of which they are indeed formed, and of which they really are only an inconſiderable part. In this caſe each edge by inflecting gives the light paſſing neareſt to it a due divergency, and the other forms fringes therein.

In obſervation ζ a very ſmall hole, $\frac{1}{30}$ of an inch wide, being uſed, gives a due divergency to the light, and, therefore, the two edges, when even at a diſtance, form fringes to themſelves of an arrangement of colours, the inverſe of thoſe in the preceding obſervation inſtead of borders of white light, but upon their nearer approach break up and diſſolve theſe, and by their more powerful mutual attractions form fringes in a contrary order, ſimilar to thoſe of obſervation ε, thus exhibiting, under different circumſtances, two different ſets of fringes.

Obſervation η, ſhews that even in different diſtant planes the ſame phænomena, with due change of circumſtances, are produced at the edges of two knives.

Obſervation ϑ, by placing parallel ſided plates of lead, of different breadths, in a divergent beam of light paſſed through a very ſmall hole, produced not only the two uſual ſets of fringes at their external edges, but alſo a ſecond double ſet within the ſhadow beginning from the centre thereof, both ſets of fringes growing broader as the breadth of the lead was diminiſhed, and the fringes in the ſhadow vaniſhing by pairs, and at laſt entirely diſappearing. Theſe fringes are the coloured ſtreaks obſerved by others on the ſhadows of ſmall bodies. Cylinders alſo of ſucceſſively reduced diameters down to the hair of the firſt obſervation, produce the ſame appearances, and even with the ſame cylinder or plate of a due ſize, the ſame appearances may be produced by diſtance or obliquity of obſervation.

In Newton's eighth obſervation the edges of the knives being inclined until they met, and the beam paſſing through a hole $\frac{1}{42}$ of an inch wide, at a very ſmall diſtance from the knives the fringes appeared at, ran along the edges and met, and where they met ended; but at a much greater diſtance they appeared to grow broader as they approached, then met and croſſed each other, and then appeared broader than before, whence he concluded, that by the approach of the edges the diſtances of the fringes therefrom are not *increaſed* or *changed*, but that the bendings of the rays are thereby *increaſed*, and that the neareſt knife determines which way the ray ſhall be bent, and the other *increaſes* the bent. Obſervation ι, repeats the experiment, and ſhows how by the inclination of the edges all the appearances which at different times were before produced by the ſucceſſive approach of the edges, are now all at one time exhibited at their different diſtances from each other. The fringes where the edges are diſtant are formed in the light rendered divergent by the ſmall hole, where they appear to have croſſed they are made by each remote edge in the light bent by the neareſt edge, in light which, if the remote edge was abſent, would there form the fringes of the neareſt edge; upon the approach, therefore, of the knives, the diſtances of the fringes of each knife *are changed* and increaſed, contrary to what Newton obſerves, for they are then made in light beyond that of the firſt fringes of each edge, that light neareſt to each edge compoſing *then* the fringes of the remoter edge, and as each edge thus makes its fringes in the light bent by the oppoſite edge, the nearer it approaches in more bent light will the fringes be made; not that the approach bends the light, it leſſens the bent by contrary attraction, but changing at the ſame time the light of the fringes, it makes them in other light nearer to the bending edge, and, therefore, more bent than the former light, and, therefore, the fringes are alſo broader. This making of broader fringes in light more bent led Newton into a wrong concluſion. The light does not increaſe its bending and divergence, the fringes change their light: by the approach of the edges they are made in light more bent and diverging, but the general bent of the rays is really *diminiſhed* by that approach, and whilſt the neareſt knife continues to bend the rays towards itſelf, the other knife *diminiſhes* that bent.

In obſervation the ninth, Newton having meaſured at different diſtances from the knives the dimenſions of the fringes, intervals, and ſhadows, and finding that the diſtances of the point of croſſing of the dark lines between the firſt and ſecond fringes from that of the concourſe of the ſhadows of the edges, continually increaſed, and taking it for granted, contrary to the fact, that theſe gave the true meaſures of the diſtances of paſſage of the light itſelf from the concourſe of the edges, concluded, that the light which made the fringes was *not the ſame* light at all thoſe diſtances, but paſſed *nearer* to the knives, and was *more* bent when the fringes were obſerved at ſmall than at greater inſtances. Obſervation κ, ſhows that theſe increaſed dimenſions are merely produced by the divergency of the light of the experiment, a circumſtance which eſcaped Newton in his firſt obſervation alſo, and that, conſequently, the fringes are always *the ſame* at all diſtances of obſervation, and are made in the *ſame* light *equally* bent, and paſſing at the *ſame* diſtance from the edge.

Obſervation the tenth of Newton is employed in an attempt by actual meaſurement to aſcertain the nature of the curves formed by the fringes and intervals in the preceding experiment. They are determined to be hyperbolic, and their aſymptotes drawn. The fringes themſelves are by him ſeparated, and diſtinguiſhed from another triangular light, ſuppoſed to appear between the two laſt neareſt the concourſe of the edges; but obſervation λ, remarks, that this light is really the light of the laſt pair of fringes neareſt to the concourſe of the edges exhibiting the appearance and colours which ſhould belong to theſe fringes under ſuch circumſtances, and from the overſight diſcovered with reſpect to this pair of fringes, from the accuracy required in the meaſurements of theſe ſhadowy parts, from the actual paralleliſm of the lines of the fringes on one ſide to, and the apparent probability that they will croſs and cut the aſymptotic lines on the other ſide, queſtions the accuracy of the determinations reſpecting theſe lines. Obſervation μ, ſhows how by uſing a triangular piece or cone of lead, or the points of pins of various ſizes, inſtead of the parallel-ſided pieces or cylinders of obſervation ϑ, all the appearances of that obſervation are at once produced at the edges of the triangle or cone.

In obſervation eleventh, Newton examines the fringes made by differently coloured priſmatic lights, and finding the fringes made by red light alone formed at greateſt, by violet at ſmalleſt, and by intermediate colours at intermediate diſtances, as if all thoſe lights were mixed in the white, and early ſeparated at the time of inflexion, concludes that the colours ariſe not from any modifications impreſſed upon the rays of light by the hair, but from ſimple ſeparation by various inflexions of the ſeveral ſorts of rays. To this mode of explanation another is oppoſed in obſervation ν, namely, that the different rays when apart are, indeed, inflected in the ſame manner to make fringes of various colours, as are rays of the ſame colour when being at once variouſly ſeparated from white light they make, together with the reſt of the colours, party-coloured fringes. This, however, is attributed not to ſimple ſeparation alone, but to modification at the time of ſeparation, which gives to each portion of the white light thus ſeparated a permanent condition, diſtinct from all the ſeparated portions, and the original portion out of which they were ſeparated.

Separating by inflection a ſmall portion of white light into various colours, and then returning the various rays back again to the ſame point under the ſame circumſtances, or ſuppoſing them ſo returned, they will unite into the original portion, or ſuppoſing them returned without other change to different points, they will move in parallel lines to each other, preſerving their diſtinct characters; or if now a ſecond time returned without further change, coming in the ſame parallel direction, they will be inflected differently. Theſe circumſtances evince a change and permanent conſtitution, ſuperinduced upon each of theſe portions, and by which they are rendered diſtinct from each other, and from the original portion until their re-union into the ſame, as a portion ſimilar to that from which they were derived. Theſe ſmall portions of white light are its rays, which paſs through the intervals between the particles of bodies, and coloured rays are always formed therefrom by ſimilar modifications, produced by the actions of the particles of other bodies at the ſame time, modifying as well as ſeparating them. The very mode of diſtribution of the white light argues the action of a principle, modifying as well as ſeparating, rather

than

than the exiſtence of different ſorts of rays, which may not eaſily be ſuppoſed ſo numerous, and capable of ſo regular a diſperſion. Thus modification is eſtabliſhed, as well as ſeparation, and theſe principles of condition and formation alone ſuit the phænomena. The fringes before obſerved, require complete ſets of the different coloured rays to iſſue *at the ſame time*, from *each interval* or point of paſſage of the medium. For this purpoſe an appropriate arrangement is required of the rays, if of originally different ſorts in different paſſages, and only mixed in the white light, leſt a nearer red ſhould be more inflected than a further blue, and the like, and this arrangement will even then agree with only one ſtation of the inflecting body; but this ſtation is indifferent in producing the phænomena. A confuſed mixture therefore, of rays, is inconſiſtent with the phænomena, no collateral arrangement will anſwer; and if all the different rays muſt be conſidered as paſſing through all the intervals of the medium, they cannot all iſſue from, or be returned at the *ſame time*, to *the ſame* place as the phænomena require. By ſuppoſing, however, that the time of ſucceſſion of all the rays in the ſame paſſages is imperceptible to ſenſe, then the hypotheſis of ſimple ſeparation, and original diſtinctneſs of the rays, may anſwer for theſe phænomena, as well as that of ſeparation and modification; but as they will both then anſwer equally well, other the general phænomena can alone determine which is true, and by them they muſt be judged.

Omitting, however, all conſideration of theſe two hypotheſes, it is clear, that in the preceding obſervations concerning inflections, Newton is miſtaken as to all the moſt important points. He has upon different occaſions, endeavoured to eſtabliſh a power altogether repulſive, or a power changed at different diſtances from attractive to repulſive. This repulſive power has been always hitherto uſed in philoſophy, to account for the reflections of light, and as it is clearly ſhewn not to exiſt, another manner of accounting for reflection is to be ſought for.

The concluding obſervations of the work are general. It is ſtated among other things, that the Newtonian doctrine of light and colours is unfounded as to both its principles; the conſtitution of ſolar light (queſtioned indeed in the laſt obſervation), and the fits of eaſy tranſmiſſion and reflection of the rays. The ſame Author is preparing to publiſh, Obſervations concerning the Colours of thin Plates, ſhewing the phænomena to be merely inflections of light, that the Newtonian fits of eaſy tranſmiſſion and reflection have no exiſtence, and that Newton's doctrine of the grounds of the colours of natural bodies is unfounded.

The obſervations of Newton in the above work marked with the numerical figures, are printed with a reduced margin; the new obſervations marked with the letters of the Greek alphabet, are printed with a full margin.

VIII.

Singular Inſtance of Fecundity. In a Letter from the Rev. W. Pearson *of Lincoln.*

To. Mr. NICHOLSON.

SIR,

I Was lately informed by Mr. Morris of Dunham, in this county, that he has an ewe which on Friday night, and Saturday morning laſt, yeaned *five* lambs all full grown; which I conſider as ſo remarkable an inſtance of fecundity, that I am induced to tranſmit a notice of it for public record in your Journal.—The ewe is ſomewhat above a middle ſize, and is a *croſs* of the Lincolnſhire and Leiceſterſhire breeds, three years old, and the ram which ſhe was put to, is of a thorough Leiceſterſhire ſtock: three of the lambs were male, and two female; they are all dead, but four of the five were found alive, and it is ſuppoſed would have ſurvived, if the inclemency of the night had not ſtarved them. The ſhepherd, who vouches for the veracity of theſe facts, aſſerts, that the fourth lamb came in an unnatural poſition, and that the ewe when he firſt went to her about midnight, was turned upon her back, and muſt have periſhed, on account of being in that poſition, if he had not accidentally come to her aſſiſtance: ſhe is, however, perfectly recovered, and many circumſtances concur in proving indubitably, that, though there were about thirty other ewes in the ſame grounds at the time, yet the five lambs in queſtion were all yeaned by the ſame ewe: I will mention one proof, which of itſelf may ſatiſfy thoſe readers, who may otherwiſe feel diſpoſed to doubt the authenticity of my information:—the ſhepherd, upon a ſuppoſition that the two firſt lambs that came were all that he had to expect, took them into a ſhelter, and returned to take care of the ewe, when, to his ſurpriſe, he was preſented with two more; theſe were taken away alſo, and on his ſecond return, the fifth was found dead newly yeaned, and no other ſheep was near:—the ſhepherd is not guilty of fabricating tales, and Mr. Morris, who is a reſpectable yeoman, has at this time all the five ſkins, which were taken from the lambs.

It may not be improper to add, that the other ewes of the ſame ſtock, and in the ſame paſture, have ſome two lambs, and ſome only one each, with the exception of one which has three living.

I am, Sir,
Your obliged Correſpondent,

Lincoln, April 14, 1800. W. PEARSON.

P. S. Since the receipt of the above, the learned writer has favored me with an extract from the Lincoln, Rutland, and Stamford Mercury of 18th laſt, ſtating that " on the 24th ult. at Mardon Woole, in Northumberland, an ewe, the property of T. Reed, Eſq.

yeaned five lambs, four of which were then living, the other appeared to be ſomething of an embryo fœtus, though nearly perfect;" and that "what tends to heighten aſtoniſhment, is, that the ſame ewe has had an equal number at each time for two preceding years."—W. N.

IX.

On the Phenomena of the Morning and Evening Dew. By C. A. Prieur [*].

THE evening and morning dews (*le Serein et la roſée* [†]) preſent themſelves ſo often to our obſervation, and in ſo many different ſituations, that it is ſurpriſing that philoſophers ſhould have attended ſo little to theſe phenomena, or that they ſhould have been contented with very looſe explanations on this ſubject.

In the year 1788, I had occaſion to ride out very frequently on horſeback in the morning and the evening, and being thus expoſed to the impreſſions of the dews, I was induced to meditate particularly on theſe appearances, hitherto ſo little examined. I well knew that the humidity depoſited on bodies in the open air at the ſetting of the ſun, is not the ſame as is afterwards obſerved upon them at its riſing; and that conſequently there is an interruption in the appearance, namely, an evaporation of the evening dew, or humidity, and a new production of moiſture in the morning. I was alſo aware of that partial explanation of the evening dew, by which it is ſaid, that the diminution of the heat in the air renders it incapable of retaining any longer the water it had taken up during the day. But why is there a wind always blowing from that ſide of the horizon occupied by the ſun, and conſtantly accompanying this precipitation of water; and again, how does it happen that the ſame luminary a ſhort time before its riſe, and even after having given a ſlight degree of warmth to the air by its preſence, appears to cauſe a greater cold, a ſtronger wind, and a more abundant precipitation of water than in the evening.

To remove this difficulty, I had recourſe to the fundamental principles, by the aſſiſtance of which, Monge has ſo ingeniouſly accounted for the greater part of the phenomena of meteorology [‡]; and I ſoon perceived, that they were ſufficient to explain the appearances which formed the ſubject of my enquiry.

Theſe principles, which it will be proper to enumerate, are the three following; 1. The air in like circumſtances diſſolves more water the greater its denſity, that is to ſay, the more it is mechanically compreſſed.

2. And alſo more the higher its temperature.

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[*] From a letter addreſſed to Cit. Haſſenfratz, and inſerted in the Journal of the Polytechnic School, vol. ii. cahier 6. p. 409.

[†] We have not diſtinct words correſponding with theſe in our language?---N.

[‡] Annales de Chimie, tome V.

3. Under equal preſſure, and at like temperatures, air holding water in ſolution has a leſs ſpecific gravity than air alone; and this ſpecific gravity is leſs the greater the proportion of water ſo diſſolved.

It muſt alſo be recollected, that the changes of preſſure and weight which may take place in certain columns of the atmoſphere, muſt neceſſarily diſturb the equilibrium, and produce motion or currents in the ſame.

Theſe principles being eſtabliſhed, let us ſuppoſe for a moment, to render our inquiry more ſimple and eaſy, that the earth deprived of its rotatory motion, ſhall remain motionleſs in the preſence of the ſun, and let us beſides diſmiſs from our conſideration all local influence, and in general all thoſe cauſes which might diſturb the regularity of the effects.

In this ſtate of things, what will be the conſequence with regard to our atmoſphere?

The air expoſed to the rays of the ſun will be heated, and principally in the part contiguous to the earth, on account of its greater denſity, and the reverberation of that planet. This heated air will acquire a greater diſſolving power, and will, in fact, diſſolve much water if that fluid be preſent. It will take it from the ſeas, lakes, rivers, ponds, and other reſervoirs, whoſe ſurface is expoſed to its action, and will even abſorb a portion of the humidity of the ground.

Let us in the next place conſider, what happens in any aſſumed vertical column in the atmoſphere, and firſt in that placed immediately beneath the ſun. The air heated at the lower part diſſolving water, and thus becoming ſpecifically lighter, will riſe, and be replaced by other contiguous air. An aſcending current will therefore be eſtabliſhed in the column. If this column were ſeparated from the others, as if it were contained in a vertical tube, in proportion as the lower air roſe loaded with water the upper air would deſcend, become charged in its turn, riſe and be replaced; and that perpetually. And the air holding water in ſolution, would in conſequence of under ſaturation from cold and diminiſhed preſſure, when it had arrived at a certain height, let fall the exceſs of water, and form a miſt or cloud, which might continue to riſe by virtue of its acquired motion, but which, after a certain accumulation, would fall again in rain. It may alſo be conceived, that this tranſlation of the air upwards and downwards, would be made either by a mutual infiltration, or currents in both directions, which would be conſtantly and regularly kept up.

But this is not entirely the caſe with our vertical column, becauſe it is not in fact ſeparated from the others in its vicinity. Theſe alſo are ſubjected to the ſame operations, with the exception only, that the effects are leſs the more remote they are from the column immediately beneath the ſun. The heat, the ſolution of water, and the force of aſcenſion, gradually diminiſh as the diſtance from this central column increaſes. If therefore the ſurface of the earth were a plane, the proper repreſentation of the riſing air would be a cone with its ſummit directed to the ſun; and on the other hand, as the abſolute weight of each column is increaſed by the whole of the water it has diſſolved, the equilibrium requires that there ſhould be on all ſides a lateral divergence, which muſt evidently take place where the preſſure is leaſt. Thus on the ſame ſuppoſition of the earth being a plane, we ſhould obſerve the upper air deſcend and precipitate itſelf along the ſides of the cone, producing by this oblique direction to the axis, a current on all ſides directed from the ſun; and this current would be increaſed by the vacuum formed at thoſe places where the air is ſufficiently elevated, and could precipitate its ſuperabundant water.

This image will require very little more dification to apply it to the exterior of our globe. The conical ſurface will be converted into a cap, enveloping the enlightened portion of the earth, and following the curvature produced by the currents.

Inſpection alone of the figure in Plate 3, will ſhew this curvature. The globe may be readily ſeen placed in the center of a circular ſtratum, ſuppoſed to be filled by the atmoſphere. The circumference of the earth has twenty-four diviſions, from which are raiſed a like number of perpendiculars to its ſurface, or vertical lines, in order to afford a notion of the change of phenomena from hour to hour. The ſun ſuppoſed to be placed at S, in the in the continuation of the line T S, is ſuppoſed to have all its maſs united in the center; and laſtly, the curve *a*, *b*, *c*, though arbitrarily drawn, becauſe its law is unknown, will be ſufficient to ſhew the exiſtence of the effects which we are intereſted to explain.

Such, therefore, will be the reſult of the ſuppoſed circumſtances. Water diſſolved by the lower air expoſed to the ſun; a motion of aſcent in this part; deſcending divergent currents on all ſides, ſpreading over the earth. Theſe currents, moreover, communicate their water to the heated columns, which riſe and diverge laterally; and this water is precipitated, becauſe the air of the currents proceeding from the upper regions is too cold to keep it in ſolution, or becauſe the preſſure is leſs from the diminiſhed weight of the columns, as their diſtance from the line immediately beneath the ſun is greater, as well as becauſe they may mix with other cold air near the ſurface; and laſtly, the ground, and all other bodies in the direction of this precipitated water, will be wetted therewith.

The morning and evening dews are here evident to our conſideration, with the wind and the cold which attend them; but a few more remarks are wanting to complete the deſcription. We muſt firſt obſerve that the deſcending currents are prolonged till the reſiſtance of the air, through which they paſs, has entirely deſtroyed their motion. Again, we ſhall ſee immediately beneath the ſun, a circular ſpace greatly heated, which does not preſent to the inhabitants of that region the phenomenon we have been deſcribing. As we depart from this ſpace, and in proportion as the ſun appears in a more oblique direction, we arrive at a region leſs heated, where the wind from the direction of the ſun, and the precipitation of moiſture, begins to be perceived. This region forms a crown round the circular ſpace before mentioned. And, laſtly, by departing ſtill more from the inner border of this crown, the wind is found to be ſtronger, the cold more perceptible, and the precipitation of water more abundant; which effects afterwards diminiſh to a certain diſtance, and entirely ceaſe at the ſurface of the earth diametrically oppoſite the ſun.

In this manner, on the hypotheſis of the immobility of the earth with regard to the ſun, there would be beneath that luminary a very extended region eternally, and uninterruptedly ſubjected to the phenomenon of the morning and evening dew, accordingly as the obſerver was placed to the eaſt or the weſt.

But let us now aſſume the real ſtate of things, and reſtore to the earth its diurnal rotation. The preceding phenomena will then take place on ſucceſſive parts of its ſurface. Thoſe places from which the ſun is deſcending, and ready to ſet beneath the horizon, will ſoon perceive the appearance of the evening dew with a weſt wind ſpringing up, which phenomena will increaſe gradually till after the ſetting of the ſun, and then the effect will diminiſh and entirely ceaſe. During the night the humidity will evaporate, and entirely diſappear, provided the air be not already too much loaded with moiſture. Towards the next morning, juſt before day-break, the phenomenon will again preſent itſelf on the eaſtern ſide, with the ſame circumſtances and gradations, the maximum of effect being alſo when the ſun is yet beneath the horizon; but with this very remarkable difference, that the effects will be much ſtronger than thoſe of the evening; that is, there will be more wind, more moiſture depoſited, and a more ſenſible degree of cold. The reaſon of this is, that in the evening the precipitation of water, the wind, and the cold which acaccompany it, ought to be diminiſhed, becauſe the whole takes place in the vicinity, and by the mixture of air which the ſun has heated during the day; whereas in the morning the coldneſs of the night air permits, or gives a much greater effect to the phenomenon.

We ſee likewiſe, that in the two temperate zones, where the winter and ſummer have a great difference of temperature, where the length of days and nights vary much, the effects of the evening and morning dews are varied and irregular. In ſummer, if on the one hand the air diſſolves more water in the day, on the other hand, the precipitation of the evening dew is made in an air very much heated, and that of the morning dew in air which the ſhortneſs of the night has cooled only to a certain point. In this caſe the ſolution of moiſture is conſiderable, and the precipitation little. In winter, on the contrary, the cauſe of ſolution is leſs, but that of precipitation more effectual. Local circumſtances, more eſpecially the vicinity of water, muſt alſo influence the effects. Fine weather increaſes, and renders them more ſenſible. Cloſe weather weakens or deſtroys them.

Under the torrid zone the days and nights are more nearly equal, and at the equator they are equal at all times. The difference between the ſummer and winter temperatures is leſs conſiderable than in our latitude, and the ſky is almoſt conſtantly clear. It muſt follow, that the morning and evening dews will be heavier under this burning climate, than elſewhere on the earth. And this agrees with the teſtimony of travellers. In Egypt, in Aſian Turkey, at the Antilles, at Mexico, in veſſels ſailing between the Tropics, the morning and evening dews fall ſo abundantly, that they produce the ſame effect as ſhowers of rain.

But there is a very important conſequence, which it ſeems allowable to draw from theſe phenomena; namely, that they muſt influence the production and permanence of the trade winds. For every day, almoſt regularly, the air of the torrid zone being ſolicited to move in two oppoſite directions by forces very different in quantity, it muſt tend in fact to acquire and preſerve a motion or current, in the direction of that ſtrongeſt power, which in this caſe is from the eaſt, being the wind which brings the morning dew. This cauſe muſt be the more effectual, as it acts in the lower part of the atmoſphere, where we feel the trade winds, and becauſe it affects the denſeſt portion, and conſequently muſt the more readily move the whole.

Under the glacial zone, where particularly during the winter the ſun ſcarcely ſkims the horizon through the whole day, the precipitation of water will be very conſiderable, by reaſon of the coldneſs of the climate, and a thick fog will prevail, which will ſcarcely be diſſipated in ſummer; but in winter will extend far into the temperate zones. Thus we ſee during the winter ſeaſon, in our country, very conſiderable fogs, which have only a feeble light even in the middle of the day.

It would be curious to preſent in this place a numerous ſeries of accurate obſervations, on the circumſtances which accompany the morning and evening dews at different times of the year, and in different countries, the hours at which theſe phenomena begin and end, the intermiſſions or irregularities, with which they are affected in different ſituations; but if the generality of the cauſes to which they are here attributed, ſhould excite the attention of philoſophers, the intereſting taſk here indicated will aſſuredly and ſpeedily be performed.

X.

Thoughts on Magnetiſm, by RICHARD KIRWAN, *Eſq; L. L. D. F. R. S. and M. R. I. A*[*].

1. THERE are two ways of explaining a natural phænomenon; the firſt, is by diſcovering the conditions and circumſtances of its production and the laws by which its action is governed; the ſecond, is by ſhewing its analogy, ſimilarity, or coincidence with ſome general fact with whoſe laws and exiſtence we are already acquainted; this laſt mode is by far the moſt perfect and ſatisfactory. In the firſt ſenſe of the word electricity and magnetiſm have been in ſome meaſure explained, but in the laſt ſenſe neither; the primary cauſe of magnetiſm in particular has hitherto been ſuppoſed to relate to iron alone, or its ores, and to ſtand unconnected with all other natural phenomena.

2. If therefore any other general fact or power can be diſcovered to which it bears ſome analogy or ſimilarity, it may ſo far be ſaid to be explained. Now ſuch fact or power I think may be aſſigned, namely, the power of *cryſtalization.*

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[*] Tranſactions of the Royal Iriſh Academy. Vol. VI.

3. By cryſtalization I underſtand that power by which the integrant particles of any ſolid poſſeſſing ſufficient liberty of motion unite to each other, not indiſcriminately and confuſedly, but according to a peculiar uniform arrangement, ſo as to exhibit in its laſt and moſt perfect ſtage regular and determinate forms.

4. This power is now known to be poſſeſſed by all ſolid mineral ſubſtances.

5. The forms which cryſtals, even of homogeneous ſubſtances, exhibit, are often very numerous; however in moſt caſes they may be reduced to a few primordial forms, which, as Abbé Hauy has lately experimentally proved, are derived from certain original forms appertaining to the minuteſt particles of their concretion.

6. The aſſemblage of theſe ultimate particles into viſible aggregates, ſimilarly arranged, neceſſarily requires that one of their ſurfaces ſhould be attractive of that particular ſurface of the other, which preſents a correſponding angle, and repulſive of that which preſents a different angle, otherwiſe the various regular rhomboidal and other polygon priſms and pyramids, which cryſtals preſent us, could never exiſt; conſequently the minuteſt priſm, being once formed, could never be prolonged if one end of ſuch priſms were not attractive, and the other repulſive of the ſame given ſurface.

7. Hence it has been obſerved that cryſtalization never takes place in the middle of any ſolution, but always begins at the ſurface or on the bottom or ſides of the veſſels that contain it, for the particles in the middle of the ſolution being confuſedly mixed with each other, and exerting their repulſive as freely as their attractive powers, the one conſtantly counteracting the other, no ſenſible accretion of a regular kind could take place, whereas the repulſive power of the uppermoſt particles, or of thoſe that reſt on the ſides or bottom of the veſſel, is reſtrained and impeded.

8. The repulſive power of cryſtalizing ſubſtances alſo appears in many other inſtances (of the attractive no doubt has ever been formed.) Thus if ſaturate ſolutions of nitre, common ſalt, and tartar vitriolate be mixed and ſet to cryſtalize, each will cryſtalize a part, which could not happen if the particles of each of theſe ſalts did not only attract their ſimilar homogeneous, but alſo repel thoſe of a different ſpecies, otherwiſe the mere caſual circumſtance of greater proximity to one than to the other would impel them to unite indiſcriminately. Again, if a ſaturate ſolution of allum be mixed with a turbid mixture of clay, and abandoned to inſenſible evaporation, after ſome time the clay will ſubſide and form a dry maſs, but in the interior of this maſs large regular cryſtals of allum will be found, whoſe component particles muſt, to reunite, have diſplaced and repelled the particles of clay with which they were ſurrounded.

9. If to a ſaturate ſolution of a ſalt that difficulty cryſtalizes, a cryſtal of a ſalt of the ſame ſpecies be inſerted, the whole ſolution will ſoon be brought to cryſtalize, as the cryſtal inſerted attracts the particles diſſolved, by its different ſurfaces; but if a ſalt of a different nature be inſerted this will not happen, cryſtalization will not be promoted.

10. If to a ſolution of 2 parts nitre, and 3 parts Glauber's ſalt in 5 parts water, a cryſtal of nitre be inſerted, the nitre alone will cryſtalize; or if inſtead of nitre a cryſtal of Glauber

be inſerted into it, the Glauber alone will cryſtalize, Do not theſe experiments fully evince both the attractive and repulſive powers, not only of different ſalts but of different ſurfaces of the ſame ſalt?

11. Theſe powers within their proper ſphere of action have been found indefinitely great; thus water confined in cannon ſeveral inches thick, and expoſed to a degree of cold much beneath the freezing point, has been obſerved to cryſtalize into ice that burſt the metallic impediment oppoſed to the form it then aſſumes.

12. The vaſt difference however attending the developement of theſe two powers (of magnetiſm and cryſtalization) will undoubtedly ſtrike many as an inſuperable objection to their identity, yet their *direction* in all its varieties being exactly the ſame, difference in other circumſtances ſeems to me to indicate rather a variety of *degrees*, in the ſame power, than any eſſential difference in the powers themſelves.

I now come to the application of the above principles to the magnetic phænomena. Theſe may in general be reduced to the following, viz. *Attraction*, *Repulſion*, *Polarity*.

Communication.

Declination.

Inclination.

Excluſive appropriation to Iron.

Deſtruction of the Magnetic power.

1ſt, *Attraction*, *Repulſion*, *Polarity*.

The quantity of iron found on and within ſuch parts of the ſurface of the globe as we are acquainted with, far ſurpaſſes that of any other mineral ſubſtance ſingly taken, or even of many of them taken together; ſcarce any ſtone or metallic ore or earth is found free from it; it enters into their compoſition in the proportion of from 2 to 18 or 20 per cent. and perhaps at a medium we may ſtate it in all of them at 6 per cent.; moreover its own ores are of all others the moſt common and the moſt copious; in many places, particularly in the moſt northern climates, whole mountains of it are found, and many of them magnetic. When to this conſideration we add that of the ſpecific gravity of the globe, which has been found to be 4,5 times heavier than water, notwithſtanding the immenſe quantity of water that covers the greater part of its ſurface to conſiderable unknown depths, and notwithſtanding that the ſpecific gravity of by far the greater part of the ſtones and earths it contains does not exceed and ſcarcely amounts even to three times the weight of an equal bulk of water, and that the quantity of mineral ſubſtances whoſe ſpecific weight exceeds four times that of water is almoſt infinitely ſmall in compariſon to the other known component parts of the globe, and finally that the weight of moſt iron ores is about four or five times that of water; all this I ſay conſidered, it is difficult to avoid concluding that the interior part of the globe conſiſts chiefly of iron ore diſpoſed in one or more aggregate maſſes; a concluſion that is farther confirmed, on reflecting that volcanic lavas ejected from the deepeſt receſſes with which we are acquainted contain from 15 to 20 or 25 per cent. of iron in the ſtate moſt favourable to magnetic attraction.

Taking then this aſſertion to be as fully proved as its ſubject matter is capable of being aſcertained, we may deduce from it the following corollaries:

1ſt. That as the ferruginous matter in the globe being by far the moſt copious, its univerſal attractive power is principally ſeated in the ferruginous part.

2d. That as all terraqueous matter was originally in a ſoft ſtate, its parts were at liberty to arrange themſelves according to the laws of their mutual attraction, and in fact did coaleſce and cryſtalize in the direction in which they were leaſt impeded by the rotatory motion of the globe, namely in that which extends from North to South, and principally and moſt perfectly in the parts leaſt agitated by that motion, namely thoſe next the centre.

3d. That this cryſtalization, like that of ſalts, might have taken place in one or more ſeparate *ſhoots*, or as we may here call them, immenſe ſeparate maſſes, each having its poles diſtinct from thoſe of the other, thoſe in the ſame direction repulſive of and diſtant from each other.

In conſequence then of the univerſal law of attraction of the particles of matter to each other, theſe internal magnets exert a *double* power of attraction; the firſt and moſt general, on the particles of *all* bodies indiſcriminately in proportion to their denſity, and the direct or inverſe ratio of the ſquares of their diſtances according as thoſe bodies are found within or without the earth's ſurface; and the ſecond, on bodies of their own ſpecies in proportion to their homogenity, and to the correſpondence of the arrangement of their integrant particles with that of the integrant particles of theſe internal magnets.

A magnet therefore is a maſs of iron, or of iron ore, whoſe oxygenation does not exceed 20 per cent. or thereabouts, whoſe particles are arranged in a direction ſimilar to that of the great internal central magnets of the globe. This I call the *magnetic arrangement.*

The particles of iron attract each other more forcibly than thoſe of any other known ſubſtance. This appears by its coheſion, hardneſs, elaſticity, and infuſibility, in each of which properties, or at leaſt in the combination of moſt of them, it exceeds all other known bodies.

Hence a magnet *attracts iron* when within the ſphere of its action, by forcing, in virtue of its attractive power, a certain proportion of its integrant particles into a diſpoſition and arrangement ſimilar to that of its own. For in this caſe it exerts a double attractive power, that of the particles of iron to each other, which we have ſeen to be the greateſt of all others, and that of cryſtalizing bodies, which we have alſo ſeen to be indefinitely great.

The cryſtalizing power being at once attractive and *repulſive*, according to the direction of the ſurfaces, (No. 6.) hence we ſee that one part or end of the magnet muſt repel that which the other has attracted, as long as the ſame diſpoſition of parts remains.

The diſpoſition of parts in a particular magnet, being ſimilar to that which obtains in the great internal general magnet, extends in the direction of from North to South. Hence magnets, when at liberty to move with a certain degree of freedom, and iron, when a ſufficient number of its particles are arranged in that direction, and has ſufficient liberty to conform to it, points to thoſe poles. Hence this property is called *Polarity*.

The magnetic power is greater or leſſer according to the number and homogenity of the particles *ſimilarly* and *magnetically* arranged. Hence ſmall magnets may be more powerful than a larger, and hence a magnet will attract a magnetized needle at a greater diſtance than one not magnetized.

The magnetic power decreaſes in a certain ratio of the diſtance of the particles that exerciſe it. Hence it is ſtrongeſt in the point of contact, and at the poles, as it is there moſt *unſaturated*, and weakeſt in the central part, which ſeparates the two oppoſite poles.

When a magnet is broken into ſmall pieces its power is nearly deſtroyed, becauſe though the poles ſhould be all of the ſame kind, yet the diſtance of each from the oppoſite pole is ſo ſmall that their powers counteract, and conſequently deſtroy each other.

If when a needle is attracted by the ſouth pole of a magnet a bar of iron be placed on the north pole, the needle is ſtill more ſtrongly attracted, becauſe the iron acquires alſo a ſouth pole, whoſe force is joined to that of the magnet.

If two needles be ſuſpended from any given pole of a magnet they will diverge, becauſe they both acquire the ſame polar arrangement. If a bar of Iron be laid on that pole of the magnet the divergence will diminiſh, becauſe the next end of the iron will acquire the diſpoſition of the oppoſite pole, and conſequently counteract the repulſive power of the magnet.

A magnet will not tranſmit its power through a bar of iron if this be too long. Muſchenbrouck limits their length to ſix feet, but this depends on the ſtrength of the magnet.

The power of a magnet (every thing elſe being equal) depends on the *number of its ſurfaces magnetically arranged*, and the *accuracy* of that arrangement.

The arrangement is accurate when the ſynonimous ſurfaces are exactly parallel to each other, and originally conformed to and parallel with thoſe of the great general magnet.

The magnetic attraction is ſtrongeſt in the direction perpendicular to the magnetic ſurfaces, and weakens in proportion to the magnitude of the angle of direction with the perpendicular, and conſequently is *null* when at a right angle with it. Hence the magnetic power ſeems concentrated at the *poles*, and the lateral powers are the weakeſt, as they originate only in the oblique direction of ſurfaces, or from ſurfaces inaccurately arranged.

(To be continued.)

Deſcription

XI.

Deſcription of the Revolving Doubler. W. N.

AS the doubler of Bennet, to which I adapted the following machinery in the year 1788, is not yet generally noticed in elementary works, and, conſequently, little known, I have thought it might be acceptable to copy the ſhort Deſcription and Plate from my paper in the LXXVIIIth volume of the Philoſophical Tranſactions.

Plate IV. repreſents the apparatus of the doubler ſupported on a glaſs pillar $6\frac{1}{2}$ inches long. It conſiſts of the following parts. Two fixed plates of braſs, A and C, are ſeparately inſulated and diſpoſed in the ſame plane, ſo that a revolving plate B may paſs very near them, without touching. Each of theſe plates is two inches in diameter; and they have adjuſting pieces behind, which ſerve to place them accurately in the required poſition. D is a braſs ball, likewiſe of two inches diameter, fixed on the extremity of an axis that carries the plate B. Beſides the more eſſential purpoſe this ball is intended to anſwer, it is ſo loaded within on one ſide, that it ſerves as a counterpoiſe to the revolving plate, and enables the axis to remain at reſt in any poſition. The other parts may be diſtinctly ſeen in fig. 2. The ſhaded parts repreſent metal and the white repreſent varniſhed glaſs. ON is a braſs axis, paſſing through the piece M, which laſt ſuſtains the plates A and C. At one extremity is the ball D already mentioned; and the other is prolonged by the addition of a glaſs ſtick, which ſuſtains the handle L and the piece GH ſeparately inſulated. E, F, are pins riſing out of the fixed plates A and C, at unequal diſtances from the axis. The croſs-piece GH, and the piece K, lie in one plane, and have their ends armed with ſmall pieces of harpſichord-wire, that they may perfectly touch the pins EF in certain points of the revolution. There is likewiſe a pin I, in the piece M, which intercept a ſmall wire proceeding from the revolving plate B.

The touching wires are ſo adjuſted, by bending, that when the revolving plate B is immediately oppoſite the fixed plate A, the croſs-piece GH connects the two fixed plates, at the ſame time that the wire and pin at I form a communication between the revolving plate and the ball. On the other hand, when the revolving plate is immediately oppoſite the fixed plate C, the ball becomes connected with this laſt plate, by the touching of the piece K againſt F; the two plates, A and B, having then no connection with any part of the apparatus. In every other poſition the three plates and the ball will be perfectly unconnected with each other.

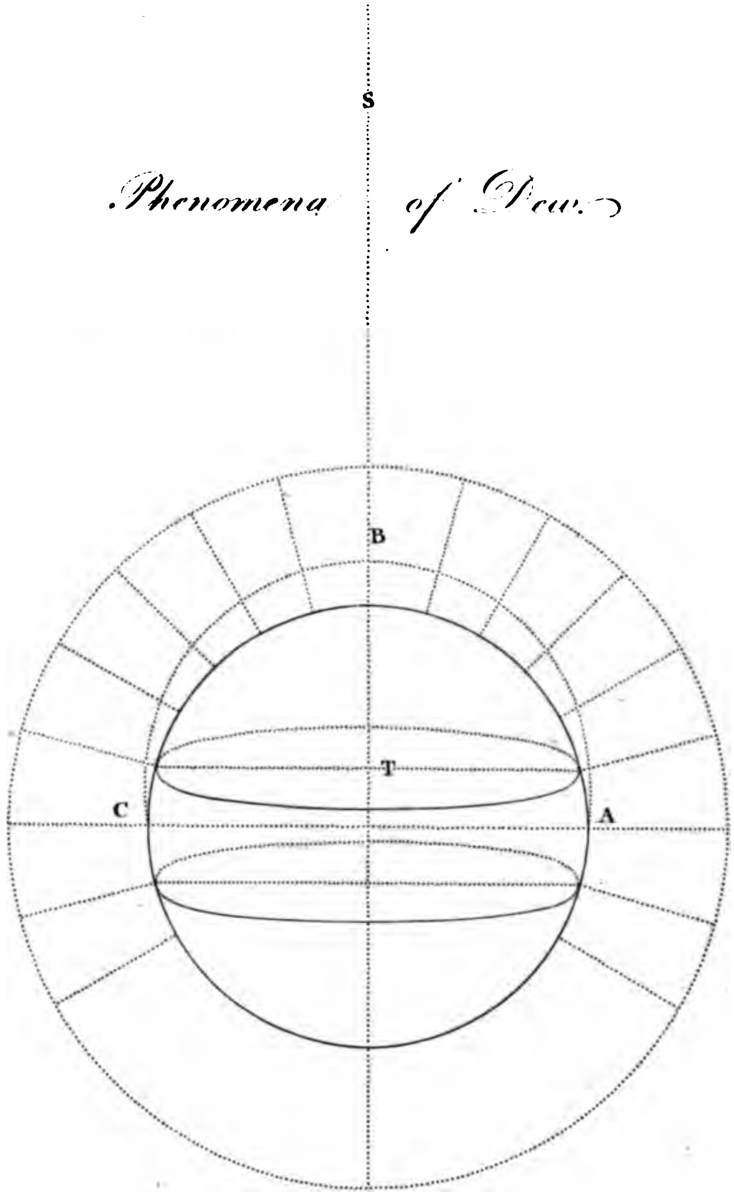
Mr. Cavallo's diſcovery, ſo well explained in the laſt Bakerian Lecture, that the minute differences of the electrization in bodies, whether occaſioned by art or nature, cannot be completely deſtroyed in any definite time, may be applied to explain the action of the preſent inſtrument. When the plates A and B are oppoſite each other, two fixed plates A and C may be conſidered as one maſs; and the revolving plate B, together with the ball

D, will conſtitute another maſs. All the experiments yet made concur to prove, that theſe two maſſes will not poſſeſs the ſame electric ſtate; but that, with reſpect to each other, their electricities will be plus and minus. Theſe ſtates would be ſimple and without any compenſation, if the maſſes were remote from each other; but as that is not the caſe, a part of the redundant electricity will take the form of a charge in the oppoſed plates A and B. From other experiments I find that the effect of the compenſation on plates oppoſed to each other, at the diſtance of one-fortieth part of an inch, is ſuch that they require, to produce a given intenſity, at leaſt one hundred times the quantity of electricity that would have produced it in either, ſingly and apart. The redundant electricities in the maſſes under conſideration will therefore be unequally diſtributed: the plate A will have about ninety-nine parts, and the plate C one; and, for the ſame reaſon, the revolving plate B will have ninety-nine parts of the oppoſite electricity, and the ball D one. The rotation, by deſtroying the contacts, preſerves this unequal diſtribution, and carries B from A to C, at the ſame time that the tail K connects the ball with the plate C. In this ſituation, the electricity in B acts upon that in C, and produces the contrary ſtate, by virtue of the communication between C and the ball; which laſt muſt therefore acquire an electricity of the ſame kind with that of the revolving plate. But the rotation again deſtroys the contact, and reſtores B to its firſt ſituation oppoſite A. Here, if we attend to the effect of the whole revolution, we ſhall find that the electric ſtates of the reſpective maſſes have been greatly increaſed: for the ninety-nine parts in A and in B remain, and the one part of electricity in C has been increaſed ſo as nearly to compenſate ninety-nine parts of the oppoſite electricity in the revolving plate B, while the communication produced an equal mutation in the electricity of the ball. A ſecond rotation will, of courſe, produce a proportional augmentation of theſe increaſed quantities; and a continuance of turning will ſoon bring the intenſities to their maximum, which is limited by an exploſion between the plates.

If one of the parts be connected with an electrometer, more eſpecially that of Bennet, theſe effects will be very clearly ſeen. The ſpark is uſually produced by a number of turns between eleven and twenty; and the electrometer is ſenſibly acted upon by ſtill fewer.

If the ball be connected with the lower part of Bennet's electrometer, and the plate A with the upper part, and any weak electricity be communicated to the electrometer, while the poſition of the apparatus is ſuch that the croſs-piece GH touches the two pins; a very few turns will render it perceptible. But here, as well as in the common doubler, the effect is rendered uncertain by the condition, that the communicated electricity muſt be ſtrong enough to deſtroy and predominate over any other electricity the plates may poſſeſs. I ſcarcely need obſerve, that if this difficulty ſhould hereafter be removed, the inſtrument will have great advantages as a multiplier of electricity in the facility of its uſe, the very ſpeedy manner of its operation, and the unequivocal nature of its reſults.

Phenomena of Dew.



Mutlow. sculp

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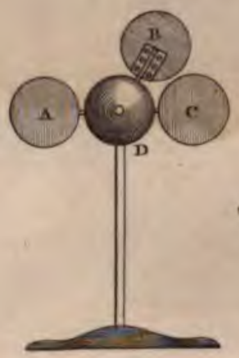

Fig. 1.

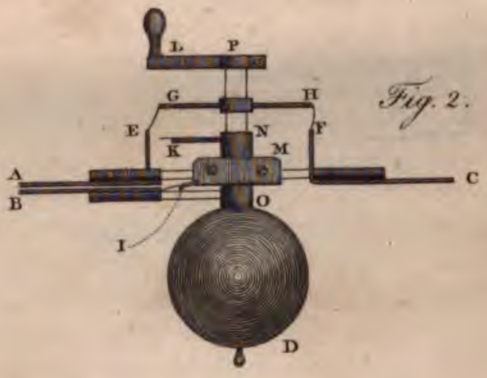

Fig. 2.

Mutlow sculp.

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A
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THE ARTS.

JUNE 1800.

ARTICLE I.

Obſervations on the Proofs of the Huttonian Theory of the Earth, adduced by Sir James Hall, Bart. By R. Kirwan, Esq. Communicated by the Author.

As ſome poſitions which I laid down in my examination of Dr. Hutton's Theory of the Earth, may ſeem queſtionable from the ingenious reaſoning employed by Sir James Hall in the third volume of the Edinburgh Tranſactions, to corroborate ſome of Dr. Hutton's aſſertions, and may even be thought inconſiſtent with ſome of the curious reſults that occurred in the highly intereſting experiments inſtituted by the worthy Baronet, and inſerted * in the fifth volume of the Edinburgh Tranſactions (a printed tranſcript of which he has had the goodneſs to ſend me), I think it a duty incumbent upon me to examine both the general reaſoning employed by him, and the conſequences fairly deducible from his experiments. Fanciful and groundleſs as the Huttonian theory ſeems to me to be, it may, like the reſearches for the philoſopher's ſtone, be highly uſeful by ſuggeſting new experiments.

In the third volume of the Edinburgh Tranſactions, Hiſt. p. 9, we are informed, that Sir James Hall, though convinced from various obſervations that granite had once flowed in a ſtate of fuſion, yet acknowledged that ſome difficulties accompanied this opinion;

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* Alſo in this Journal, IV, 8. 56.

among which the moſt conſiderable appeared to him to be this, that in ſome caſes the felſpar is ſeen in this ſtone with its cryſtals regularly defined, whereas the quartz forms a confuſed and irregular maſs, being moulded on the cryſtals of felſpar, whereas if the granite were formed by fuſion, the very contrary, he ſays, ſhould, it would ſeem, be expected; felſpar being very fuſible, and quartz, on the contrary, highly infuſible. In anſwer to which, he ſays, " that when quartz and felſpar are mixed and pounded together, it is well known they may be melted without difficulty into a kind of glaſs, the " felſpar ſerving as a flux to the quartz, or the felſpar may be conſidered as a menſtruum " in which the quartz is diſſolved; and in this view we may expect by analogy phænomena " ſimilar to thoſe of the ſolution of ſalt in water. Now it is certain that when exceſſive " cold is applied to ſalt water, the water is frozen to the excluſion of the ſalt; why ſhould " not the ſame thing happen in the ſolution of quartz in the liquid. Felſpar, when the " maſs is allowed to cool beneath the point of congelation of the menſtruum? The felſpar " may cryſtallize ſeparately from the quartz, as we have ſeen pure ice formed ſeparately " from the ſalt."

In this anſwer ſeveral particulars deſerve conſideration. In the firſt place, water (to which felſpar is here aſſimilated) is never *regularly* cryſtalized when frozen by exceſſive refrigeration, though, indeed, vapour may; conſequently, ſince in the preſent caſe the felſpar is ſaid to be regularly cryſtallized, the parity does not hold. Again, to juſtify the compariſon of felſpar acting on quartz as a menſtruum, as water does upon ſalt, the felſpar ſhould *always* be in the larger, and quartz in the ſmaller proportion to each other, as water always is to ſalt, and this is, indeed, the commoneſt caſe even where the felſpar is not regularly cryſtalized, yet in Swiſſerland this does not happen, as Mr. Hoepſner atteſts, 4 Helvetic Magaz. p. 266, of which ſpecimens may be met in 2 Leſke Catal. Engliſh edition, p. 375, 376, No. 37, 38, 40, 41; nor in Sileſia, as Gerhard remarks, 1 Grundriſs Min. Syſtem, p. 404 and 405. How then could the felſpar have ſerved as a menſtruum or flux to the quartz in theſe caſes?

3dly, It is allowed by all obſervers, that the caſes in which felſpar in granite is regularly cryſtalized, are exceeding few; ſee Lentz, Emerling, Widenman, &c. Granites, in which ſuch cryſtals are obſerved, are called *porphyraceous granites*, and from that very circumſtance judged by many obſervers not to be ancient granites, but of modern formation; ſee 2 Widenman, p. 1005, in the note. An obſervation ſimilar to that of Sir James Hall has alſo been made by Mr. Beſſon, in Limoges, 29 Roz. Jour. p. 89, for he diſcovered veins of granite in an argillite, though this ſchiſt did not border upon any granitic maſs, and hence he judged it of modern formation. Citizen Dolomieu alſo tells us, that ſuch inſtances had occurred to him in his travels, but he thinks them perfectly diſtinct from the granite which forms granitic mountains, 16 Journ. des Mines, p. 22. Neither was Sauſſure a ſtranger to ſuch granitic veins; but he accounts for their origin very differently from Sir James. §. 600.

4thly, Various attempts have been made to fuſe granites, in moſt of which, as has already been ſaid, felſpar is the moſt abundant ingredient; but in almoſt all the finely pulverized, the quartz remained unfuſed, and might be diſtinguiſhed by a lens; ſee 1 Sauſſure, §. 172, 173, and 174, 1 Gerh. Geſch. §. 51, and in the firſt part of his new Mineral Syſtem, publiſhed in 1797, p. 412, and Hacgueſt in 1 Crell Baytrage, p. 34, 35, &c. It is plain, then, that in all heats with which we are acquainted, the felſpar cannot but in very rare caſes ſerve as flux or a menſtruum to the quartz with which it is found in granites, the full proportion of quartz, which can be rendered fuſible by its other component earths, being already contained in the felſpar; and, in fact, there is no analogy betwixt water acting as a menſtruum on ſalt, and felſpar acting on quartz, for water and ſalt are ſubſtances perfectly heterogeneous to each other, whereas felſpar and quartz are both earthy ſubſtances, of which the former contains a large proportion of the latter, as eſſential to its compoſition, and is fuſible only by reaſon of its compound nature; but if the quantity of the quartzy ingredient be increaſed, the whole becomes infuſible, as I have experienced; whereas if the proportion of ſalt in water be increaſed, ſtill the water will be congealable if conſiderably cooled; moreover quartz frequently bears the impreſſion of ſtones more fuſible than itſelf, which could not happen in any poſſible ſuppoſition, if all had been in ſtate of fuſion.

Again, Sir James obſerved, that a quantity of green glaſs, which had been allowed to cool ſlowly, was found to have loſt all its vitreous properties, being opaque, white, and refractory; but being again melted by a blow pipe, and ſuddenly cooled, it reſumed its former properties, and became glaſs: hence he infers, that if the glaſs produced by the fuſion of granite had been allowed to cool with ſufficient ſlowneſs, it might have cryſtalized, producing a granite ſimilar to the original, p. 11.

The obſervation on glaſs, here mentioned, is perfectly juſt, and has been often repeated; but the analogy betwixt this caſe, and the formation of granite from a complete fuſion of its ingredients, is far from being accurate. Glaſs conſiſts of a ſimple earth, namely, the ſiliceous, united to an alkali. To form this union, it is neceſſary that the integrant affinity of the ſiliceous particles to each other ſhould yield to the chemical affinity which the alkali bears to them, and this can happen only in ſo high a degree of heat, as conſiderably leſſens the affinity of the ſiliceous particles to each other. If, when this union is effected, the compound is conſiderably and *rapidly* cooled, yet the union will ſtill continue, becauſe the alkaline menſtruum being congealed, the ſiliceous particles cannot move through it to reunite to each other, though their affinity to each other in a *low* temperature be greater than their affinity to an alkali, and thus they continue in that ſtate which we call *glaſs*. Two experiments ſet this explication beyond all doubt, the firſt is, that if a ſolution of ſalt in water be ſuddenly cooled from 140 degrees above to 6 degrees below 0 of Fahrenheit, the whole will be congealed, and no ſeparation of the ſalt will take place, ſee 8 Nov. Comment. Petropol. p. 346. This caſe is perfectly analogous to that of glaſs. The ſecond experiment is that of Tromſdorf, 22 Ann. Chym. p. 115, where we find the ſiliceous particles to have ſeparated by long ſtanding (8 years) from the alkaline in a ſolution of ſilicited alkali, and to have formed perfect cryſtals hard enough to ſtrike fire with ſteel.

That the glaſs thus formed, being ſuffered to cool ſlowly, ſhould be decompoſed, is very natural; it is what happens when certain ſalts, for inſtance, nitre, are diſſolved in water to ſaturation, in a boiling heat; if the water be ſlowly cooled, moſt of the nitre will cryſtalize and ſeparate itſelf. That the ſiliceous earth, thus ſeparated, ſhould be more refractory than before, ſhould be alſo expected; both becauſe it is not repulverized (at leaſt not ſtated to have been ſo) and becauſe much of the alkali, which is its menſtruum, evaporates, and is volatilized during the ſlow refrigeration. But if the heat applied be much greater than at firſt, it may be vitrified a ſecond time, as more of moſt ſalts may be diſſolved in a ſmall quantity of at 212 than at 150.

But to reproduce granite from a general fuſion of all its ingredients by a refrigeration ever ſo low, is a very different caſe from that we have juſt conſidered.

Granite is an aggregate ſtone, conſiſting of quartz, felſpar, and mica; of theſe the moſt fuſible is undoubtedly the felſpar, and the quartz the leaſt: let us then, to indulge the worthy Baronet, ſuppoſe all three in perfect fuſion in a high degree of heat, and afterwards ſlowly cooled, and thus each (though vouched by no experiment) gradually reproduced; the quartz, with the exception of the proportion thereof, which enters into the compoſition of felſpar and of the mica, would undoubtedly cryſtalize firſt on the ſmalleſt diminution of heat, and being congealed in a medium ſtill in a liquid ſtate, I do not ſee why it ſhould not form regular cryſtals, which, nevertheleſs, ſcarce ever occur in granite, except in cavities: over this, and after a conſiderable interval of time, the mica ſhould alſo be regularly cryſtalized, and laſt of all the felſpar ſhould coaleſce and congeal (at leaſt in the Baronet's ſuppoſition) in regular cryſtals; now as the cryſtalizations of theſe three ſpecies of ſtone take place each at a diſtinct portion of time, each ſhould occupy alſo a diſtinct portion of ſpace, the firſt ſet of cryſtals being loweſt, the next over that, and the laſt uppermoſt, as we find to happen when ſalts of very different ſolubility, and yet in equal quantity, are diſſolved and cryſtalized in water, or when ſubſtances of different degrees of volatility are ſublimed by fire. Now among the immenſe maſſes of granite that have been obſerved and examined in various parts of the globe, not above half a dozen have occurred in which the three conſtituent parts of granite were regularly cryſtalized, very few in which diſtinct layers were ſeen, and *none at all* conſiſting of diſtinct regular cryſtals of each, ſuperimpoſed on each other. On the contrary, in far the greater number of granitic maſſes the three above-named conſtituent maſſes lie intermixed with each other in the moſt confuſed and irregular manner, without any appearance of regular cryſtalization, inſomuch, that none can ſay, from bare inſpection only, which was cryſtalized firſt and which laſt. Nay, granitic maſſes not unfrequently occur, in which it is evident that the mica muſt have cryſtalized contemporaneouſly with the quartz; for in breaking the quartzy part, flakes of mica are found within it. See 6 Sauſſ. § 1621.

Laſtly,

Laſtly, I muſt add, that even on the ſuppoſition that diſtinct cryſtals of quartz, felſpar, and mica, could be produced by fuſion, they ſtill would be far from reſembling thoſe we are acquainted with, which eſſentially contain ſome particles of water, as I have elſewhere ſhewn.

Perhaps ſome may ſay, that the ſame difficulties occur in accounting for the cryſtalization of granite in the moiſt way; on mature conſideration, however, it will readily be ſeen, that the cauſes of coadinution in the dry and moiſt way are very different, and that their effects ſhould alſo be different; for ſuppoſing the earths that enter into the compoſition of granites diſſolved in the moiſt way, their precipitation and imperfect cryſtalization may be aſcribed to the union they contract with each other, forming maſſes of each of the conſtituent ingredients of granite, which water can no longer hold ſuſpended; hence the precipitation of each of the three ſpecies of ſtone is nearly contemporaneous, whereas if the formation of theſe ingredients ſhould take place in the dry way, it would neceſſarily be ſucceſſive keeping place with the ſucceſſive diminutions of heat, and then the above-mentioned conſequences would naturally enſue.

The ſtate of the granatic ingredients in fuſion, which I have above given, agrees pretty nearly with that preſented by Sir James himſelf; he ſuppoſes the quartz felſpar, ſhorl, mica, and garnet, &c. melted together, and the moſt fuſible of them to be the menſtruum in which the reſt are diſſolved, and that they differ from each other in their properties of ſolution, as ſalts differ from each other. Some of them being more ſoluble in the menſtruum when very much heated, than when it is comparatively cold, and others may be ſoluble in it when little warmer than its point of congelation. "If then we ſay, for in-" ſtance, that the congealing point of the ſolvent is 1000° of Fahrenheit, and if the ſolu-" tion is at the temperature of 2000°, we may conceive one portion of the matters diſ-" ſolved, as held by the ſimple diſſolving power of the menſtruum, and another as held by " means of its elevated temperature; when therefore a maſs of this kind is allowed to cool " very ſlowly, thoſe ſubſtances held in ſolution by the heat of the ſolvent, will firſt ſeparate, " and being formed in a liquid, will aſſume their cryſtaline form with regularity." This conſequence is truly deduced from the Baronet's hypotheſis, but being contrary to fact, diſcovers the falſhood of that hypotheſis; for if any of the fore-mentioned component parts of granite can be ſaid to be held in ſolution by the high heat of the ſolvent, it is ſurely the quartz; now the quartz is ſcarce ever found regularly cryſtalized when forming a component part of granite, as all mineralogiſts atteſt, and is a matter of univerſal obſervation.

" But the Baronet continues—" whereas thoſe ſubſtances which were held by the men-" ſtruum ſimply as a fluid, will not ſeparate until the congelation of the ſolvent itſelf takes " place, when the cryſtals of the various ſubſtances will intermix and confound the regu-" larity of form which each would have aſſumed if left to itſelf. In this manner one of the " common kinds of granite will be produced, conſiſting of perfect cryſtals of ſhorl, mica, " or garnet, incloſed in a confuſed maſs of felſpar, quartz, and ſhorl."—This concluſion is as objectionable as the foregoing; for not to mention that granites, in which ſhorl, and eſpecially garnets are found, are far from being common aſſured by ſhorl and garnet, approach more to the fuſibility of felſpar, (the ſuppoſed menſtruum) than either quartz or mica. Theſe, therefore, are thoſe which ſhould cryſtallize without any regular form in the Baronet's hypotheſis, and not the quartz and mica; which is juſt the contrary of what he himſelf has obſerved, for he tells us, p. 9, " he found the cryſtals of felſpar regularly " defined."

Sir James has ſince, very wiſely, declined juſtifying his theory of the formation of granite by fuſion; and by the advice, Dr. Hope very properly applied himſelf to experiments on various ſpecies of *whin*, a denomination which in Scotland comprehends grunſtein, baſalt, trap, wacken, and porphyry.

Porphyry ſtones, in which, except the laſt, none of the component ingredients are found regularly cryſtalized, and on the laſt he has made no experiment. The former, he tells us, were ſoftened or fuſed in a heat of from 38° to 55° of Wedgwood, the glaſſes to which they were reduced were ſoftened on a range of from 15° to 24°, and the maſſes of the original ſtony appearance, to which theſe glaſſes, reduced by ſlow cooling, were ſoftened in degrees of heat from 32 to 45. To the formation of theſe laſt he conſtantly applies the term *cryſtalization*, and calls them *cryſtalites*. To the vague term of cryſtalization I muſt however object, for as thoſe ſtones in their original ſtate preſent no regular cryſtals, but are at moſt internally, and imperfectly cryſtalized, ſo they muſt be when reduced from a glaſſy ſtate, to one reſembling their original, and thus diſcover rather a *niſus* towards cryſtalization, than perfect cryſtals, which latter the term cryſtalization generally applied, would lead us to expect.

Before I proceed to the detail of theſe experiments, I muſt obſerve, that the different fuſibilities of theſe *cryſtalites*, as he calls them, indicate a very different ſtate from that in which they originally exiſted; the former requiring a heat of from 32° 45°, and the latter a heat of from 38° to 55°, the reaſon of which is eaſily diſcovered, when the two ſtates are deduced from a different origination, but is in vain ſought for when both are to be deduced from one and the ſame origin.

Paſſing over the general preliminary accounts of theſe experiments, which are to be found from p. 7, to p. 10 of this diſſertation, I ſhall now examine the moſt important particulars of each, as far as they give occaſion to any ſtriking obſervations. In this examination I am much aſſiſted by the ingenious, accurate, and ſkilfully conducted analyſes of Dr. Kennedy, who bids fair to rival the excellence attained by the greateſt maſters of that ſublime and difficult art.

(*To be concluded in our next.*)

II.

Experiments on the Combuſtion of the Diamond, the Formation of Steel by its Combination with Iron, and the pretended Tranſmiſſion of Carbon through the Veſſels. By Sir GEORGE STUART MACKENZIE, *Bart.*

To MR. NICHOLSON.

SIR,

THE account which I now ſend you, of ſome experiments with regard to the properties of the diamond, and the formation of ſteel, was read at a meeting of the Royal Society of Edinburgh, on the 3d of February laſt. By giving it a place in your valuable Journal, you will much oblige,

Sir,

Your obedient ſervant,

Edinb. 1ſt. May, 1800. GEORGE S. MACKENZIE.

The phænomena exhibited by the diamond during combuſtion have been deſcribed by various authors *, but the temperature at which this combuſtion takes place has not as yet been aſcertained. I have made ſeveral experiments on diamonds with a view both to ſatiſfy myſelf, as to their appearance while burning, and alſo to determine at what degree of Wedgwood's pyrometer they begin to be conſumed. My experiments were conducted in the following manner †.

A diamond was placed on a thin flat piece of baked Corniſh clay, and introduced into a muffle, previouſly heated red hot. It ſoon acquired the ſame redneſs as the muffle, but in a few moments more, became diſtinguiſhed by a bright glow ‡. It was then removed, and on examination, its tranſparency and luſtre appeared to be affected. It had a ſlight milky appearance §, its angles were blunted, and its bulk was alſo conſiderably diminiſhed. After being replaced in the muffle, it was conſumed ſlowly, and at laſt diſappeared entirely.

The French chemiſts take notice of their having obſerved on diamonds after being ſubjected to a ſtrong heat, black ſpecks, and a blackiſhneſs upon their ſurfaces. Mitouard ſays, that in one of his experiments, a diamond had become as black as jet. In the many experiments detailed in the Journal de Phyſique, this effect was not uniformly produced, and has not been properly explained. I did not obſerve any of theſe appearances on diamonds which had been expoſed to various degrees of heat between 15° and 30°. There was a ſort of luminous haze round the diamond while burning, but ſo feeble as to be ſcarcely perceptible[*].

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* D'Arcet, Rouelle, Cadet, Lavoiſier, &c. Journal de Phyſique for 1772,-73,-89.

† All the diamonds uſed in theſe experiments were cut and poliſhed.

‡ This glow is deſcribed by D'Arcet and Rouelle as being more or leſs bright, in different diamonds, expoſed to heat at the ſame time. This difference, I ſuppoſe, was owing to their being nearer to, or farther from the mouth of the muffle, and conſequently differently expoſed to the action of the air.

§ "Qui eſt d'une tranſparence un peu *laiteuſe*." Journ. de Ph. Vol. I. for 1772, p. 485.

During the combuſtion of a diamond, I placed beſide it a ſmall piece of plumbago, and obſerved, that the latter exhibited a luminous appearance ſimilar to that of the former, but it began at a lower temperature. The air being excluded for a few minutes by cloſing the mouth of the muffle, the diamond and plumbago both loſt their brightneſs; but this ſoon returned, after the air was allowed to circulate in the muffle, and was much increaſed by blowing on them with a pair of bellows.

I expoſed a diamond fixed in a fine platina wire to the action of the blow pipe, during which treatment its conſumption was extremely ſlow, owing, I preſume, to the impurity of the air blown upon it.

In order to aſcertain at what temperature this ſubſtance begins to be conſumed, I placed a diamond with a pyrometer on a piece of baked clay, and puſhed them gradually into the muffle. As ſoon as both were perfectly red throughout, the pyrometer was withdrawn, and indicated 13°. The diamond had acquired the dim milky appearance already mentioned. The diamond and pyrometer were replaced in the muffle, and the heat being ſlowly increaſed, till the glow appeared, was continued as equal as poſſible, till the diamond was totally conſumed: the pyrometer was then meaſured, and indicated 14°[†]. In another diamond, the heat requiſite to produce the glow was 15°, and at this temperature it was wholly conſumed.

Theſe obſervations were made in preſence of Lord Webb Seymour, Sir James Hall, and Dr. Kennedy.

The experiments above-mentioned were ſeveral times repeated with different diamonds: and the reſult of the two laſt ſhows, that the heat required for their combuſtion is far below what has hitherto been ſuppoſed neceſſary.

The experiment of Guyton, in which the diamond was totally converted into carbonic acid by its combuſtion in oxygene gas, has afforded a deciſive proof of its identity with carbon. This important diſcovery has been farther confirmed by obtaining ſteel from the union of diamond with ſoft iron. I repeated the latter experiment in the following manner.

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[*] "On a apperçu à leur ſurface, déja diminuée de volume, une flamme legère, et entierement ſemblable, pour la couleur, a celle que l'on voit onduler ſur une portion de Phoſphore exposè a l'air libre." Journ. de Ph. Vol. II. for 1772, p. 114.

[†] The pyrometers uſed in theſe experiments were made ſome years ago by the late Mr. Wedgwood; and are the ſame with thoſe which he has deſcribed in Vol. 76 of the Philoſophical Tranſactions of London. I mention this becauſe thoſe lately ſold at the manufactory, do not agree with the former ſtandard.

Into a ſmall hollow cylinder of ſoft iron, cloſed at one end, I put ſome diamonds, and having introduced into the cavity above them a ſtopper of the ſame ſoft iron, which fitted exactly, the two pieces were rivetted together at the top. The ſtopper was nearly in contact with the diamonds, but I did not fill up the intermediate ſpace with iron filings, as the French chemiſts did, for fear of introducing ſome fragments of ſteel from the file. Having placed the cylinder in a Heſſian crucible, I ſurrounded it with a mixture of dry ſand and clay*. I luted a lid on this crucible, and placed it in another, on which a lid was alſo luted, and a ſmall clay caſe containing a pyrometer piece, was attached to its outſide. I wiſhed to ſubject the iron to a heat not exceeding 150°; for which purpoſe I placed the crucibles in a forge. After they were red hot, the fire was raiſed, till I thought it had attained the pitch deſired, and was then continued as equal as poſſible for an hour. The pyrometer indicated 151°.

On opening the crucibles, I found that the upper part of the iron had been melted, and I obſerved ſeveral bright metallic globules adhering to the compacted maſs of ſand and clay next the iron. The lower part of the cylinder retained its ſhape, but except a portion of the bottom which remained ſmooth, was bliſtered on the ſurface. Having poliſhed both ends of it, I found that on touching them with diluted nitric acid, they exhibited the ſpot which that acid uſually produces on ſteel. The ſpot on the end which had been fuſed, was conſiderably darker than that on the other. I cut off the portion which had remained ſmooth, and having heated it red hot, and plunged it into cold water, it became ſo hard, that no impreſſion could be made on it with a file. Several cavities were found within the cylinder, but the diamonds had totally diſappeared.

The whole cylinder was thus converted into ſteel; one end being reduced to the ſtate of caſt ſteel, and the other remaining in that of cemented ſteel; from which it ſeems probable, that the upper part alſo had paſſed into the ſtate of cemented ſteel before it began to melt. It may therefore be inferred, that diamond may be combined with iron ſo as to form ſteel by the ſimple proceſs of cementation.

Mr. Muſhet, of the Clyde Iron Works, has lately publiſhed in the Philoſophical Magazine, an account of certain proceſſes, with a view to prove "That the experiment performed at the Polytechnic School reſpecting the converſion of iron into ſteel, by means of the diamond, was not concluſive." The manner in which Mr. Muſhet's experiments were conducted, appeared to me to be liable to ſome objections; and it was therefore neceſſary to aſcertain, how far the diamond had contributed to the formation of the ſteel in the laſt experiment, by expoſing part of the iron of which the cylinder was made to the ſame degree of heat in ſimilar circumſtances. Accordingly, a portion of the ſame iron was imbedded in ſand and clay, and the apparatus was arranged, and the forge managed preciſely as in the laſt experiment. At the end of an hour, the crucibles being withdrawn from the

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* The quartzy ſand, called Lynn ſand, waſhed and heated red hot.--Stourbridge clay.

fire, and allowed to cool, I found that one end of the cylinder was ſlightly bliſtered; but otherwiſe had retained its ſhape and original qualities. The temperature was 152°.

I may here once for all obſerve, that I took care to examine whether the iron made uſe of was real ſoft iron, before it was ſubjected to experiment. I likewiſe, in every inſtance, reſerved a part of the iron, as a ſtandard with which I compared that on which the experiment had been made. In this I was aſſiſted by a ſkilful workman, who in my preſence made trials of the hardneſs and malleability of the metal when heated to various degrees of temperature, and alſo when cold, after having been ignited and plunged into water. Whenever therefore, in future, I mention ſoft iron as remaining unaltered after an experiment, I wiſh it to be underſtood, that it was ſubjected to theſe proofs of its malleability, and that it had the ſame qualities with the portion of ſoft iron that had been reſerved.

This reſult affords a proof, that iron expoſed to a heat of 150° during one hour, cannot be converted into ſteel without the aid of carbon; and that in the firſt experiment, the converſion into ſteel was ſolely to be attributed to the action of the diamond.

Mr. Muſhet's doubt, "whether the diamond afforded even one particle of carbon to the iron," is thus clearly removed. In the paper above-mentioned, he ſtates it as his opinion, founded on his own experiments, that when ſoft iron is expoſed to a high temperature, carbon diſſolved in caloric penetrates the crucibles, and the reſt of the apparatus in which it is encloſed, and converts the iron into ſteel. In order to determine how far this opinion was well founded, I made the following experiments:

My firſt object was to expoſe ſoft iron to ſtrong heat, in an apparatus which might abſolutely exclude all carbonaceous matter, except ſuch as might be ſo diſſolved. This I hoped to accompliſh by making uſe of compact crucibles, and, for greater ſecurity by the interpoſition of ſome ſubſtance between the iron and the crucibles.

On conſidering with Dr. Kennedy what ſubſtance would moſt effectually anſwer this purpoſe, we agreed that pure white felſpar (adularia) would be the beſt; for Dr. Kennedy, in the courſe of an analyſis of this ſubſtance, having found that it began to paſs into the ſtate of glaſs about 90°, we concluded, that before the heat was of ſufficient intenſity to melt the iron, it would be ſurrounded by a vitreous maſs perfectly free from carbonaceous matter; and, as we preſumed, impervious to the action of carbon from without. Another reaſon for uſing adularia was, that its action on crucibles is but ſlight, which would prevent any riſk of the experiment failing by their being melted.

For the proceſſes next to be deſcribed, I made ſome ſmall crucibles of the porcelain clay of Cornwall, and baked them in a ſtrong heat. Into one of theſe I put ſome felſpar in fine powder, upon which having placed a ſmall cylinder of ſoft iron, the crucible was filled with the felſpar, ſo that the iron was completely ſurrounded with it. On this crucible I luted a lid of the ſame clay, and placed it in a Heſſian crucible, which was filled up with powdered felſpar. Having alſo luted a lid on the Heſſian crucible, I attached to its outſide a ſmall clay caſe containing a pyrometer piece, and covered the whole with a coating of Stourbridge clay and quartzy ſand. The crucible and its contents were then placed in an air furnace, and the heat was raiſed gradually for an hour. During a ſecond hour it was continued at its higheſt pitch; after which the apparatus was removed from the fire [*]. The heat marked by the pyrometer was 152°.

The iron had retained its ſhape perfectly, and its properties remained unchanged. The felſpar was reduced to a glaſs, perfectly tranſparent and colourleſs, except that it had received a greeniſh tinge where it was in contact with the iron. This, I preſume, it derived from a ſmall portion of oxide formed on the ſurface of the iron, by the air unavoidably included in the crucibles.

Wiſhing to exclude the felſpar from the iron, I varied the proceſs by placing a piece of iron alone in a corniſh clay crucible, luting on a lid, and imbedding this in another crucible filled with felſpar. The heat was continued for an hour and a half. I found that the melted felſpar had penetrated into the interior crucible, and that part of it had reached the iron which ſtill remained unaltered.

In order effectually to prevent the felſpar from coming into contact with the metal, I next put a piece of iron into one of the ſmall corniſh clay crucibles, and inverted another of a larger ſize over it. The two crucibles were luted together with a mixture of Corniſh clay baked in a heat of 160°, and a little of the ſame clay in its raw ſtate. I placed them in a Heſſian crucible, ſurrounded them with fragments of felſpar, and luted on a lid as before. The heat was managed as in the former experiment, and continued in its greateſt intenſity for an hour and a half. I found the ſmall crucibles intire, and imbedded in the glaſs of the felſpar. The oxide of the iron had acted on the interior crucible, and formed a ſmall quantity of browniſh glaſs. The iron retained its ſhape and ſoftneſs, and poſſeſſed none of the properties of ſteel.

Mr. Muſhet is of opinion, that carbon diſſolved in caloric, penetrates but ſlowly through earthy bodies. It therefore occurred, that he might object to theſe reſults as unſatisfactory, becauſe I had not allowed ſufficient time for the carbon to find its way through the thick maſs of crucibles and felſpar in which the iron was imbedded. This I hoped to obviate by the following experiments.

Having placed in a Heſſian crucible a piece of ſoft iron, weighing about half a pound troy, I ſurrounded it with ſand. This crucible was included in two others of the ſame kind, and the ſpace between each being filled with ſand, lids were luted on, and the whole was placed in the reverberatory at Mr. Barker's Iron Foundery, and allowed to remain in its utmoſt heat for ſix hours. When taken out, I found that the aſhes carried up by the flame had vitrified a great part of the crucibles, ſo as to form two ſmall apertures in the interior crucible which contained the iron; but to my ſurprize I found the iron unaltered, except that it was ſlightly oxidated on the ſurface. The pyrometer was deſtroyed.

In a ſecond experiment with another piece of iron, I filled the interſtices between the crucibles with lute made of ſand and clay. The heat was continued for four hours only, at the end of which I found the outer crucible vitrified, but the interior ones intire. The pyrometer meaſured 153°, and the iron remained unchanged.

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[*] In all theſe experiments the fuel was coak.

Finding that under the circumſtances above-mentioned no carbon penetrated to the iron, I now reſolved to repeat Mr. Muſhet's firſt experiment, following exactly the proceſs which he has deſcribed; and as he does not inform us what kind of crucibles he uſed, I took Engliſh crucibles, which are leſs compact than the Heſſian.

Mr. Muſhet ſays, that he uſed ſand, obtained by pounding the ſtone of which the furnace hearths at Clyde Iron Works are made. I am unacquainted with this particular ſtone; but it is well known, that many ſand-ſtones contain coaly matter. I therefore made uſe of Lynn ſand.

Into an Engliſh crucible I put three ſmall cylinders of ſoft iron, and on this luted a lid made of ſand, with a very ſmall proportion of clay. I placed this in a larger crucible containing ſome ſand, and covered it up with the ſand, till the outer crucible was almoſt full. Having adjuſted a lid to it, I placed the whole in the air furnace. The fire was managed as in the former experiments, and the greateſt heat continued for one hour and a half, when the crucibles were taken out entire.

Part of the interior crucible, and the oxide of the iron, occaſioned by the included air had entered into fuſion, and formed a ſmall quantity of browniſh glaſs. The three pieces of iron adhered ſlightly together, and on thoſe parts which had not been in contact with the glaſs, there was a thin film of oxide, which eaſily ſcaled off: but the ſhape of the cylinders was not altered. On ſeparating them, I found that they retained all the qualities of ſoft iron. Their ſoftneſs muſt therefore have been ſuch as to unite them by a proceſs ſimilar to that of welding.

As Mr. Muſhet informs us, that in one of his experiments his crucible was cracked, I conceived that carbon might have found acceſs to the iron through the aperture in the form of carbonic acid, of flame, or of ſmoke. In order, therefore, to aſcertain how far the proceſs was affected by this circumſtance, I followed the detail of his experiment preciſely; uſing a cracked crucible, and ſand pounded from a ſand ſtone, in which, however, I could diſcover no traces of coaly matter. The heat in this experiment was continued for an hour and a half. The iron remained unchanged.

With the ſame view I made a ſecond experiment, which I imagined would be ſtill more deciſive. A ſmall ſhallow crucible containing a cylinder of ſoft iron, ſimilar to thoſe formerly uſed, was placed in a larger one, through the ſides of which I had bored three holes. In this inſtance, no ſand was introduced into either of them. Having adjuſted a lid to the outer crucible, I expoſed the whole to the greateſt heat that the forge could produce for forty-five minutes; after which the iron was found not to have received any alteration in its qualities.

In the experiments already deſcribed, the heat generally exceeded 150, but was not of ſufficient intenſity to melt the iron, and the reſults would ſcarcely have ſatisfied me, had I not found that ſoft iron, in one ſolid piece, in fragments, or in filings, may be perfectly fuſed without undergoing any change in its properties.

A cylindrical piece of ſoft iron, about an inch in length, and half an inch in diameter, after being placed on its end in a Heſſian crucible, and ſurrounded with quartzy ſand, was expoſed to a ſtrong heat in a furnace. The upper half of the cylinder loſt its ſhape and ſunk down: but the iron retained its original properties.

The fuſibility of ſoft iron is proved in a more ſatisfactory manner by the following experiments: five thin flat pieces of ſoft iron, making when laid together half a cubic inch, ſoftened into one ſolid maſs, which ſtill poſſeſſed all the properties of malleable iron.

I took two Corniſh clay crucibles, and in one placed a ſmall cylindrical piece of ſoft iron, which weighed only twenty-five grains, and in the other the ſame weight of filings of ſoft iron. Both were placed, together with a pyrometer, in a Heſſian crucible, on which a lid was luted, and the apparatus was expoſed to the heat of the forge for three quarters of an hour. Both the cylinder and the filings were melted into buttons. The heat was 158°.

Wiſhing to aſcertain whether oxide of iron would unite with ſoft metallic iron when in fuſion, and render it brittle, I mixed ſome iron filings with a tenth part, by weight, of ſcales of iron from a blackſmith's anvil, in the ſtate of fine powder, and put the mixture into a Heſſian crucible, included within a larger, which I placed in the forge, together with another containing ſome of the ſame filings without oxide, and a pyrometer. The heat was applied for three quarters of an hour, after which I found a ſolid button in each of the crucibles. The fuſion was ſo complete, that in the crucible containing the mixture of filings and oxide, ſome of the melted iron had penetrated through a very ſmall hole, and remained between the interior crucible and the exterior. The heat indicated by the pyrometer was the ſame as in the laſt experiment, 158°. The workman with whom I tried the malleability of theſe two buttons, ſaid, he could diſcover no difference between them, and that he thought they conſiſted of the ſofteſt and moſt ductile iron he had ever examined.

As the iron was here found to be melted at 158°, and as in preceding experiments in the furnace, and alſo in Mr. Barker's reverberatory, pieces of iron did not melt at 153°, it appears that the heat requiſite for reducing ſoft ductile iron to fuſion, exceeds 153°, but is not higher than 158°.

From the reſult of the foregoing experiments, I hope it is evident, that ſoft iron cannot be converted into ſteel, by carbon penetrating from the fuel through the crucibles. As Mr. Muſhet has not proved by humid analyſis, that his reſults contained carbon, I am inclined to think, that in certain circumſtances, iron may perhaps be combined with earths, ſo as to form a compound in ſome degree reſembling ſteel. The experiments publiſhed by Clouet ſeem to afford grounds for this ſuppoſition. One of theſe I have repeated, namely, the fuſion of iron filings with a mixture of carbonate of lime, and powdered crucibles, and obtained a button which became very hard, when heated red hot and plunged into water. But I did not ſucceed when I employed pieces of nails inſtead of filings; for the nails remained unmelted and ſoft. I propoſe to make ſome experiments on the fuſion of iron with earths, and to analyſe the products, in order to learn whether ſuch a combination can take place,

I ſhall conclude by relating a new experiment, to which I have ſubjected the diamond, by which the identity of this ſubſtance with carbon is ſtill farther confirmed.

Having prepared ſome pure oxide of iron from a ſolution of the ſulphate, by precipitation with cauſtic ammonia, I mixed a ſmall quantity of it with one fourth of its weight of diamond powder, prepared in the following manner:

The diamond being reduced to powder in a ſteel mortar, was boiled in muriatic acid, to diſſolve the iron which might have been abraded from it. After proper edulcoration, it was heated in a muffle to burn off the carbon of the ſteel which remained after treatment with the acid, and which rendered the powder of a grey colour. I obſerved the coaly matter take fire at the edge of the heap of powder next the ſtrongeſt heat, and gradually ſpread itſelf, till at laſt the whole appeared as if burning. The glow through the powder ceaſed ſoon after, and on removing it I found it perfectly clean and white. From the diminution of the original weight of the diamond, I found that a part of it had alſo been conſumed.

The mixture of oxide and diamond powder was put into a Corniſh clay crucible, and expoſed to a pretty ſtrong heat for half an hour, after which the oxide was found to be reduced into a metallic button of caſt iron.

Another portion of the oxide of iron uſed in this experiment, was not reduced when placed in the ſame circumſtances without the diamond.

III.

Account of certain Phenomena obſerved in the Air Vault of the Furnaces of the Devon Iron Works[*]*; together with ſome practical Remarks on the Management of Blaſt Furnaces: By Mr.* JOHN ROEBUCK, *in a Letter to Sir* JAMES HALL, *Bart*[†].

SIR,

I HAVE examined my memorandums, concerning the obſervations I made on the condenſed air in the *air vault* of the Devon iron works, near Alloa; and, according to your requeſt, I now tranſmit you an account of them; and alſo of an experiment I made, when a partner and manager of theſe works, in order to increaſe the produce of blaſt furnaces.

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[*] Theſe iron works are on the banks of the river Devon, which runs into the Frith of Forth near Alloa. They are 3 miles from Alloa, and 8 from Stirling.

[†] Communicated by him to the Royal Society of Edinburgh, and inſerted in the fifth Vol. of their Tranſactions.

It was omitted to be mentioned, that Sir James Hall's paper in our two laſt Numbers is inſerted in the ſame volume.

The two blaſt furnaces at Devon are of large dimenſions, each being 44 feet high, and about 13 feet wide in the boſhes, or wideſt part, and are formed on a ſteep bank, by two pits ſunk in a very ſolid ſtratum of coarſe grained freeſtone.

Theſe pits were afterwards ſhaped and lined in the uſual manner of blaſt furnaces, with common bricks and fire bricks, and the hearth was laid with large blocks of the ſtone that had been dug out, and which ſerve the purpoſe of fire ſtones. At the back of the two furnaces, next the bank, the air vault is excavated, and formed by a mine drove in the ſolid rock, diſtant from the furnaces about 16 feet. The bottom of the air vault is only about 4 feet higher than the level of the bottom of the furnaces. This vault has an aperture at one end to receive the air from the blowing machine, and has two at the oppoſite end, one of which receives the eduction pipe, and the other is a door to give admittance occaſionally into the vault. As the rock is extremely cloſe and ſolid, the vault is dry, except that a little water ouzes very gently from the ſide next the bank in ſmall drops, and does not appear to exceed an Engliſh pint in 24 hours.

Theſe furnaces are provided with air, or blaſt, as it is termed, by the means of a fire-engine of the old, or Newcomen's conſtruction. The diameter of the ſteam cylinder is 48¾ inches; and the ſquare area of its piſton being about 1866½ ſquare inches, the power of this ſort of engine cannot be rated at more than 7 lb. to the ſquare inch, amounting in all to about 13062 lb. This power was employed to work an air pump, or blowing cylinder, of 78 inches diameter, and about 7 feet long. The number of ſquare inches on the piſton of the air pump is 4778, and therefore this area, being multiplied by 2¾, will produce 13139, being a reſiſtance that nearly balances the above-rated power, and ſhows that the air, which was expelled from the air pump, could not be condenſed more in the ordinary way of working, than with a compreſſing power of about 2¾ lb. on each ſquare inch: As the engine was not regulated, at firſt, to make a longer ſtroke than about 4 feet 8 inches, only one furnace being uſed, the quantity of air expelled at each ſtroke of the machine was about 155 cubic feet, which it diſcharged through a valve into the air vault, about 16 times in a minute. When two furnaces afterwards were blown, the engine was regulated to work much quicker, and with a longer ſtroke. The air vault is 72 feet long, 14 feet wide, and 13 feet high; and contains upwards of 13,000 cubic feet, or above 80 times the contents of the air pump. The top, ſides, and bottom of this vault, where the leaſt fiſſure could be diſcovered in the beds of the rock, were carefully caulked with oakum, and afterwards plaſtered, and then covered with pitch and paper. The intention of blowing into the vault is to equalize the blaſt, or render it uniform, which it effects more completely than any machinery ever yet contrived for the ſame purpoſe. The air is conducted from the vault by the eduction pipe, of 16 inches diameter, into an iron box or wind cheſt, and from this it goes off to each furnace, in two ſmaller pipes that terminate in nozles, or blow-pipes, of only 2½ to 3¾ inch diameter, at the tweer of the ſurface.

When the ſurface was put in blaſt, after having been filled with coakes, and gently heated for more than ſix weeks, the keepers allowed it to have but little blaſt at firſt, giving it a ſmall blow-pipe of about 2¼ inch diameter, and likewiſe letting off a very conſiderable quantity of air, at the eſcape, or ſafety valve on the top of the iron wind cheſt, as it is a received though erroneous opinion among them, that the blaſt muſt be let on very gradually for ſeveral months. From the conſtruction of this valve, it was impoſſible to aſcertain the exact proportion of the blaſt they thus parted with, but I believe it was very conſiderable. The conſequence was, that the furnace, after it had been in blaſt for ſeveral days, never ſeemed to arrive at its proper degree of heat, but was always black and cold about the tweer in the hearth, and appeared in danger of choking, or gobbing, as it is termed.

After various experiments tried in vain, by the keepers and the company's engineer, and others, (indeed they tried every thing, except giving the furnace a greater quantity of air, which, as I afterwards aſcertained, was all that it wanted), they concluded, that the air vault was the cauſe of the whole miſchief; and, to confirm their opinion, they ſaid they had now diſcovered that water was, in conſiderable quantities, driven out of the air vault through the blow-pipe, which cooled the furnace; and they inſiſted, that the power of the engine was ſuch as to force water out of the ſolid rock; ſo that this method of equalizing the blaſt never would ſucceed. The other managing partner was ſo much alarmed by theſe repreſentations, that he began to conſult with the engineer, and others, about finding a ſubſtitute for the air vault at any expence.

As the plan of the blowing apparatus had been adopted at my recommendation, and was now ſo loudly condemned on account of the water, I had other motives, than mere intereſt, for trying to become better acquainted with the phænomena attending it. I accordingly determined to go into the air vault, and to remain incloſed in the condenſed air while the engine was blowing the furnace. It is an experiment that perhaps never was made before, as there never exiſted ſuch an opportunity. I could not perſuade the engineer, or any other of the operative people about the work, to be my companions, as they imagined that there was much danger in the experiment. Mr. Neil Ryrie, however, one of the clerks of the Devon company, had ſufficient confidence in my repreſentations to venture himſelf along with me.

The machine had been ſtopped about two hours previous to our entering the vault, and we found a dampneſs and miſtineſs in it, which diſappeared ſoon after the door was ſhut faſt upon us, and the engine began to work in its uſual manner. After four or five ſtrokes of the engine, we both experienced a ſingular ſenſation in our ears, as if they were ſtopped by the fingers, which continued as long as we remained in the condenſed air. Our breathing was not in the leaſt affected. I had no thermometer with me, but the temperature of the air felt to us the ſame as that without the vault. Sound was much magnified, as we perceived, when we talked to each other, or ſtruck any thing; particularly, the noiſe of the air eſcaping at the blow-pipe, or waſte valve, was very loud, and ſeemed to return back to us. There was no appearance of wind to diſturb the flame of our candles; on the contrary, I was ſurpriſed to find, that when we put one of them into the eduction pipe, which conveys the wind from the vault to the furnaces, it was not blown out. There was not the ſmalleſt appearance of any drops of water iſſuing out of this pipe. The ouzing and dropping of water from the ſide of the rock, next the bank, ſeemed the ſame as before the condenſation was made in the vault. In ſhort, every thing appeared, in other reſpects, the ſame as when we were in the common atmoſphere. Having remained about an hour in the condenſed air, and ſatisfied ourſelves that no water, during that time, that we could in the leaſt diſcover, was agitated and forced out of the rock and vault by the power of the blaſt, as was imagined and inſiſted on, we gave the ſignal to ſtop the engine. As ſoon as it ceaſed to work, and the condenſation abated ,and before the door of the vault was unſcrewed, *the whole vault, in a few ſeconds, became filled with a thick vapour, ſo that we could hardly ſee the candles at four or five yards diſtance.* The door being now opened, the work people, anxious to know our ſituation, and what had occurred, came into the vault, and prevented any further obſervations.

I now endeavoured to account for this curious appearance of the water, which only ſhewed itſelf occaſionally, in very ſmall quantities, at the tweer, at a hole I ordered to be made in the bottom of the wind cheſt to collect it more accurately, for it never was obſerved, but either when the engine, after working ſlowly, was made to work quicker, or, after having been ſtopped for a few minutes, was ſet to work again.

I conſidered the vapour which we had diſcovered in the vault to ariſe from the moiſture of the ſide of the rock next the furnace, which being expelled by the great heat of the furnace, and converted into vapour, was able to force its way through the pores of the rock into the vault, but that being in a manner confined within the rock, by the preſſure of the condenſed air, it found itſelf at liberty to come into the vault, only when the condenſation abated conſiderably, or was totally removed by the going ſlow, or ſtopping of the engine. It alſo occurred to me, that the air, in a ſtate of condenſation, might poſſibly be capable of holding a greater quantity of water in ſolution, which might precipitate ſuddenly into vapour or miſt when the condenſation abated. I imagined, therefore, that the very ſmall quantities of water we at times diſcovered, proceeded from nothing elſe but this vapour, in its paſſage to the furnace along with the blaſt, being condenſed into water, by the coolneſs of the eduction pipe and iron wind cheſt. The quantity of water did not appear to amount to a gallon in twenty-four hours.

A few days after I had made this experiment, the water ceaſed entirely to make its appearance, either at the tweer, or at the hole in the wind cheſt; but the furnace did not come into heat for a long while after, and indeed not till the keepers let much more air into it by a larger blow-pipe, and allowed leſs air to eſcape at the ſafety valve. It is probable that the rock was now become perfectly dry by the continued heat of the furnace.

My experiment had the good effect to remove all the prejudices againſt the plan I had adopted of blowing the furnaces, and likewiſe prevented the other partner from laying out a large ſum of money, by ſtopping the works, and altering the blowing machinery. Indeed, it has ſince been admitted, by all who have ſeen it at work, to be the moſt ſimple and effective method of equalizing the blaſt of any yet put in practice.

This experiment led me, ſome time afterwards, to apply a wind guage that I contrived, to aſcertain preciſely the ſtate of the condenſation of the air thrown into the furnaces. I found that a column of quick-ſilver was raiſed five inches, and ſometimes, though ſeldom, ſix inches, and, in the interval of the engine to receive air into the air pump, it fell only half of an inch. At this time only one furnace was worked. But when two furnaces were in blaſt, the engine only raiſed the mercurial guage about 4 inches, becauſe the Devon company, for certain reaſons, did not, while I continued a partner, think proper to allow the blowing machinery to be completed, by the putting to work their ſecond boiler of 20 feet diameter for the fire engine, according to my original deſign, which, by adjuſting the machinery, would have enabled us to blow two furnaces, with two boilers, with as much effect, in proportion, as one furnace with one boiler. This inſtrument had the advantage of enabling the work people to diſcover the real power of their blaſt, and know the exact condition of the air valves, and the gearing of the blowing piſton; for if theſe were not tight, and in order, (although the engine might, to appearance, be doing well, by making the ſame number of diſcharges of the air pump as uſual *per* minute), yet the wind guage would not riſe ſo high, and would ſhew that there was an imperfection ſomewhere, by reaſon of a quantity of air eſcaping at the valves, or piſton, that could not ſo eaſily otherwiſe be known. This contrivance was conſidered as of much uſe, and was afterwards always quoted in the company's journal books, to ſhow the actual ſtate of the blowing machine, in comparing the daily produce of the furnaces.

I hope you will not think me tedious, when I explain to you another experiment, which appears to me to be of conſiderable importance to all manufacturers of caſt iron.

I had reaſon to conjecture, from my own obſervations on the effects of blowing machinery on blaſt furnaces, as well as from the knowledge I had acquired from my father Dr. Roebuck, and from my communications with other experienced iron maſters, that a great part of the power of ſuch machinery was miſapplied in general practice, by throwing air into furnaces with much greater velocity than neceſſary, and that, if this velocity was, to a certain degree diminiſhed, the ſame power, by properly adjuſting the blowing machinery, of whatever nature, would be capable of throwing into the furnace a proportionally greater quantity of air. For, "*Since the quantities of any fluid, iſſuing* "*through the ſame aperture, are as the ſquare roots of the preſſure*;" it follows, that it would require *four* times the preſſure, or power, to expel *double* the quantity of air, through the ſame aperture, in the ſame time: but if the area of the aperture was doubled, then the quantity of air expelled by the ſame power, and in the ſame time, would be increaſed in the ratio of the ſquare root of 2 to 1, though its velocity would be diminiſhed exactly in the ſame proportion. Again: I conſidered that the quantity and intenſity of heat, produced in blaſt furnaces, and conſequently its effects in increaſing the produce, might be only in proportion to the quantity of air decompoſed in the proceſs of combuſtion, without regard to its greater velocity; that is to ſay, whether or not the ſame quantity of air was forced, in the ſame time, into the furnace through a ſmall pipe, or through one of larger dimenſions; for, in attending to the proceſs of a common air furnace for remelting of iron, where there is a very large quantity of air admitted through the large areas between the bars, it is well known, that a much greater intenſity of heat is produced than takes place in a blaſt furnace, and yet the air does not enter into the fire through the bars with increaſed denſity or great velocity. I therefore thought it probable, that increaſing the *quantity* of air, thrown into the blaſt furnace in a conſiderable degree, although the *velocity or denſity* might be much leſs, would have the effect of increaſing its heat, and operations, and produce. And as, from the principles above ſtated, with regard to the machinery, I ſaw I could greatly increaſe the quantity of air thrown into the furnace, by enlarging the diameter of the blow-pipe, and regulating the engine accordingly, without being obliged to employ more power, I was anxious to make this experiment.

A ſyſtem of management, of which I did by no means approve, was adopted by the other partners of the Devon company, ſoon after the works were begun to be erected; and, in the proſecution of it, they ordered their ſecond furnace to be put in blaſt, without permitting thoſe meaſures to be taken that were neceſſary to provide and maintain a ſufficient ſtock of materials; and alſo without allowing their blowing machine to be completed, according to the orignal deſign, by the addition of its ſecond boiler. As might have been expected, a trial of ſeveral months to carry on two furnaces, with only half the power of ſteam that was neceſſary, and an inadequate ſtock of materials, proving unſucceſsful, the company, as a remedy, inſtead of making up the above deficiencies, ordered one of the furnaces to be blown out, and ſtopped altogether. This improper meaſure, however, afforded me the opportunity of immediately putting in practice the plan I have mentioned.

When one of the furnaces was ſtopped, the other continued to be blown by a blow-pipe of 2¼ inches diameter, and the produce of the furnace, for ſeveral weeks thereafter, was not 20 tons of iron *per* week at an average. The engine at this time was making about 16 ſtrokes a minute, with a ſtroke of the air pump, about 4 feet 8 inches long; but when I altered the diameter of the blow-pipe, firſt to 3, and immediately after to 3¼ inches diameter, and regulated the working gears of the engine, ſo as to make a ſtroke of 5 feet 2 inches long, and about 19 ſtrokes in a minute, on an average, the produce was immediately increaſed. It continued to be, on an average of nine months immediately after this improvement, at the rate of 33 tons of iron *per* week, of as good quality as formerly; for during this period, from the 21ſt November 1795 to July 30, 1796, this one furnace yielded 1188 tons of iron. No more coals were conſumed in working the blaſt engine, or other expences about the blowing machine incurred, and therefore no more power was employed to produce this great effect. It is alſo of much importance to remark, that the conſumption of materials, from which this large produce was obtained, was by no means ſo great as formerly. The furnace required very conſiderably *leſs fuel, leſs ironſtone, and leſs limeſtone,* than were employed to produce the ſame quantity of iron by the former method of blowing; and according to the ſtatements made out by the company's orders, as great a change was effected in the œconomical part of the buſineſs.

From the ſucceſs of this experiment, ſo well authenticated, and continued for ſeveral months, I am led to be of opinion, that all blaſt furnaces, by a proper adjuſtment of ſuch machinery as they are provided with, might greatly and advantageouſly increaſe their produce, by aſſuming this as a principle, viz. "*That with the giving power it is rather by a great*" *quantity of air thrown into the furnace, with a moderate velocity, than by a leſs quantity*" *thrown in with a greater velocity, that the greateſt benefit is derived, in the ſmelting of iron-*" *ſtones, in order to produce pig-iron.*" However, it is by experiment alone, perhaps, that we can be enabled to find out the exact relations of power, velocity, and quantity of air requiſite to produce a *maximum* of effect *.

But, an unfortunate diſagreement among the partners of the Devon company, put it out of my power to make further progreſs in this matter, by laying me under the neceſſity, two years ago, of withdrawing myſelf entirely from the concern.

I have the honour to be, reſpectfully,

SIR,

Your moſt obedient ſervant,

Edinburgh, June 30, 1798. JOHN ROEBUCK.

To Sir James Hall, Bart.

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* If Q be the quantity of a fluid, iſſuing in a given time through an aperture of the diameter D, V its velocity, and P the power by which it is forced through the aperture: then the area of that aperture being as D^2, the quantity of the fluid iſſuing in the given time will be as VD^2, or $VD^2 = Q$.

Again, this quantity multiplied into its velocity, will be as the *momentum* of the fluid expelled, or as the power by which it is expelled, that is, $V^2D^2 = P$, or $VD = \sqrt{P}$.

Here, therefore, if D is given, V is as $\sqrt{P}$, as Mr. Roebuck affirms. Alſo, becauſe $V = \frac{Q}{D^2}$, and alſo $V = \frac{\sqrt{P}}{D}$, $Q = D\sqrt{P}$, ſo that, while P remains the ſame, Q will increaſe as D increaſes, and V will diminiſh in the ſame ratio.

The problem, therefore, of throwing the greateſt quantity of air into the furnace, with a given power, ſtrictly ſpeaking, has no *maximum*, but the largeſt aperture of which the engine can admit muſt be the beſt. It is probable, however, that there is a certain velocity with which the air ought to enter into the furnace; this will produce a limitation of the problem, which, as Mr. Roebuck ſuggeſts, is not likely to be diſcovered but by experiment. J. P.

In order to illuſtrate what is ſaid above, a ground plan of the air vault and furnaces of the Devon Iron Works is given in Plate V.; of which the explanation follows:

Explanation of Fig. 1. Plate V.

A	The air vault, formed by a mine drove in the ſolid rock of coarſe grained freeſtone.
B	The blowing cylinder.
C	The pipe that conveys the air from the blowing cylinder to the air vault.
D	The eduction pipe that carries the air from the air vault to the iron wind cheſt.
E	The iron wind cheſt, (about 2½ feet cube), in which is inſerted a wind-gauge, repreſented in fig. 3.
F F	The two blow-pipes for each furnace, which terminate in apertures of 3¼ inches diameter at the tweers of the furnaces.
G G	The two blaſt furnaces, placed in two pits ſunk in the ſolid rock.
H H	The timps of the furnaces from whence the caſt-iron is run off into the caſting room, L L.
O	The door to give occaſional admittance into the air vault.
M	The excavation, in which is placed the blowing machine.

Explanation of Fig. 2.

A	The end of the wind-guage, (about 12 inches long), which is open to the atmoſphere, being half filled with quickſilver.
B	The end that is inſerted in the iron wind cheſt, and expoſed to the preſſure of the condenſed air of the air vault.

IV.

Apparatus for making the Hydraulic Engine at Schemnitz work itſelf without Attendance. By Mr. John Whitley Boswell.

To Mr. NICHOLSON.

SIR,

May 7, 1800.

HAVING laſt ſummer met with a deſcription of an engine, ſimilar in its principles to the hydraulic machine of Schemnitz, (of which there is an account in the Number of your Journal[*], in which you did me the honour to inſert my communication of a new ventilator); I was induced to contrive a method for making it work itſelf, as well from the utility of ſuch an invention, as from its being obvious at firſt ſight that it was very practicable.

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[*] Vol. IV. p. 18.

From the intereſt you ſeem to take in the above-mentioned engine, I conclude it will not be unacceptable to you to have a deſcription of this method of ſaving the labour at preſent employed in working it, and therefore take the liberty to ſend you a drawing, in which all the veſſels are ſuppoſed to be open at one ſide, that the diſpoſition of the pipes within may be more clearly ſeen.

Simplicity of form, and facility of execution, have been chiefly aimed at in this conſtruction; and I have no doubt but it muſt perfectly anſwer the intention, or I would not trouble you with it. If, however, you ſhould be of a different opinion, it will give me pleaſure to be ſet right by you.

DESCRIPTION: Plate VI.

A. The reſervoir, or upper level of water.

B. A chamber made of ſufficient ſtrength to bear the internal preſſure of a column of water of the height of A above it, multiplied by its own baſe.

C. A chamber of the ſame ſtrength as B, but of a ſmaller ſize; it is placed at the bottom of the pit from which the water is to be raiſed, and under the level of the water.
These chambers would be ſtronger with the ſame materials, if of a globular or cylindrical form; but the ſquare ſhape is uſed in the drawing, merely for the facility of repreſenting the poſition of the parts.

D. A pipe, from the reſervoir A which paſſes through the top of B, and ends near its bottom, to convey water from A to B.

E. A pipe from the top of B to the top of C, to convey air from B to C.

F. A pipe from the bottom of C to the level of the ground at the top of the pit, to carry off the water from the pit.

G. A pipe from the bottom of B to carry off the water from it.

H. A veſſel to contain the water uſed in working the cocks; it is only placed on the top of B to ſave the conſtruction of a ſtand on purpoſe for it.

I. A cock, or moveable valve, (worked by the lever there repreſented,) in the large pipe D.

K. A ſtop cock in the ſmall pipe which conveys water from D to H. Its uſe is to make the engine work faſter or ſlower, by letting water more or leſs quick into H; or to ſtop it altogether from working when required.

L. A moveable valve, or cock in the ſmall pipe L K. The lever which works it is connected by a ſtrong wire with the lever which works I, and is balanced by a weight at its oppoſite extremity, ſufficient to open both thoſe cocks and ſhut N, when not prevented by a counter weight.

N. A cock in the pipe G to open and ſhut it as wanted.

O. A ſelf moving valve in the pipe F, which permits the water to paſs upwards, but prevents its return.

P. A ſelf-moving valve at the bottom of C, which permits the water to paſs into C, but prevents any from paſſing out of it; it is furniſhed with a grating to prevent dirt getting in.

R. A veſſel ſuſpended from the levers of I and L, capable of containing a weight of water ſufficient to ſhut them.

S. A veſſel ſuſpended from the lever of N, it muſt contain water enough by its weight to open N: it is connected by a chain to R, to keep it down as long as N is open.

T. A ſyphon paſſing from the bottom of H, near its upper edge, and down again to the mouth of R.

V. A ſelf moving valve, of a ſufficient levity to riſe, when the water in B comes up to it, and cloſe the pipe E; into which no water would elſe paſs from B. A ball-cock, ſuch as uſed in common water ciſterns, would alſo do here.

X. A ſyphon from the bottom of R riſing within an inch of its top, and paſſing down again to the mouth of S.

Y. A ſmall pipe at the bottom of S; this may have a ſtop-cock to regulate it, which when ſtopped will alſo ſtop the engine.

EXPLANATION.

The mode of this engine's working is as follows: ſuppoſe the veſſels V H R and S empty of water, and the cocks K and Y open, and the veſſel C full of water. The weight on the lever of L will then open the cocks L and I, on which the water from A will flow into B and H. As the water riſes in B it will force the air through E into C, which ſtrongly preſſing on the water in C will force it up through the pipe F, till the water in B riſes to the level of V and cloſes it, at which time H will be full of water, (the quantity flowing in being ſo regulated by the cock K) and the water will flow from it through the ſyphon T into the veſſel R, which as it fills ſhuts the cocks I and L, and prevents any more water coming into B and H. When R is full, the water flows through its ſyphon X, which fills S, and by it opens N, which empties B of water, and keeps N open as long as there is any water in H.

When H is empty, B will be ſo too (being ſo regulated by the cock K) on which in a moment or two R and S will alſo be empty; which will cauſe the cocks I and L to open, and all things be again in the ſtate firſt ſuppoſed, for a repetition of the operations deſcribed.

To ſtop the engine the cocks at K and Y ſhould be ſhut, while S is full of water. To ſet it working they ſhould be opened, and this is all the attendance it will require. As no one but an engineer ſhould attempt to conſtruct ſuch an engine as this, it was uſeleſs to repreſent the manner of connecting the pipes by flaches, or otherwiſe, or the proper methods of faſtening and cloſing all parts, which are all well known to ſuch as have made this art their ſtudy. If this method of effecting the purpoſe ſhall be eſteemed worthy of being adopted by any of them, it will give me great pleaſure to have contributed to the improvement of an engine ſo uſeful, where it can be adopted.

Your very humble Servant,

JOHN WHITLEY BOSWELL.

P. S. I beg leave to requeſt that an error of the preſs in the paper on Ventilators, may be corrected, page 7, line 16 and 17, for "and this way of uſing it might be made" read "and *for* this way of uſing it, *it* might be made."

I alſo will thank you to notice, that my ventilator was conſtructed in March 1798, before Venturi's book came to England, which I underſtand was in November 1798. Mr. Bonnycaſtle, of Woolwich, to whom I ſhewed it, and its mode of action at that time, can no doubt recollect this circumſtance.

J. W. B.

V.

On the Chemical Action of the different Metals upon each other at the common Temperature of the Atmoſphere, and upon the Explanation of certain Galvanic Phenomena. By M. Fabbroni[*].

THE phenomenon ſpoken of by Sultzer, in his *Theorie des Plaiſirs*, publiſhed in 1767, has been claſſed among galvanic phenomena, that is to ſay, the myſterious ſenſation which is manifeſted on the tongue by the contact of two metals in mutual contact, which would have excited none if they had been ſeparately applied to that organ. In fact, I have been perſuaded, that this very principle which produces an unexpected taſte in this caſe, may alſo produce a convulſive contraction in the animal fibre, when it comes to touch the irritable and ſenſible parts uncovered at the ſame time. But ſo far from attributing theſe effects with all the world to an agent almoſt unknown, ſuch as the electric fire, I immediately ſuppoſed them to depend upon a mere chemical operation, in the ſame manner as probably the ſenſe of taſte itſelf is produced, which renders the mechaniſm much more intelligible. I made reflections, and inſtituted experiments on this curious ſubject, of which I gave an account to the Academy of Florence in 1792. The volume has not yet been printed; but I think the ſubject is ſpoken of by Brugnatelli in his Journal. I have neither his abridgment, nor my own memoir at hand; I ſhall therefore repeat in this place nothing but what remains clearly in my recollection.

I had before obſerved on many occaſions, that running mercury retains its metallic ſplendor a long time while alone and uncombined; but that its amalgam with any other metal whatever is ſoon tarniſhed or oxided, and increaſes in weight in proportion to that progreſſive change.

I have kept, for many years, fine tin without any alteration in its argentine appearanee, but this was not the caſe with regard to the different alloys I had made with the ſame metal for economical ſpeculations.

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[*] Tranſlated from the Journal de Phyſique VI. new Series, p. 348.

I had ſeen in the muſeum at Cortona, Etruſcan inſcriptions engraved upon pure lead, which are ſtill in perfect preſervation, though of the moſt remote antiquity; and on the contrary, I had found with ſurprize in the gallery of Florence, medals of lead of the different pontiffs, in which tin, and perhaps arſenic had been mixed, to render them more beautiful and ſolid, were intirely reduced to a white powder, or changed into oxide, notwithſtanding they had been wrapped in paper, and kept in drawers.

I had likewiſe obſerved, that the alloy made uſe of in ſoldering the plates of copper which cover the moveable roof of the Obſervatory at Florence, was ſpeedily changed, and converted manifeſtly into a white oxide at its extreme contact with that metal.

Laſtly, I had learned in England, that the iron nails formerly uſed in attaching the copper to the bottoms of ſhips, corroded this laſt metal ſo much by their contact, that the holes ſoon became dilated, ſo as to exceed the ſize of the head of the nail itſelf.

Theſe facts appeared to me to be amply ſufficient to ſhew, that the metals exerciſe a mutual action in this caſe, and that the cauſe of the phenomena produced by their union, or contact, was to be attributed to this action.

It is known, that the metals are in general ſuſceptible of combining together by mutual ſolution. We may therefore form a notion, that their tendency to mutual combination begins as ſoon as the particles are brought into contact. It is only by virtue of the immenſe ſuperiority of their force of coheſion, that they are prevented from mutual penetration and ſolution in the cold. Fire is neceſſary to diſunite and give mobility to their particles. We ſee this happen in amalgams which are formed without heat; and it is known, that in the manufacture of tinned plates, the tin penetrates the iron without this laſt metal being liquified. It is, probably, the ſame force of coheſion which ſometimes prevents the oxidable metals from attracting oxigen with ſpeed. If a rapid movement tends to diſaggregate the particles of mercury in the midſt of water, nothing more is required to enable it to aſſume the principle of oxidation in a very ſhort time, by attracting it from the fluid. Theſe facts, as well as many others of the ſame nature, no leſs common than well known, ought to have proved to philoſophers, that the metals by exerciſing their mutual attractive force, muſt by the ſame energy diminiſh their reſpective powers of aggregation; that though neither of them ſeparately may be able ſeparately to attract oxigen from the atmoſphere, or from water, they may acquire that power by ſimple mechanical touch, as they paſs to new combinations. We might therefore ſuſpect, that ſome, at leaſt, of the effects produced on the bodies of animals, by the application of metallic coatings to the nerves and the muſcles, may be attributed to a chemical operation; to the tranſition of oxigen into a combination; to the formation of a new compound; or to the developement of a ſoluble or ſapid taſte, which is ſo perceptibly manifeſted on the organ of that ſenſation.

Galvani, Aldini, Volta, and other philoſophers equally ſkilful, who have ſo ſucceſsfully directed their attention to this kind of reſearch, not reflecting that the chemical action exerts itſelf with the ſwiftneſs of lightning, and, ſurprized at the ſuddenneſs with which the two different metals exert their effects on the animal fibre, were of opinion, that it could only be attributed to the electric fluid. The tranſmiſſion of galvaniſm to a diſtance, and by a circuit, favored their notion, which was generally received, notwithſtanding the very ſtrong objections which might, in ſome caſes at leaſt, be oppoſed to their ſyſtem. In fact, ſome ſigns of electricity have been obſerved on the ſeparation of two metals, which had been before placed in contact; but it is very well known, that even ſeveral chemical operations are conſtantly attended with a change of equilibrium in the electric fluid, and conſequently by perceptible marks of electricity. Thus it is that flaſhes of lightning are ſeen in the great volcanic fuſions or eruptions; and this alſo is one of the caſes in which philoſophers have taken that for the cauſe of theſe fires, which is merely one of their effects. The ſigns of electricity may be produced by melting a ſmall quantity of ſulphur, or of chocolate, or even by boiling or evaporating water; but aſſuredly theſe are fuſions, and are not cauſed by the electricity. I do not pretend to exclude all electrical influence from the prodigious effects of galvaniſm. I propoſe merely to prove, that this principle is not concerned in the phenomena of Sultzer, and that various other ſimilar facts are derived from the ſame ſource.

The metals having a mutual affinity, their particles muſt mutually attract each other when they are brought into contact. We cannot eſtimate the magnitude of this force; but I think it is ſufficient to weaken the force of their aggregation, ſo as to diſpoſe them to engage in new combinations, and to yield more readily to the action of the weakeſt ſolvents.

I had obſerved in repeating the experiment of Sultzer, that if I wiped my tongue as accurately as poſſible, the ſenſation excited by the approach of the two metals in contact was diminiſhed, ſo as to be hardly diſtinguiſhed. The ſaliva, or ſome other moiſture, muſt therefore be of ſome importance in this phenomenon. But to aſcertain the truth of my ſuppoſition, I placed in different goblets filled with water,

1. Separate pieces of metal; for example, in the one gold; in another ſilver; in a third copper; and in others tin, lead, &c.

2. In other ſimilar goblets I put the ſame metals as before, but two pieces in each goblet, one of which metals was more, and the other leſs oxidable. They were ſeparated from contact by a ſmall ſlip of glaſs.

3. Laſtly, I placed in other goblets metals of different kinds, but two in each, and in immediate contact. The two firſt ſetts exhibited no perceptible change, but in the laſt the moſt oxidable metal was viſibly loaded with oxide, a few moments after having been in contact with a different metal. This oxide gradually increaſed, ſo as to hang beyond the lower metal, uniting with it in a maſs, and flowing in a caſcade along the whole of the ſides. This phenomenon commences, though inſenſibly, at the very inſtant of contact; but I left the metals of this experiment for a conſiderable time, to ſee what would happen. I examined them at the end of a month, and found that the two metals had contracted ſo conſiderable an adheſion, that in order to detach a piece of braſs about two centimeters broad from a plate of tin, a force of no leſs than two kilogrammes was neceſſary.

I afterwards obſerved, that ſeveral metals were not only charged with oxide, but that ſmall ſaline cryſtals of different figures were formed. It appeared therefore to me, that an evident chemical action had taken place, and that it was unneceſſary to ſeek farther for the nature of the new ſtimulus, which in the experiment of Sultzer has been called galvaniſm. It was manifeſtly a combuſtion or oxidation of the metal; the ſtimulating principle might therefore be either the caloric, which is diſengaged, or the oxigen, which paſſes to new combinations; or laſtly, the new metallic ſalt: but which of theſe it may be I have not well aſcertained. I have in ſome inſtances coloured the water, in which I placed the metals with tournſol; but I have remarked no other effect, than a precipitation of that colouring fecula, without any change of its colour. I have remarked, that the water in which this experiment is made, contracts a ſlight metallic, or as I may ſay, arſenical taſte; which laſts for ſome time, and produces a diſpoſition to ſpit; but it does not appear to contain enough of metal to be detected by the moſt ſenſible chemical re-agents. I have therefore thought it proper to limit my concluſions reſpecting this phenomenon, that it is merely a ſlow combuſtion of the metal, which muſt be accompanied with an attraction of oxigen, and a developement of light and caloric. It is well known that when a metal, ſuch for example as gold, is amalgamated with mercury, there is an immediate expulſion of caloric, not perhaps in conſequence of the ſolid ſtate of the mercury, but becauſe the diminution of the force of aggregation in the particles of the latter metal facilitates its combuſtion. The progreſſive augmentation of weight which is obſervable in amalgams, ariſes only from the oxigen which they attract from the atmoſphere. I attempted in vain to meaſure the quantity of caloric, which is developed by the contact of two ſolid metals, whatever may be their weight. This quantity is too ſmall and too diffuſed, as it were, over a large ſurface, to be aſcertained by inſtruments of ſo little delicacy as thoſe we poſſeſs. Nevertheleſs, the light which flows from this metallic combuſtion, may be ſeen if the eye forms part of the apparatus. For example, it is only requiſite to hold a piece of ſilver in the mouth, and apply a ſmall piece of tin upon the ball of the eye; as ſoon as theſe two metals are made to communicate directly, or even by means of a third metal, a faint, but very diſtinct light is perceived, which is not an electric ſpark, nor a convulſive irritation. For though this light appears to affect the organ at the firſt inſtant only, becauſe the eye ſoon becomes accuſtomed to this weak ſenſation, we may be aſſured, that the emanation of light is continual in this caſe, by ſliding alternately the tranſparent and the opaque cornea againſt the metal, when it will be ſeen that the light is ſtronger whenever the metal is touched by the more tranſparent part of the organ. And again, if this experiment be made as it ought in the dark, nothing more will be neceſſary, than to attend at the time when the communications between the two metals is interrupted, and the obſcurity will be more deep and perfect, which is a proof of the conſtant preſence of ſome light before that interruption. I do not ſpeak of that kind of flaſh which ſome perſons affirm they have ſeen, by applying the two metals ſimply to the tongue and the gums, without the eye forming part of the circuit. I have not been able to verify this fact on myſelf, and have remarked, that ſeveral individuals have affirmed, that they ſaw what others could not perceive, and that the action in all was a convulſive ſenſation, or deluſive appearance, like the light which is ſeen when the eye is preſſed with the finger, or when a blow is given in the vicinity of that organ. It appears, therefore, that the ſenſation of taſte, and the emanation of light, are in this caſe the reſults of a chemical operation. But thoſe who have been deſirous of attributing the whole to electricity, have not been in want of plauſible obſervations to juſtify their hypotheſis. It has been remarked, for example, that the ſenſation here deſcribed, is felt even when the communication is made by a chain, or a long metallic conductor. But it is known, that electricity is propagated by this means to an indefinite diſtance; and I have obſerved, that ſix or ſeven metres are the extreme limit to which the manifeſtation of the metallic action on the tongue or the eye can be extended. It is certainly at the preciſe point of contact of the two metals, that their mutual action is the ſtrongeſt, and it is natural to think, that the particles which are the moſt affected, muſt communicate from the one to the other in their vicinity, to a certain point, the diſpoſing force which they have received, It muſt propagate itſelf with diminiſhed force, like the circles impreſſed in ſtagnant water by the fall of a body; and the limit of its action is nearly that which I have pointed out.

By varying my experiments in ſeveral manners, I obſerved, that if I covered the water in which the two metals in contact were placed, with a thin coat of oil, the oxidation was very ſlight, and ſtopped at a certain point. But this aſſuredly did not ſo happen, becauſe the intervention of a non-conducting fluid had oppoſed the completion of an electric phenomenon, as it may appear at firſt ſight; for I attempted to keep up the communication of the water and the metals with the common ſtock, by plunging a metallic conductor beneath the oil, and the combuſtion was not continued more than before. The ſame interruption or limitation obtains, if the free contact of the atmoſphere be excluded, by means of a ſmall bell glaſs reverſed over mercury, which does not oppoſe the paſſage of electricity. Beſides which, the Galvaniſts believe, that their phenomena do not depend on univerſal electricity, but on the ſpecific electricity, as it is called, of the different metals. If this were the caſe, it would be difficult to conceal, why the effect ſhould not be produced at the very inſtant of contact, in the ſame manner as it happens when two charged bottles of oppoſite electricities are brought into contact. And again, nothing would prevent the continuation of the phenomenon when the two metals came into contact, whatever might be the circumſtances. I am well aware, however, that if it may here perhaps be objected to me, that if the two metals acquire the faculty of decompoſing water by their ſimple contact, and the mere diſpoſition of their attraction, or mutual affinity, a ſlight coat of oil, or inverted glaſs, ought not to oppoſe the continuation of their complete oxidation, while they continue ſurrounded by that element. I have remarked, that the free contact of the atmoſphere is neceſſary to this phenomenon; becauſe it is neceſſary that the water ſhould contain that portion of oxigen gas, which it always holds after having remained for a certain time in contact with the air. It is neceſſary in my apprehenſion, that the hydrogen of the water ſhould be in a ſtate of quartation, or overcharged with oxigen, in the ſame manner as gold with ſilver, in order that the ſolution or parting may take place. The contact of the atmoſphere is therefore neceſſary to the water in the goblet, in order that it may re-aſſume the neceſſary ſtate of quartation, by abſorbing oxigen gas from the atmoſphere, in proportion as the oxidation of the metal ſeizes the radical of that principle.

If the experiment be made in a calm air, a kind of pellicle may be obſerved at the ſurface of the water, immediately over, and of the ſame figure and ſize as the metal, beneath which ſtreams even indicate the points of the ſurface and columns of the water, through which the atmoſpheric oxigen has been tranſmitted. This re-abſorption is ſo true, that if in ſome caſes a metallic oxide, well charged with oxigen, be ſubſtituted inſtead of the contact of the atmoſphere, the combuſtion of the metal under experiment is effected with facility. It is known that iron decompoſes water without addition, though very ſlowly; but if the red oxide of lead be added at the bottom of the water, the iron becomes changed into the black oxide without decompoſition of the water.

I have obtained, though after a very long time, the oxidation of tin in water in contact with ſilver, in a bottle of flint glaſs filled with water, and almoſt hermetically cloſed. But I have remarked, that the lead which enters into this kind of glaſs, yielded its oxigen to the tin, and became changed into a black opaque oxide, in the ſame manner as happens when a bottle of flint glaſs, filled with hydrogen gas, is made red hot in the midſt of charcoal. The hydrogen is burned, and ſeizes the oxigen of the lead, which it revives, as the tin operated in the caſe before-mentioned.

It appears evident, therefore, that the experiment of Sultzer is nothing more than a combuſtion or chemical operation, as is proved not only by its reſult, but its duration. For electricity acts always inſtantaneouſly, whereas the effects of the chemical affinities continue as long as the re-agents are not ſaturated. I left for a very long time in water, pieces of ſilver wrapped in ſeveral folds of tin foil. I took out ſome of them at different periods, and found the progreſs of the combuſtion exactly proportional to the time. In thoſe which I took out the laſt the tin was corroded, and pierced through all its folds, as if it had been plunged in an acid. But if other proofs be required, to ſhew that electricity has no part in the phenomenon in queſtion, we might vary the experiments, ſo as not to hinder the effects of the electric fluid, and evidently to prove by the eyes, that the combuſtion which happens depends on the diſpoſition of the metals, and their chemical affinity.

For example, if a piece of tin of conſiderable thickneſs be placed on the eye, and touched at its oppoſite ſurface with a bar of ſilver, there is no decompoſition of water, nor combuſtion, nor light, and yet the contact of the two metals ought to produce theſe ſenſible effects, if they depended on the communication of their electricity. 2. If a piece of tin be held upon the eye, and another piece in the mouth, and a communication be made between them by a bar of ſilver, there is no more appearance of light than in the former experiment. 3. If a piece of gold be applied on the eye, one of ſilver on the tongue, and a com-a communication be made with an iron key, there is no more appearance of light than in the former inſtances. 4. Neither does this effect take place if the iron be applied to the eye, and the tin to the tongue, with a communication between them. 5. Gold and ſilver ſeparately applied to theſe two organs, give hardly any ſenſation by their contact. 6. The ſame event takes place if two pieces of ſilver be uſed, and the communication be made by iron. 7. And likewiſe if copper be placed on the eye, tin on the tongue, and a communication be made by iron. 8. Neither is the ſenſation more evident if the ſilver be placed on the eye, the gold on the tongue, and copper be the medium of communication. 9. On the contrary, a conſiderable light is ſeen if the iron touches the eye, the ſilver the tongue, and the copper forms the communication. 10. Or if gold be ſubſtituted inſtead of ſilver. 11. Or if the communication between the iron on the eye, and the gold on the tongue, be made by means of a ſilver ſpatula. 12. Or if the iron on the eye, and the ſilver on the tongue, communicate directly. 13. Or if the order of theſe two metals be reverſed. 14. Or if gold be uſed inſtead of ſilver. 15. And laſtly, the ſame light of the combuſtion may be ſeen, if inſtead of placing one of the metals on the tongue, both be placed on the eyes.

By theſe experiments, which are all that I recollect at preſent, and may very eaſily be repeated and varied in different manners, we ſee that it is not electricity which produces the reſults. For it is well known, that the electric fluid thoroughly and inſtantaneouſly penetrates all the metals which moſt eminently conduct it, whatever may be their ſituation or relative poſitions.

But if it be true that the water affords the oxigen to the metal in theſe caſes, it may be demanded, what has become of its hydrogen. It muſt be immediately obſerved, that on account of the contact of the atmoſphere affording oxigen gas in proportion as the metal conſumes it, very little of the water muſt be decompoſed.

I have remarked, that I left the different metals for a long time in contact together, at the end of which I examined them, and I not only found them to be abundantly oxided, but I alſo found regular aluminiform ſaline cryſtals, adhering particularly to the pieces of ſilver, and ſalts of a very determined figure, conſiſting of two tetrehedral pyramids attached at their baſe, which appeared to me to be hydrogenated tin.

It is already known that hydrogen diſſolves ſeveral metals; for in hydrogen itſelf are found iron, zinc, arſenic, &c. It is known, that the amalgam of zinc and mercury contains hydrogen, which may be driven off by heat.

I muſt add, that ſometimes inſtead of placing my apparatus of tin and ſilver in water, I have left it a long time in alcohol, and found parallellipipedic cryſtals upon the ſilver, which were very tranſparent, and appeared to contain copper by their light greeniſh colour. This copper probably came from the ſilver; for in general I gave the preference to crown pieces, becauſe I obſerve, that the irregularities on their ſurface occaſioned by the impreſſion, was greatly favorable to the formation of cryſtals which lodged in theſe cavities, and round their edges. I attempted to place the ſame metals in ammoniac incloſed in a cryſtal bottle, but without any remarkable effect; probably becauſe the combination of the hydrogen is too ſtrong, and the oxigen of the atmoſphere could not unite with it in the decompoſition of the metal. The ammoniac merely took a ſlight blueiſh colour, which ſhews that it had diſſolved a portion of copper from the ſilver piece.

We ſee very clearly, from the reſults which I have obtained from the ſimple contact of the two metals, namely, the oxide and the ſaline cryſtals, that the ſenſations experienced on the tongue, and on the eye, are the conſequence of a chemical operation: and it therefore appears to me, that it is to theſe new compounds, or their elements, that we are to attribute that myſterious ſtimulus, which produces convulſive motions of the animal fibre in a great part, at leaſt, of the phenomena of galvaniſm.

VI.

Report concerning the Art of making fine Cutlery.—W. N.

THE fabrication of edge tools is one of the firſt arts among men in every ſtate of ſociety. Artizans are well aware of the neceſſity, that the inſtruments of their reſpective trades ſhould be made to poſſeſs the qualities adapted to the operations by which they gain their ſubſiſtence; and among the various ſub-diviſions of labour, there is perhaps no material, upon which the ſkill and judgement of practical men are more multifariouſly exerciſed than ſteel. The makers of files, of chiſſels, of planes, ſaws, and the infinite variety of knives, all occupy their ſeveral departments ſeparate from each other, and poſſeſs their reſpective degrees of celebrity among workmen, which are grounded on their knowledge of the peculiar kinds of ſteel, as well as the methods of working them, which are beſt ſuited to the intended operations. Many of theſe methods are kept ſecret; but in general the philoſophical enquirer will find the communications of operative men, to the full, as liberal and open as the circumſtances of the caſe may ſeem to warrant. Many manufacturers have no reſerve with regard to the manipulations of their art, and have the ſpirit to aſſert their claims to public encouragement, upon the open ground of the addreſs and integrity with which they conduct their profeſſional labours.

Among the inſtances of this kind which have occurred to me during a life of diligent enquiry, I have lately been much gratified by the ready aſſiſtance and communications of Mr. Stodart of the Strand, which enable me at preſent to communicate my own notions on the ſubject of fine cutlery, with the advantage and ſupport of his ſucceſsful experience; which I ſhall proceed to do without farther preface.

It appears to be at preſent generally agreed, that for all works which do not require welding, caſt ſteel is preferable to any other. For fine cutlery it undoubtedly is. Mr. Stodart uſes thoſe bars which are marked Huntſman, but does not ſuppoſe it to be of a better quality than that of Walker, and other manufacturers. He complains, that it is much worſe in quality now than formerly, which complaint I have alſo heard from other intelligent artiſts. I did not aſk him concerning the art of forging, but take it for granted, that it conſiſts in little more than the acquired ſkill of managing the bar and the hammer, with the precautions not to injure the texture by ſtrong hammering at too low a heat, or to degrade the quality of the ſteel, by too much heat or expoſure to the current of air from the bellows.

Cutlers do not uſe any coating to their work at the hardening heat, as the file cutters do; and indeed it ſeems evidently unneceſſary when the article is intended to be tempered and ground. Mr. S. agrees with me, that the beſt rule is to harden as little as poſſible above the ſtate intended to be produced by tempering. Work which has been overheated has a crumbly edge, and will not afford the wire hereafter to be deſcribed. The proper heat is a cherry red viſible by day-light. He has not found that any advantage is obtained from the uſe of ſalt in the water, or cooling that fluid, or from uſing mercury inſtead of water; but it may be remarked, that queſtions reſpecting the fluid are, properly ſpeaking, applicable only to files, gravers, and ſuch tools as are intended to be left at the extreme of hardneſs. Yet though Mr. Stodart did not ſeem to attach much value to peculiarities in the proceſs of hardening, he mentioned it as the obſervation and practice of one of his workmen, that the charcoal fire ſhould be made up with ſhavings of leather: and upon being aſked, what good he ſuppoſed the leather could do, this workman replied, that he could take upon him to ſay, that he never had had a razor crack in the hardening ſince he had uſed this method, though it was a very common accident before.

It appears to me from the conſideration of other facts, that this proceſs is likely to prove advantageous. When brittle ſubſtances crack in cooling, it always happens from the outſide contracting and becoming too ſmall to contain the interior parts. But it is known, that hard ſteel occupies more ſpace than when ſoft, and it may eaſily be inferred, that the nearer the ſteel approaches to the ſtate of iron, the leſs will be this increaſe of dimenſions. If then we ſuppoſe a razor, or any other piece of ſteel, to be heated in an open fire with a current of air paſſing through it, the external part will by the loſs of carbon become leſs ſteely than before; and when the whole piece comes to be hardened, the inſide will be too large for the external part, which will probably crack. But if the piece of ſteel be wrapped up in the cementing mixture, or if the fire itſelf contain animal coal, and is put together ſo as to operate in the manner of that mixture, the external part, inſtead of being degraded by this heat, will be more carbonated than the internal part, in conſequence of which it will be ſo far from ſplitting or burſting during its cooling, that it will be acted upon in a contrary direction, tending to render it more denſe and ſolid.

One of the greateſt difficulties in hardening ſteel works of any conſiderable extent, more eſpecially ſuch articles as are formed of thin plates, or have a variety of parts of different ſizes, conſiſts in the apparent impracticability of heating the thicker parts, before the ſlighter are burned away; beſides which, even for a piece of uniform figure, it is no eaſy matter to make up a fire which ſhall give a ſpeedy heat, and be nearly of the ſame intenſity throughout. This difficulty formed a very conſiderable impediment to my ſucceſs in a courſe of delicate ſteel work, in which I was engaged about ſeven years ago; but after various unſucceſsful experiments, I ſucceeded in removing it by the uſe of a bath of melted lead, which for very juſtifiable reaſons has been kept a ſecret till now. Pure lead, that is, to ſay, lead containing little or no tin, is ignited to a moderate redneſs, and then well ſtirred. Into this the piece is plunged for a few ſeconds; that is to ſay, until when brought near the ſurface that part does not appear leſs luminous than the reſt. The piece is then ſpeedily ſtirred about in the bath, ſuddenly drawn out, and plunged into a large maſs of water. In this manner a plate of ſteel may be hardened ſo as to be perfectly brittle, and yet continue ſo ſound as to ring like a bell; an effect which I never could produce in any other way. Mr. Stodart has lately made trial of this method, and conſiders it to be a great acquiſition to the art, as in fact I found it.

The letting down, or tempering of hard ſteel, is conſidered as abſolutely neceſſary for the production of a fine and durable edge. It has been uſual to do this by heating the hardened ſteel, till its bright ſurface exhibits ſome known colour by oxidation. The firſt colour is a very faint ſtraw colour, becoming deeper and deeper by increaſe of heat, to a fine deep golden yellow, which changes irregularly to purple, then to an uniform blue, ſucceeded by white and ſeveral ſucceſſive faint repetitions of theſe ſeries. It is well known, that the hardeſt ſtate of tempered inſtruments, ſuch as razors and ſurgeon's inſtruments, is indicated by this ſtraw colour; that a deeper colour is required for leather cutter's knives, and other tools that require the edge to be turned on one ſide; that the blue which indicates a good temper for ſprings, is almoſt too ſoft for any cutting inſtrument except ſaws, and ſuch tools as are ſharpened with a file; and that the lower ſtates of hardneſs are not at all adapted to this uſe. But it is of conſiderable importance, that the letting down or tempering, as well as the hardening, ſhould be effected by heat equally applied, and that the temperatures, eſpecially at the lower heats, where greater hardneſs is to be left, ſhould be more preciſely aſcertained than can be done by the different ſhades of oxidation. Mr. Hartley firſt practiſed the method of immerſing hard ſteel in heated oil, or the fuſible compound of lead five parts, tin three, and biſmuth eight. The temperature of either of theſe fluids may be aſcertained in the uſual manner, when it does not exceed the point at which mercury boils: and by this contrivance the ſame advantages are obtained in lowering the temperature of an whole inſtrument, or any number of them at once, as have already been ſtated in favour of my method of hardening. Oil is preferable to the fuſible mixture for ſeveral reaſons. It is cheaper; it admits of the work being ſeen during the immerſion by reaſon of its tranſparency; and there is no occaſion for any contrivance to prevent the work from floating.

I requeſted Mr. Stodart to favour me with an account of the temperatures at which the ſeveral colours make their appearance upon hardened ſteel; in compliance with which he made a ſeries of experiments upon ſurgeons [illegible] hardened, highly poliſhed, and expoſed to a gradual heat while floating at the ſurface of the fuſible mixture. The appearances are as follow:

No. 1. taken out at 430° of Fahrenheit. This temperature leaves the ſteel in the moſt excellent ſtate for razors and ſcalpals. The tarniſh, or faint yellowiſh tinge it produces, is too evaneſcent to be obſerved without compariſon with another piece of poliſhed ſteel. Inſtruments in this ſtate retain their edge much longer than thoſe upon which the actual ſtraw colour has been brought, as is the common practice. Mr. S. informs me, that 430° is the loweſt temperature for letting down, and that the lower degrees will not afford a firm edge.

No. 2 at 440°, and 3 at 450°. Theſe needles differ ſo little in their appearance from No. 1, that it is not eaſy to arrange them with certainty when miſplaced.

No. 4 has the evident tinge which workmen call pale ſtraw colour. It was taken out at 460°, and has the uſual temper of penknives, razors, and other fine edge tools. It is much ſofter than No. 1, as Mr. Stodart aſſures me, and this difference exhibits a valuable proof of the advantages of this method of tempering.

Nos. 5, 6, 7 and 8, exhibit ſucceſſive deeper ſhades of colour, having been reſpectively taken out at the temperatures 470°, 480°, 490°, and 500°. The laſt is of a bright browniſh metallic yellow, very ſlightly inclining to purple.

No. 9 obtained an uniform deep blue at the temperature of 580°. The intermediate ſhades produced on ſteel by heats between 500° and 580° are yellow, brown, red, and purple, which are exhibited irregularly on different parts of the ſurface. As I had before ſeen this irregularity, particularly on the ſurface of a razor of Wootz *, and had found in my own experience, that the colours on different kinds of ſteel do not correſpond with like degrees of temper, and probably of temperature in their production, I was deſirous that ſome experiments might be made upon it by the ſame ſkilful artiſt. Four beautifully poliſhed blades were therefore expoſed to heat on the fuſible metal. The firſt was taken up when it had acquired the fine yellow, or uniform deep ſtraw colour. The ſecond remained on the mixture till the part neareſt the ſtem had become purpleiſh, at which period a number of ſmall round ſpots of a purpleiſh colour appeared in the clear yellow of the blade. The third was left till the thicker parts of the blade were of a deep ruddy purple, but the concave face ſtill continued yellow. This alſo acquired ſpots like the other, and a ſlight cloudineſs. Theſe three blades were of caſt ſteel; the fourth, which was made out of a piece called Styrian ſteel, was left upon the mixture till the red tinge had pervaded almoſt the whole of its concave face. Two or three ſpots appeared upon this blade, but the greater part of its ſurface was variegated with blue clouds, diſpoſed in ſuch a manner as to produce thoſe waving lines which in Damaſcus ſteel are called the water. Two reſults are more immediately ſuggeſted by theſe facts; firſt, that the irregular production of deep colour upon the ſurface of brightened ſteel, may ſerve to indicate the want of uniformity in its com-

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* On Wootz, ſee Pearſon in the Phil. Tranſ.

poſition, as well as the method by an acid which has before been explained in this work*; and ſecond, that the deep colour being obſerved to come on firſt at the thickeſt parts, Mr. Stodart was diſpoſed to think, that its more ſpeedy appearance was owing to thoſe parts not having been hardened. But upon trial with a plate of ſteel made quite hard at one end, and left ſoft at the other, I found that heat applied in the middle produced the regular changes at both ends preciſely in the ſame manner. I ſuppoſe, therefore, that the thicker parts ſinking deeper into the hot metal, experienced a ſtronger re-action and better contact, which may have accelerated the communication of heat. It may be here noticed, that we found upon repeating the experiment of applying nitrous acid to bright ſteel, which was hardened in part only, the black tinge appeared more ſpeedily and ſtrongly upon the hard parts, than the reſt of the ſurface: a remarkable event, for the explanation of which I have no theory to offer.

Let us now ſuppoſe our cutting inſtrument to be forged, hardened, and let down or tempered. It remains to be ground, poliſhed, and ſet. The grinding of fine cutlery is performed upon a grindſtone of a fine cloſe grit, called a Bilſon grindſtone, and ſold at the tool ſhops in London at a moderate price. The cutlers uſe water, and do not ſeem to know any thing of the method by tallow †. The face of the work is rendered finer by ſubſequent grinding upon mahogany cylinders, with emery of different fineneſs, or upon cylinders faced with hard pewter, called laps, which are preferable to thoſe with a wooden face. The laſt poliſh is given upon a cylinder faced with buff leather, to which crocus, or the red oxide of iron is applied with water. This laſt operation is attended with conſiderable danger of heating the work, and almoſt inſtantly reducing its temper along the thin edge, which at the ſame time acquires the colours of oxidation.

The ſetting now remains to be performed, which is a work of much delicacy and ſkill: ſo much ſo indeed, that Mr. Stodart aſſures me, he cannot produce the moſt exquiſite and perfect edge if interrupted by converſation, or even by noiſes in the ſtreet. The tool is firſt whetted upon a hone with oil, by rubbing it backwards and forwards. In all the proceſſes of grinding or wearing down the edge, but more eſpecially in the ſetting, the artiſt appears to prefer that ſtroke which leads the edge according to the action of cutting, inſtead of making the back run firſt along the ſtone. This proceeding is very judicious; for if there be any lump or particle of ſtone, or other ſubſtance lying upon the face of the grinder, and the back of the tool be firſt run over it, it will proceed beneath the edge, and lift it up, at the ſame time producing a notch. But on the other hand, if the edge be made to move foremoſt, and meet ſuch a particle, it will ſlide beneath it and ſuffer no injury. Another condition in whetting is, that the hand ſhould not bear heavy: becauſe it is evident, that the ſame ſtone muſt produce a more uniform edge if the ſteel be worn away by many, than by few ſtrokes. It is alſo of eſſential importance, that the hone itſelf ſhould be of a fine texture, or that its ſiliceous particles ſhould be very minute. Mr. Stodart informs me, that there are no certain criterions by which an excellent hone can be

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* Philoſophical Journal, I. 470.

† Philoſophical Journal, I. 131.

diſtinguiſhed from one of ordinary value, excepting thoſe derived from the actual uſe of both: that the Turkey ſtone cuts faſt, but is never found with a very fine grit: that the yellow hone is moſt generally uſeful, and that any ſtone of this kind requires to be ſoaked in oil, and kept wet with that fluid, or otherwiſe its effects will be the ſame as that of a coarſer ſtone under the better treatment: and laſtly, that there is a green hone found in the old pavement of the ſtreets of London, which is the beſt material yet known for finiſhing a fine edge.

The grindſtone leaves a ragged edge, which it is the firſt effect of whetting to reduce ſo thin, that it may be bended backwards and forwards. This flexible part is called the wire, and if the whetting were to be continued too long, it would break off in pieces without regularity, leaving a finer, though ſtill very imperfect edge, and tending to produce accidents while lying on the face of the ſtone. The wire is taken off by raiſing the face of the knife to an angle of about 50 degrees with the ſurface of the ſtone, and giving a light ſtroke edge foremoſt alternately towards each end of the ſtone. Theſe ſtrokes produce an edge, the faces of which are inclined to each other in an angle of about 100 degrees, and to which the wire is ſo ſlightly adherent, that it may often be taken away intire, and is eaſily removed, by lightly drawing the edge along the finger nail. The edge thus cleared is generally very even: but it is too thick, and muſt again be reduced by whetting. A finer wire is by this means produced, which will require to be again taken off, if for want of judgment, or delicacy of hand, the artiſt ſhould have carried it too far. But we will ſuppoſe the obtuſe edge to be very even, and the ſecond wire to be ſcarcely perceptible. In this caſe the laſt edge will be very acute, but neither ſo even nor ſo ſtrong as to be durably uſeful.

The finiſh is given by two or more alternate light ſtrokes with the edge ſlanting foremoſt, and the blade of the knife raiſed, ſo that its plane forms an angle of about 28 degrees with the face of the ſtone. This is the angle which by careful obſervation and meaſurement, I find Mr. Stodart habitually uſes for the fineſt ſurgeon's inſtruments, and which he conſiders as the beſt for razors, and other keen cutting tools. The angle of edge is therefore about 56 degrees.

The excellence and uniformity of a fine edge may be aſcertained, by its mode of operation when lightly drawn along the ſurface of the ſkin, or leather, or any organized ſoft ſubſtance. Lancets are tried by ſuffering the point to drop gently through a piece of thin ſoft leather. If the edge be exquiſite, it will not only paſs with facility, but there will not be the leaſt noiſe produced, any more than if it had dropped into water. This kind of edge cannot be produced, but by performing the laſt two or more ſtrokes on the green hone.

The operation of ſtrapping is ſimilar to that of grinding or whetting, and is performed by means of the angular particles of fine crocus, or other material bedded in the face of the ſtrap. It requires leſs ſkill than the operation of ſetting, and is very apt, from the elaſticity of the ſtrap, to enlarge the angle of the edge, or round it too much.

Thoughts

VII.

Thoughts on Magnetiſm, by RICHARD KIRWAN, *Eſq*; *L L.D. F.R.S. and M.R.I.A.*

(*Concluded from page* 94.)

IF the ſouth pole of a magnet be charged with filings of iron, and then approached to a bar of iron ſtanding erect, part of the filings will drop off, becauſe the poles of the ſame name, by exciting a contrary arrangement, weaken each other; but if the filings were ſuſpended from the north pole of the magnet it would take up ſtill more of the filings, as the oppoſite poles ſtrengthen each other, the uppermoſt pole of the iron in this caſe becoming magnetic by poſition.

If the ſynonimous poles of two magnets of unequal powers be approached to each other, if the powers be very unequal the ſtronger immediately deſtroys the weaker, and inducing a contrary diſpoſition attracts inſtead of repelling it; if the powers be leſs unequal it requires a longer time; ſo alſo if one be *ſofter* than the other. Even if their powers be equal, yet after ſome time the ſofter will yield to the harder. If both be equally hard they only *weaken* each other.

If a magnet be cut in two, in a direction parallel to the axis, the parts before conjoined will now repel each other, becauſe they ſtill retain two ſynonimous poles.

But if the magnet be cut in two in a direction perpendicular to the axis, the two ends before conjoined will now attract each other.

If a magnetic wire be twiſted, its powers are ſo diſordered that one ſide of the wire, in ſome places of it, will be attracted and the other ſide repelled by the ſame pole of the magnet.

The power of the magnets (*cæteris parib.*) is in proportion to their ſurfaces or as the ſquares of their diameters.—See *Hutton's Magnetiſm*, p. 72.

Communication.

When iron is applied to or brought within the ſphere of activity of a particular magnet, it acquires the arrangement requiſite to form the heteronymous pole, and thus becomes itſelf in ſome degree magnetic in its whole length, if this length be not totally diſproportioned to the power of the particular magnet.

Hence the other end of ſuch bar of iron acquires the the arrangement of the oppoſite pole, according to the laws of cryſtalization already laid down.

Iron becomes magnetic either by contact or proximity to a magnet, or by poſition, or by internal commotion.

If a bar of iron be placed in a vertical poſition its inſenſible fibrillæ gradually acquire the magnetic arrangement, ſo that after ſome years it becomes a complete magnet, its *loweſt* part becoming a north pole, that is, pointing when free to the north, and the *upper* a ſouth pole. In the S. hemiſphere the under end becomes a ſouth pole.

A bar of iron not previouſly magnetic does not acquire this diſpoſition in the ſlighteſt degree while lying in a horizontal or nearly a horizontal diſpoſition, but if one end of it be raiſed it immediately acquires it in ſome degree, as appears by approaching a magnetic needle to either end, becauſe in that direction it is then expoſed to the activity of the polar ends of the great general magnet.

But if a bar of iron be heated, though only at one end, and while hot ſet in a vertical or nearly a vertical poſition, it will acquire the magnetic power much more readily.

So alſo if one end of a bar of iron not magnetic be ſtruck againſt the ground it will become in ſome degree magnetic, the lower end becoming a north pole, &c. and if afterwards the other end be ſtruck in the ſame manner the poles will be reverſed.

Hence it is evident that *any motion* communicated to the integrant particles of iron placed in a proper ſituation helps them to aſſume the magnetic diſpoſition already impreſſed upon them by the great general magnet.

If the oppoſite poles of two magnets of *equal power* be approached to each other the power of both is increaſed; and if one of them be *more powerful* than the other it will increaſe the magnetic diſpoſition, and conſequently the power of the weaker.

Soft iron, as its parts are moſt eaſily moved, receives the magnetic diſpoſition moſt eaſily, *hard* iron or tempered ſteel more difficultly, and *caſt* iron, as being both hard and abounding in the heterogeneous particles, moſt difficultly and imperfectly.

Whatever way iron is applied to a magnet the magnetic power is diffuſed in the direction of its length. Hence it ſhould ſeem that when a bar of iron is laid on a magnet the contiguous ends of the iron become poles of the *ſame name* with thoſe of the magnet to which they are contiguous, and hence may be derived the power of *armed magnets*, for the ſurfaces of the *armour* immediately beneath thoſe of the magnet impreſs a direction oppoſite to their own on thoſe of the magnet, and conſequently rectify ſuch ſurfaces of the magnet as may have been inaccurately directed, and thus ſtrengthen it.

To communicate the magnetic power to iron by friction againſt a magnet, it is neceſſary that its pole ſhould ſlide along the magnet ſeveral times in the ſame direction, for if the directions be alternately oppoſed the powers received will ſucceſſively deſtroy each other.

A ſynonimous pole is formed at the end at which the friction begins, to that of the magnet applied, and an oppoſite at that at which it terminates.

Appropriation to Iron.

It has of old been obſerved that the magnetic phænomena were peculiar to iron, and the reaſons why they are ſo have been already aſſigned, but of late ſome ſemi-metals have been obſerved to partake of theſe properties, as Nickel, Kobalt and Manganeſe; this has been thought to ariſe from a mixture of ferruginous particles from which they can be ſcarcely freed, and with reſpect to Manganeſe, and in many caſes of the others alſo, this ſeems to hold true; but with reſpect to Nickel, and in ſome inſtances of the others alſo, the magnetic properties they diſcover ſeem to me to proceed from their great attraction to iron, particularly when their particles are duly arranged, for then they are expoſed to the power of the great general magnet, which acts on them in proportion to this arrangement and their affinity to iron.

Of Iriclination and Declination.

Theſe phænomena, which are ſo different in different parts of the globe, and even in different ſeaſons and hours of the day, not being as yet noted with ſufficient certainty and preciſion, I ſhall for the preſent decline entering into their explanation.

VIII.

On the Fetid Gas of Drains.

SIR,

London, May 16, 1800.

THERE is a phenomenon which occurs too frequently for the comfort and eaſe of many of the inhabitants of this and other towns which have ſubterraneous drains, and has not been explained as I can yet find by reading or enquiry. Previous to, or about the time of the late change from fair to frequent and continued rains, the apertures of the common ſewers, or great public drains communicating with the ſtreets as well as with houſes, privies, &c. emitted a well-known and abominably diſguſting ſmell, which is often perceived upon change of weather. It is alſo obſervable in ſome houſes that this ſmell is always more or leſs predominant when the windows are ſhut in at night. Perhaps I may be troubling you upon a very well-known ſubject, when I requeſt you will explain theſe facts, and point out the remedy; but I am ſure, however, that many of your readers would be glad to have your thoughts upon it, as well as

Your obliged reader,

S. S.

The ſubject to which the enquiries of S. S. are directed is, in fact, but little underſtood. The emiſſion of fetid gas from the drains is found to take place when the barometer falls; and perhaps alſo upon changes of the thermometer, though I am diſpoſed to think this laſt is not the caſe. The offenſive gas which thus riſes from its ſubterraneous ſituation conſiſts, probably, for the moſt part of ſulphurated hydrogen, together with humid effluvia and putreſcent matter. Houſes which are ſubject to this infection are remarkable for the ſpeed with which ſilver plate becomes blackened, even when the preſence of the gas is not perceptible to the inhabitants; and they are alſo characteriſtically remarkable for diſeaſe and mortality. I do not know of any experiments which have been ſtrictly inſtituted to ſhew that the laws of the elaſticity and conſequent expanſion of the gaſes under variations of preſſure are different, and in what direction and quantity. But it is evident, in the preſent caſe, that when the ſuperincumbent preſſure of the air above us is diminiſhed, the gas in the drains expands more than the common air near the ſurface of the earth: ſo that the former is made to aſcend into the apartments of houſes and the ſtreets. The action of the fires in dwelling houſes cauſing an aſcending current up the chimneys, there muſt of courſe be a ſupply at the doors, windows, and other openings: but when theſe openings are moſt effectually ſhut, as is the caſe at night, they will leſs readily ſupply the air, and a larger portion will be preſſed in from the drains. It is certain, nevertheleſs, that this preſſure, as far as it regards the openings into a dwelling houſe, is in all caſes very ſmall. Indeed, there is no probability that it ſhould equal the preſſure of half an inch of water. The remedy for this ſerious and very diſguſting inconvenience is, therefore, very eaſy and ſimple, and likewiſe very well-known, though much leſs ſo than could be wiſhed when we reflect on the number of houſes which are thus incommoded. I do not, therefore, think that the want of novelty ought to be conſidered as a ſufficient reaſon why I ſhould heſitate to deſcribe for the uſe of my preſent correſpondent, and others in the like want of information, the very common apparatus which bricklayers call a ſtink trap.

Let us ſuppoſe a drain, or ſubterraneous gutter, to communicate from beneath the pavement of a kitchen to the principal drain in the ſtreet, and that there is a grate, or a ſet of holes, in the pavement for the purpoſe of ſuffering waſte water to flow off. Theſe ſame holes will admit the noxious vapours of the drain into the houſe. But if any contrivance could be made that the drain ſhould ſuffer water to run out, and at the ſame time prevent air from returning back, we ſhould evidently be in poſſeſſion of a remedy for the ſubject of complaint. A variety of methods preſent themſelves for the accompliſhment of this. The moſt common is diſtinguiſhed by the name given above. It conſiſts in incloſing the drain with a flat ſtone, which ſerves as a wall, to prevent all communication as to the upper part of the cavity: but the water is ſuffered to paſs off beneath its lower edge by ſinking the floor of the drain in that particular part. The depreſſed part or cavity, therefore, forms a ſmall well or pool, in which water is conſtantly lodged; and the edge of the ſtone being deſignedly low enough to remain conſtantly immerſed, the gas cannot return, unleſs its preſſure be ſufficient to ſink that ſurface of the water which is furtheſt from the houſe to a depth at leaſt equal to that depreſſion;—an event which never happens.

Though this contrivance is attended with a charge which is very little when counterbalanced againſt the evils it removes, yet as it requires the pavement to be taken up, and a few other diſpoſitions to be made, there are ſufficient motives for preferring ſtill ſimpler methods if offered. One of theſe conſiſts in making the cavity or depreſſion of the floor immediately beneath the opening in the pavement, and fixing a piece of wooden or other pipe to paſs from the ſurface of the ground into the ſtagnant water. It may eaſily be underſtood that a perfect cloſure muſt be made round the ſides of the pipe before the grate or drain ſtone is laid down. Another apparatus, which is ſtill ſimpler in its application is caſt for that purpoſe at ſome of our iron founderies, or may be made in wood or metal by thoſe in remote ſituations, who may prefer it. A bowl about ſix or ſeven inches in diameter, or even ſmaller, is caſt with a flanch or flat rim, for the purpoſe of ſetting it in the pavement. In the bottom, or loweſt part of the cavity, there is a round hole, defended by a ſhort upright pipe, through which water, if poured into the bowl, would paſs as ſoon as the bowl is filled to the top of the pipe. A perforated cover, with holes in it, is made ſo as to fit in the cavity of the bowl; and there is another ſtill ſmaller bowl rivetted, bottom upwards, to the cover itſelf. When the cover is put on, the ſmall bowl therefore ſurrounds the pipe, and when water is poured through the holes, it runs into the larger bowl, and a quantity always remains therein ſufficient to ſtand above the edge of the inverted bowl, becauſe the middle pipe riſes higher than that edge. It is ſcarcely neceſſary to add, that the waſte water will flow through the bowl by means of its pipe; but that the air of the drains can only have acceſs to the inſide of the inverted bowl, and cannot enter the houſe unleſs its preſſure were ſufficient to ſink the ſurface of the included water, and raiſe that in the outer bowl as much as the inverted edge lies lower than the top of the pipe. The preſſure is never equal to this quantity.

IX.

A Memoir, in which the Queſtion is examined, whether Azote be a ſimple or compound Body? By CHRISTOPHER GIRTANNER, *Doctor of Phyſic at Gottingen**.

THE moſt celebrated chemiſts have long been aware of the important part which is performed by azote in all the operations of nature. Lavoiſier, Fourcroy, Berthollet, Van Mons, Guiton, Chaptal, Vauquelin, Prieſtley, Van Marum, Goettling, Wiegleb, Von Hauch, Paets Van Trooſtwyck, Deiman, and many other chemiſts, have ſtudied the nature of this ſubſtance with various degrees of ſucceſs. To theſe united labours it is that we are indebted for our knowledge of its ſingular properties, which are ſo very different from the properties peculiar to other elaſtic fluids.

But this ſingular principle appears to be more eſpecially of importance with regard to its effects in organized bodies.

In the inquiries which I have made for more than twelve years paſt, on the mechaniſm of life in animals and plants, I found myſelf every where ſtopped by this principle ſo little known. I ſaw it appear and diſappear in my experiments, without being able to fix it, or to explain the manner in which it had been introduced into the bodies from which I had extracted it. I ſoon began to ſuſpect that azote was not a ſimple, but a compound body.

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* Communicated by the author to Citizen Van Mons, and publiſhed in the Annales de Chimie, XXXIV. 1.

I formed various conjectures, which experiment proved to be falſe, and had entirely renounced this reſearch in deſpair, when the diſpute which was agitated concerning the azote obtained from the vapour of water again excited my attention. This diſpute is not yet terminated, notwithſtanding what has been ſaid for and againſt the change of water into gas by Goetling, Wiegleb, Von Hauch, Weſtrumb, Achard, Wurzer, Juch, Van Mons, Paets Van Trooſtwyck, and Dieman. It appears to me that this famous diſpute has much analogy with ſeveral other controverſies mentioned in the annals of our ſcience, and by which the moſt important points of chemical theory have been fixed. Such was the diſpute reſpecting the exiſtence or non-exiſtence of the carbonic acid in chalk, which engaged all the chemiſts of Europe; ſuch was the diſpute reſpecting the exiſtence or non-exiſtence of oxigen in the red oxide of mercury: a diſpute in which I was myſelf engaged, and ill-treated by the German chemiſts, particularly Gren, who was eaſily put out of temper *.

Wiegleb, Goettling, Von Crell, maintained:

1. That the vapours of water in their paſſage through ignited tubes are changed into azote;
2. That this change always happens, and in all circumſtances, provided that vapours of the water are brought into contact with red hot bodies;
3. That water is changed into azotic gas by combining with caloric;
4. That water is the ponderable baſis of azote gas, and every other gas.
5. That, conſequently, the theory of Lavoiſier, is falſe.

The Dutch chemiſts, as well as Meſſrs. Von Hauch, Juch, Van Mons, &c. maintained:

1. That the vapours of water in paſſing through red hot tubes are never, and in no caſe, converted into azote gas;
2. And that it is only by error that azote gas has been obtained, becauſe it was not a product of water, but part of the atmoſpheric air which paſſed through the tubes;
3. That, conſequently, the theory of Lavoiſier remains unſhaken, and the theory of phlogiſton, or that of water being the baſe of all the gaſes, is erroneous.

I was very much intereſted in this diſpute, of which I very attentively followed the progreſs. But I was ſorry to obſerve that the ſpirit of party mixed in the diſcuſſion; that harſhneſs prevailed on both ſides; that the diſpute was not for the acquiſition of truth, but victory, and the enquirers had previouſly decided to find only ſuch facts as their theories required; and, conſequently, that their diſcernment, even in matters of fact, became impaired. Experiments were multiplied; the truth of the narratives was on both ſides diſputed; reſults were found abſolutely contrary to each other; and the parties,

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* Out of all the opinions which Gren has ſupported, there is not one in which he has been in the right: but he poſſeſſes the merit of having written his Syſtematiſches Handbuch, which is an excellent compilation. The hands of Gren were of more value than his head; he compiled well, but his meditations poſſeſſed no force. G.

inſtead of diſcovering the truth as a common baſis of union, became more and more remote from each other in their concluſions.

For my part I was no otherwiſe intereſted in this diſpute than to ſupport the intereſts of truth. I am intimately perſuaded that the ſyſtem of Lavoiſier is conformable to nature: An experiment is announced to me which is ſaid to overthrow this ſyſtem totally. Let us ſee; let us examine; let us repeat; and if the ſyſtem be falſe, it is better to abandon it in good time, and not to wait till it abandons us. Let us not be attached to ſyſtem, but to truth; and when nature ſpeaks, let us liſten to her voice in preference to Stahl or Lavoiſier, Deſcartes or Newton. Whatever may be the reſult of our experiments, we ſhall not fail to profit. The thing of which we riſk the loſs being merely error, we cannot be too deſirous of undergoing it.

Thus it was that I reaſoned. I had learned by the hiſtory of chemiſtry that in all diſputes where the parties obtained oppoſite reſults from the ſame experiments, there is an error in the manner of expreſſing themſelves, and that at the bottom both parties are in the right. I was very much diſpoſed to think that the caſe before us was a queſtion of this kind.

I, therefore, propoſed to myſelf the reſolution of the following queſtions:

1. Are the vapours of boiling water changed into azote gas in their paſſage through ignited tubes?
2. In what circumſtances does this change take place?
3. What is the cauſe of this production of azote gas?
4. Are the experiments contrary to the ſyſtem of Lavoiſier, or not?

I muſt firſt confeſs, before I enter upon this diſcuſſion, that the manner in which the production of azote gas had been explained, in ſuppoſing the external air to paſs through the tubes, did not appear to me at all ſatisfactory.

I had myſelf formerly adopted this explanation [*]; but I very ſoon gave up an opinion ſo contrary to every thing we know of chemiſtry.

I am really concerned to ſee this improbable opinion maintained by chemiſts of the firſt rank. But the hiſtory of chemiſtry ſupplies us with many examples of the ſame nature. Before the immortal Lavoiſier had proſcribed the doctrine of phlogiſton, the Stahlians removed a leading difficulty of their ſyſtem in a manner abſolutely ſimilar. The oxide of mercury was put into a crucible, which was cloſed in the moſt accurate manner, and then expoſed to heat. Upon opening it after cooling, it was found to contain running mercury. Now a reduction of this kind not being poſſible, according to the doctrine of Stahl, without the intermedium of phlogiſton, the partizans of that doctrine were aſked for an explanation of a fact ſo contrary to it. They replied, that the phlogiſton had paſſed through the crucible to unite with the mercury. Even Bergmann and Scheele were content with ſo abſurd an explanation [†], which proves that the ſpirit of ſyſtem miſleads the beſt underſtandings, and renders them ridiculous in the eyes of poſterity leſs prejudiced and more enlightened. I ſhall ſhew that azote does not paſs through ignited tubes, any more than phlogiſton paſſes through crucibles. The experiments which Prieſtley has made to prove that air paſſes through earthen retorts do in no reſpect prove it. He ſaw the retorts ſmoke on their outer ſurface: but this vapour was not, as he imagined, water paſſing through: but the external ſurface of the earthen retort attracted water from the atmoſphere.

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[*] Anfangsgrunde der Antiphlogiſtiſchen Chemie, ſecond edition, page 89---90.

[†] Scheele Von luft und feuer, edition of Leonhardi, page 42.

(To be continued.)

X.

On the Power of the Oxygenated Muriatic Acid in Vegetation. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

FROM a conviction that you have a ſatisfaction in giving a place in your uſeful Journal, to any hints that tend to reſtore an important diſcovery to its rightful owner, I am induced to tranſmit to you the annexed tranſcript of part of an eſſay written by the late ingenious Dr. Ingenhouſz.

It was publiſhed in the year 1796, in an appendix to the general report of the Board of Agriculture: and clearly demonſtrates, that at that period, the Doctor not only poſſeſſed an idea of the power of the oxygenated muriatic acid in accelerating the germination of ſeeds, but had actually put it to the teſt of experiment. I need hardly add, that the effects of this acid in reſuſcitating the dormant powers of vegetable animation, has lately been imported as a philoſophical novelty from the Continent.

" As I have made mention more than once in this paper, of a letter I wrote to Sir John Sinclair, dated Dec. 2d. 1794, on different articles relating to agriculture, and among others, upon the beneficial effects of alkaline ſalts in promoting vegetation, and reſpecting the effects of ſome other ſalts; and as that letter makes no part of this paper, I think it proper to inform the reader, that the particularly good effect of alkaline ſalt was ſo manifeſt in my own garden, that all the gardeners who ſaw it thought it equal to the effects of the beſt horſe dung. I repeated the application of that ſalt laſt year (1795) at Hartford with the Hon. Baron N. Dimſdale, M. P. in his garden, and that gentleman was equally convinced as myſelf, of its manifeſt good effects. We tried at the ſame time, the application of different neutral ſalts, the particulars of which experiments I may poſſibly publiſh on ſome future occaſion. We made alſo many experiments with different ſolutions, and medicated liquors poured upon the ground, as well as ſteeping the ſeeds of different grains in them. Be it ſufficient to ſay here, that of all the neutral ſalts we tried, the glauber ſalt did ſeem to be one of the beſt in promoting vegetation; and that the ſteeping the ſeeds in the oxygenated marine or muriatic acid, (which is now much employed in bleaching linen in an expeditious way) had a particularly beneficial effect in producing early and vigorous plants.' The beneficial effects of theſe different ſubſtances may be eaſily accounted for by an intelligent reader, according to the theory laid down in this paper."

"We were ſomewhat aſtoniſhed that thoſe ſeeds, *viz.* wheat, barley, rye, and oats, which had been ſteeped in the above-mentioned oxygenated muriatic liquid, even during forty-eight hours, did thrive admirably well; whereas the ſame ſeeds ſteeped during ſo long a time in ſome of the other medicated liquids, were much hurt, or had loſt their vegetative power. The ſame oxygenated liquid poured upon the ground had alſo a beneficial effect."

The diſſertation from which the above extract is made, although evidently a haſty and unfiniſhed production of its truly ſcientific and reſpectable author, contains ſo many original hints, and uſeful views, reſpecting the important object of increaſing the ſubſiſtence of mankind, as derived from the production of farinaceous vegetable matter, that it is to be regretted, it is not known beyond the circle to which its peculiar mode of publication has neceſſarily limited it.

Your's
A conſtant reader,
B.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

An Account of the Irides or Coronæ, which appear around and contiguous to the Bodies of the Sun, Moon, and other luminous Objects [*]. Octavo, 46 p. one Plate. Cadell and Davies. 1799.

THIS work, as well as the other concerning the Inflexions of Light, noticed in the Journal of the laſt month, page 78. is the production of Gibbes Walker Jordan, Eſq. the initials alone of whoſe name are affixed to the concluding page of each work. This contains an application of ſome of the principles and diſcoveries contained in that, to the explanation of the phænomena named in the title, an explanation which how greatly ſoever it might have been hitherto deſired, was certainly unattainable before thoſe diſcoveries were known.

All irides, or coronæ, our author divides into four ſorts; 1. irides conſiſting of many concentric orders of colours contiguous to the ſun or moon; 2. the iris of 45 degrees diameter; 3. the iris of about 84°; and 4. the iris of about 100 degrees diameter, the primary and ſecondary rainbows of philoſophy. After obſerving the inſufficiency of the principles applied by Des Cartes, Huygens, and Newton, to the explanation of theſe different

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[*] For this article I am obliged to the ſame correſpondent as for the former.

ferent appearances, and reprobating the conſtruction of frozen machineries for the purpoſe, he proceeds to ſtate the circumſtances under which the irides of the firſt ſort, the objects of immediate enquiry, are exhibited.

Theſe irides appear of one or more concentric orders and circles of colours, cloſe around and contiguous to the ſun or moon, and ſometimes around the brighteſt of the planets and fixed ſtars. The colours from the luminary outwards are greyiſh, black or blue, or faint white, ſucceeded by a broad denſe white, followed by yellow, then by red; then by violet, blue, green, yellow, red; green, diluted yellow, red; diluted green, diluted red. The ſecond order is frequently only green, yellow, red; and the third only diluted green, red. The two firſt orders are moſt frequently ſeen, the third leſs frequently, the fourth moſt rarely. The firſt may be ſeen every night, when the moon, with more than half a face, ſhines through thin white fleecy clouds, and through ſimilar clouds by receiving the ſun's image through a ſmall hole into a darkened chamber on a ſheet of white paper.

Of theſe irides the diameters vary conſiderably: that of the firſt order from one degree to 5½, and that of the ſecond from 3½ to 10½ degrees. The general breadth of the firſt order of colours is rather more than 45 minutes, and thoſe of the others are ſucceſſively leſs.

In explaining theſe phænomena, icy cryſtalizations of all ſorts are rejected, and globules of water, the only regular forms of concretion which the vapours of the atmoſphere admit of, are alone to be reſorted to for the principles of exiſtence of regular and conſtant phænomena.

For explaining theſe phænomena as produced by globules of water, all the hitherto accepted principles are ſhewn to be inſufficient. The principle of parallel, or efficient rays, employed in the ſchools to account for the exhibition of the primary and ſecondary rainbows, is exceptionable in its former uſe, and is not here to be received, becauſe viſion of images is not by parallel rays, becauſe the light after paſſage, if ſufficiently ſtrong to be ſeen, would only produce a broad circle round the luminary of decaying white light.

Newton explains theſe phænomena by the fits of eaſy tranſmiſſion and reflection, and by comparing the refraction of the rays paſſing through a drop of water, to the reflection of a ſlender beam of light from the back part of a glaſs lens. The fits of eaſy tranſmiſſion and reflection have no exiſtence. The circumſtances of the refracted light in the phænomena are altogether, and eſſentially different from thoſe of the light in the experiment by reflection from a lens, and it is further ſhewn to be impoſſible by any refractions through a drop to produce appearances of the obſerved orders of colours. It is a principle derived from the doctrine of images, and uſed in explaining the primary and ſecondary rainbows, that the colours of the drop are inverted in the bow; and according to this principle, theſe irides, if formed by refractions through the drops, would have their blues external, their reds internal, contrary to obſervation. For this ſame reaſon, and from an eſſential change in the attendant circumſtances, theſe phænomena cannot be produced by the inflections obſerved in a ſlender beam of light paſſing by the edge of a ſingle body.

The only true principles of explanation of theſe phænomena, are to be found among thoſe new obſervations concerning the inflections of light before ſpoken of.

When two bodies approach one another in a ſolar beam, the paſſing light between their edges is divided and diſtributed on both ſides into two complete ſets of fringes. *Vide* Journal, Obſervation ε, page 81. Each of theſe ſets of fringes beginning from the central diſtance between the bodies, and advancing towards either of the bodies, is compoſed of the ſame colours and orders of colours as the obſerved irides. Of theſe the blues are leaſt, the reds moſt bent, the blues furtheſt from, the reds neareſt to the body.

Whenever therefore two globules of water in a cloud or vapour approach near each other, the light paſſing between them will be diſtributed into ſimilar fringes, and the union of theſe fringes from between different drops, will produce all around the luminary circles, and orders of colours, ſuch as by obſervation are really found to be exhibited.

The nearer to, or more remote the two bodies of the experiment are from each other, the more or leſs broad will be the fringes, and therefore the breadths alſo of the irides will depend upon, and vary with the diſtanees of the globules, and thence the obſerved variations of breadth are eaſily accounted for. The breadths are varied alſo, and more eſpecially in vapours of conſiderable extent or thickneſs, by the actions of the ſucceeding ſeries of globules, for although not in the ſame plane, bodies, and conſequently theſe globules, act upon the light paſſing between them, and form fringes. *Vide* Journal, Obſervation η, page 81.

Theſe principles being applied to explain the various phænomena of lights and coloured circles, exhibited in various ſituations by the ſun and moon through the vapours of the atmoſphere, they are next extended to all ſimilar phænomena obſerved round other luminous objects as well as theſe. The flames of candles and lamps ſeen through ſteam, through the exhauſted receiver, through thin flocks of wool or cotton, through ſtuffs of cotton, linen, ſilk, or wool, through the glaſſes of coach windows, on which ſmall drops of water are precipitated from the breath of the incloſed perſons, are ſurrounded with coronæ produced in a ſimilar manner. Tapers are ſeen to burn blue by an eye charged with ſemipellucid humours ariſing from injury, watchfulneſs, and other cauſes producing rings of colours, ſometimes one of duſky blueiſh green, faintly terminated with yellow and red; and ſometimes a ſecond of duſky green and red. A phænomenon of this ſort occurred to Des Cartes. A hole in a window ſhutter will alſo, under ſimilar circumſtances, produce ſimilar appearancts.

Theſe obſervations are illuſtrated by figures of reference, and may further be confirmed by viewing a candle at the diſtance of ten feet or more through a thin flock of wool, or by breathing on a pane of window-glaſs, and looking at the image of a candle reflected obliquely from the glaſs through the precipitated drops of moiſture, when a ſingle halo, compoſed of the firſt order of colours terminated with yellow and red, will be diſtinctly ſeen.

All the circumſtances of theſe phænomena being thus fully conſidered and explained, and no doubt remaining of their characters and cauſes, they may be called *Irides by Inflection.*

A general View of the Nature and Objects of Chemiſtry, and of its Nature and Application to Arts and Manufactures. By William Henry, Member of the Royal Medical, and Natural Hiſtory Societies of Edinburgh; of the Chemical Society of Jena in Saxony: and of the Literary and Philoſophical Society of Mancheſter. Octavo, 44 Pages. Johnſon. London.

It is uſual for thoſe philoſophers who undertake to explain the ſeveral departments of ſcience by lectures, to publiſh a ſyllabus, or outline of their ſubject in the order according to which they propoſe to treat it. Theſe ſhort works, if drawn out with ability, are not only of eminent uſe to the claſſes who may attend the lecturer, but alſo to the community at large, as exhibitions of the ſtate of ſcience, and admirable helps to the memory. Some of theſe works have the form of a ſyſtematical enumeration: others that of a regular diſcourſe. The preſent work is of the latter kind, and preſents an elegant and comprehenſive view of the objects, nature, and application, of chemical ſcience.

Deſcription d'un Télégraphe très ſimple et a la porteé de tout le Monde, avec une Planche. A Paris, chez l'auteur, rue de la Libertè, No. 83. *Pluvoiſe An.* 8. *de l'imprimerie de l'inſtitut des aveugles-travailleurs,* 16 Pages in 8vo. Prix 8 sols.—*Or, a Deſcription of a very ſimple Telegraph, practicable by any Perſon: with an Engraving.*

The telegraph propoſed by this author is the human body: the arms of which are capable of forming with regard to each other, as well as to the perpendicular line of the trunk, a great number of figures, ſufficiently diſtinct to be eaſily ſeen at conſiderable diſtances by the naked eye, or with a teleſcope. The author developes his method, and the means of carrying it into effect: and he thinks, that in this manner lines of moveable telegraphs may be formed, which may become very uſeful in war, to keep up a ſpeedy and conſtant communication between the different bodies of an army, or between the fixed telegraphs of other conſtructions.

This memoir is part of a larger work on the preſent and other ſubjects connected it, which will hereafter appear.

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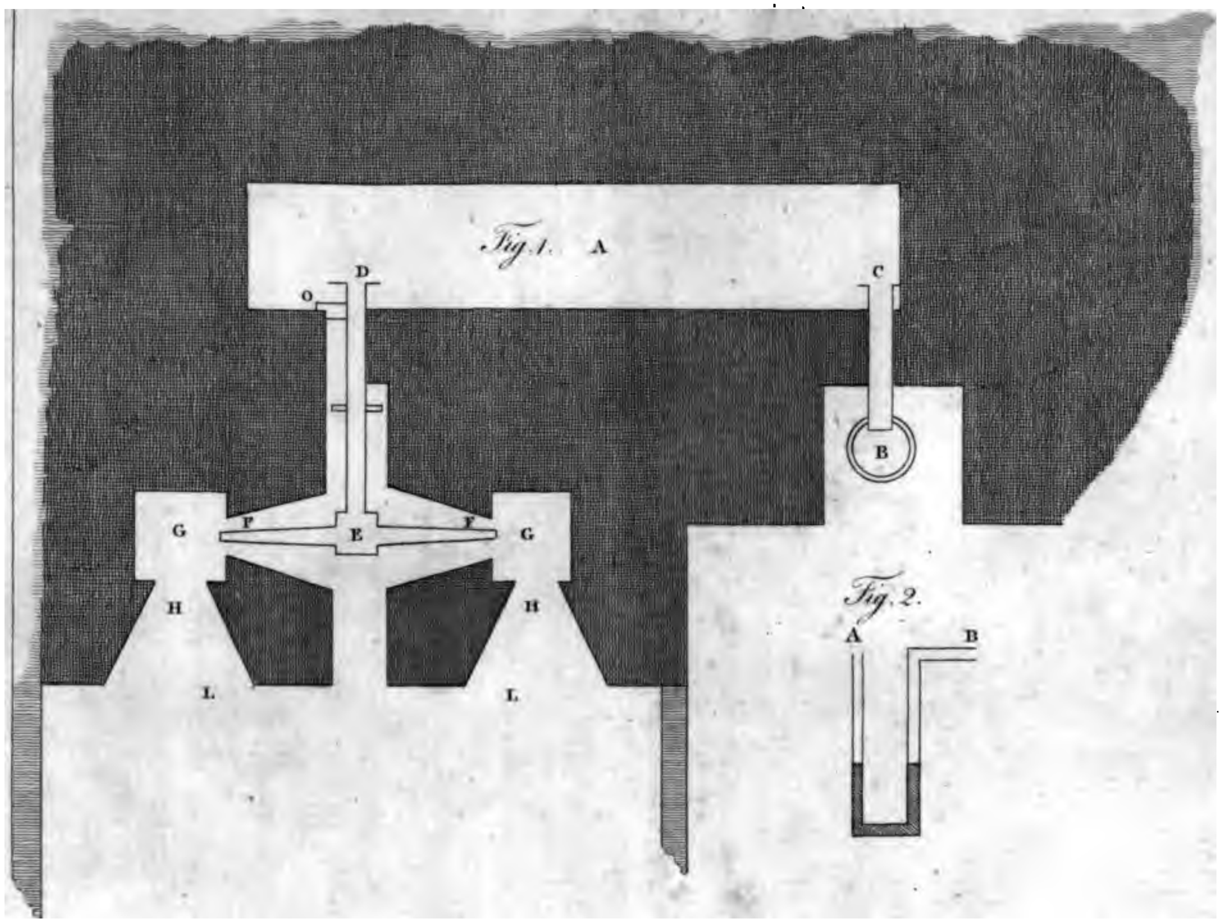
Fig. 1.

Fig. 2.

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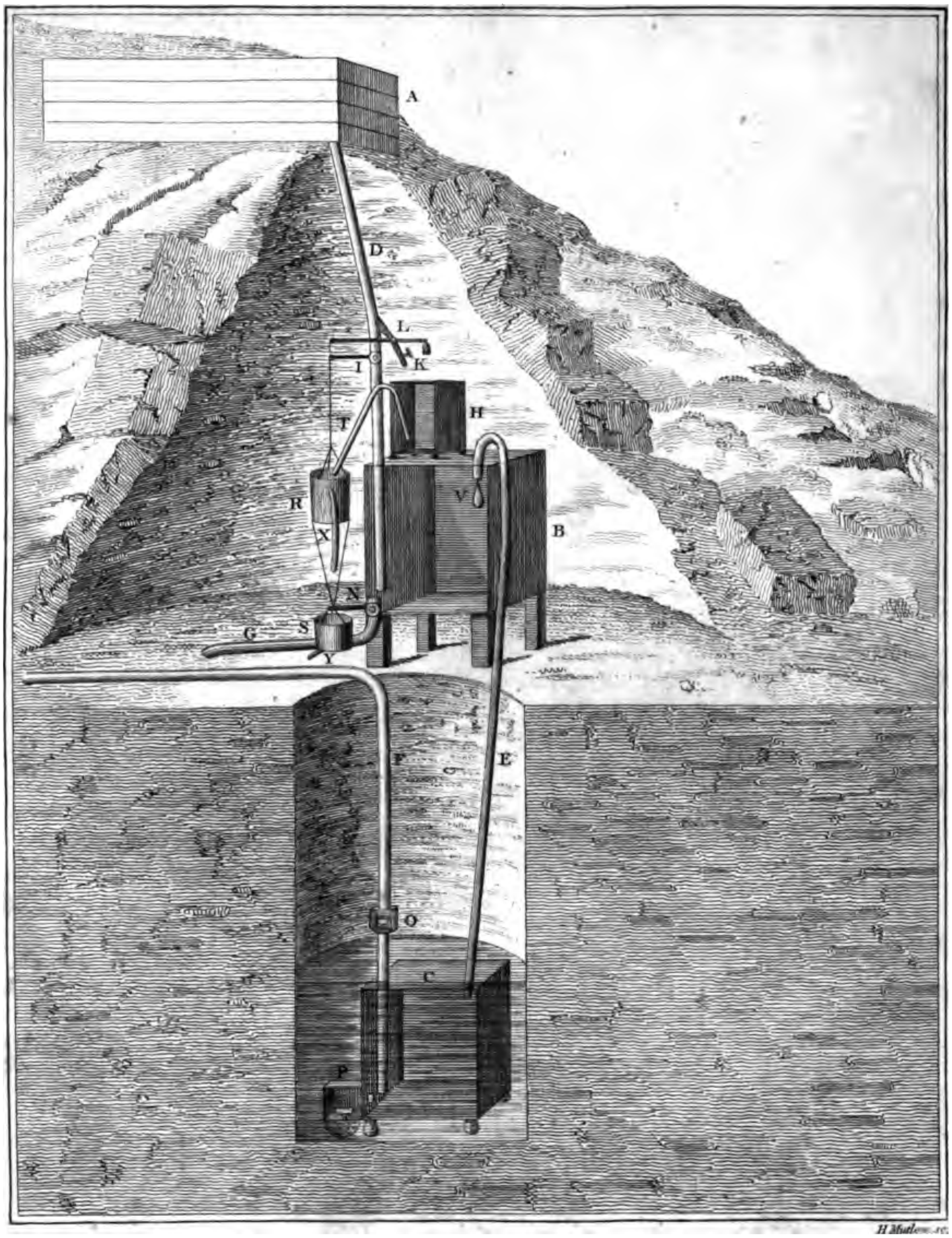

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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JULY 1800.

ARTICLE I.

Deſcription of a new Method of extracting Silver from Copper-Mat by means of Lead, by which the Eliquation of Black-Copper is rendered unneceſſary. By the late Dr. GREN*, *Prof. at Halle in Saxony.*

NOTWITHSTANDING the conſiderable progreſs which the metallurgical part of ſcientific chemiſtry has made in modern times, it muſt be allowed, that its influence on the manipulations in the ſmelting works has hitherto been but of little importance, and that many of theſe operations are carried on by the proceſſes uſed ſeveral centuries ago; with all the errors which have been diſcovered by ſubſequent improvements in chemical knowledge. The introduction of the *preſent proceſs of eliquation*, was indeed a material and great improvement on the former method of ſeparating ſilver from copper, by which the *copper-mat* † was combined with lead, and the maſs afterwards ſubjected to gradual heat. For in this method, the requiſite or preciſe proportion of lead to copper, was totally unknown. As ingenious, however, as the modern eliquating proceſs may appear in a chemical view, it is ſtill in its own nature attended with many conſiderable imperfections; namely, a great loſs of time, of fuel, and eſpecially of lead. Theſe imperfections are inſeparable from this proceſs, and render it ſo very expenſive, as to be totally inapplicable in the operations with *black copper* *, which contains leſs than four ounces of ſilver. It is well known that *Born* has recommended the amalgamation of the copper-mat as a more advantageous method to extract its ſilver. But even though the amalgamating proceſs, of which the efficacy and utility is manifeſt in the richer ſilver ores, had not been proved by experience to be totally impracticable with reſpect to ſuch products as contain leſs ſilver, for example the copper-mat; yet the operation is rendered precarious, and its eſtabliſhment becomes an object of riſque from the circumſtance, that the material requiſite, is in the hands of a poſſeſſor, who may raiſe its price at his own arbitrary choice.

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* Tranſlated from *Dr. Scherer*'s, ſucceſſor to Gren in the profeſſorſhip at Halle, *General Chemical Journal*, Vol. IV. page 155.

† *Copper-mat* (Kupfer-rohſtein) is the product obtained from pyritous copper-ores, having undergone the firſt or *crude fuſion*, by which the ſtony matrix only has been ſeparated.---*Gren*'s *Principles of Modern Chemiſtry*, § 1444.---Tranſl.

My reſearches into the copper-mat, and its conſtituent parts, as well as a more accurate ætiology of the whole of the uſual proceſs, of extracting from it the copper and ſilver in a ſeparate ſtate; together with the inquiries into the mutual powers of chemical attraction of the ſulphur and the metals contained in it, have ſuggeſted to me a new and profitable method of ſeparating the ſilver from copper-mat by means of lead, by which the whole proceſs of eliquating the black copper is rendered unneceſſary. In this method, not only the loſs of lead by oxidation, which renders the eliquating proceſs ſo expenſive, but likewiſe all the other charges of this proceſs are avoided; as for inſtance, thoſe of the building, and the requiſite furnaces and edifices, the neceſſary fuel, &c. My method is no leſs ſimple than eaſy; it is certain and effectual; and (what principally recommends it) it requires no particular edifice; but only a ſlight alteration in the form of the uſual furnace with a concave floor (*Krummofen*). It is founded on invariable chemical, hydroſtatic principles; and is found to be practically applicable and effectual in all the experiments which I have repeatedly made in the ſmall way, and which, in this caſe, cannot but be the ſame as thoſe in the large way.

Principles, on which the new Method is grounded.

1. Copper-mat conſiſts of ſulphur, copper, iron, and ſilver; ſo that theſe metals are completely diſſolved by the ſulphur.

2. Sulphur can diſſolve in fuſion only a determinate quantity of reguline metals previous to its ſaturation.

3. Sulphur has a ſtronger chemical attraction for *iron*, than for copper, lead, and ſilver.

4. Sulphur likewiſe attracts *copper* more ſtrongly, than lead and ſilver.

5. And laſtly, the chemical affinity of ſulphur with *lead* is greater, than its affinity with ſilver.

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* *Black copper* is the metal, which is obtained by a ſecond fuſion from copper ores.---Tranſl.

ſilver. Hence the graduated arrangement of the chemical attractions of ſulphur, with reſpect to the metals mentioned, is as follows:

SULPHUR,
IRON,
COPPER,
LEAD,
SILVER.

6. When the ſulphur is ſaturated with copper, it can no longer diſſolve any portion of lead and ſilver.

7. Silver and lead have a very ſtrong chemical attraction for each other.

Inferences from theſe fundamental Principles.

From theſe aſſertions, unqueſtionably proved by experience, it follows, that if *copper-mat*, the ſulphur of which (the proportion having been ſo much altered by roaſting, as to be ſaturated with copper, is made to be penetrated by lead in fuſion, the lead muſt then take up all the ſilver which it contains, and in ſuch a manner, that no part of the lead can be diſſolved by the ſulphur contained in the copper-mat.

But it would be very wrong to conclude, that the ſilver can be completely extracted from this roaſted copper-mat, merely by ſubjecting the lead, together with the copper-mat, to a melting heat. For the lead, on account of its eaſier fuſibility, would melt ſooner than the copper-mat, ſink down to the hearth of the furnace, and by its greater ſpecific gravity always remain beneath the fuſed copper-mat; and conſequently it would deprive this laſt of its ſilver merely at the ſurface, with which it is in contact, and no farther. If, therefore, it be wiſhed to obtain the intended object, it will be neceſſary that the fluid copper-mat ſhould be forced to riſe through the lead in fuſion, and to penetrate it entirely.

It is on this circumſtance, that the characteriſtic and moſt eſſential part of my new method depends.

It conſiſts in the management, by which the copper-mat (duly roaſted, and in which the ſulphur is ſaturated with the copper) is made to riſe immediately, and during its fuſion through the melted lead. I might call this new method an *amalgamation of fuſed copper-mat with fuſed lead.*

The poſſibility of effecting the conditions, requiſite for this purpoſe, appears very eaſily from *hydroſtatical* principles.—Let the ſpace of the outward floor, or hearth B, Fig. 1. Plate VIII. be connected with the cavity of the furnace A, by means of a hole (*a*). Now if there be only melted lead in the furnace, it will ſtand on the outward hearth, equally as high as in the furnace itſelf. Suppoſe its horizontal ſurface to be as marked (*b c*); then if the melted copper-mat in the interior furnace A, which is of leſs ſpecific gravity, be above that height, it will riſe by the laws of the *equilibrium* in the outward hearth B.

The ſecond chief condition is obtained, by an eaſy management of the fire employed to effect the fuſion. This melting furnace has a recurved bottom, and is provided with an outward hearth, (*Vorderheerd*) and a draining hearth (*Stichheerd*). Its eſſential form of conſtruction is ſufficiently ſhewn in the drawing, before referred to. In this manner the hearth of the furnace lies partly within and partly without; but it is not neceſſary, that the projecting part of its hearth ſhould be of conſiderable breadth. The aperture (*a*), by which the exterior part of the furnace communicates with the interior, ſhould be placed immediately above the floor or bottom of the hearth, or which is ſtill better in that bottom itſelf. And the outer hearth of the furnace ſhould have an inciſion with a gutter, through which the melted copper-mat, riſing above the lead flows off continually, and is collected in a pot (vortiegel) placed on the floor of the ſmelting-houſe. There is, beſides, on the ſide of the outward hearth, another adjoining hearth, into which the lead of the former, when ſufficiently impregnated with ſilver may be drawn off, to be afterwards put into iron veſſels. The moſt convenient and advantageous dimenſions of thoſe hearths, eſpecially the height of the outward-hearth from the floor up to the twyer of the bellows, can indeed be aſcertained by experiments only, but theſe may be eaſily made without much expence.

In order to deprive the copper-mat of its ſilver by means of lead in this apparatus of the furnace, and to manage the fuſion properly, it is required that the proportion of the ſpecific gravity of the copper-mat to that of the lead ſhould be known. For, as this proceſs is grounded on the well known hydroſtatical principle, that the heights of two fluids of different kinds, placed in connected tubes, if required to be in *æquilibrio*, muſt be to each other in the inverſe ratio of their ſpecific gravities, it is evident, that the melted copper-mat in the furnace, to keep the equilibrium with the fuſed lead in the outward hearth, muſt form a column proportionably ſo much higher, as it is exceeded by the lead in ſpecific gravity. It is therefore from the ſpecific gravities of both theſe ſubſtances, that we may find how far diſtant the twyer muſt be from the loweſt point of the hearth, and to what height the lead may be ſuffered to riſe in the outer hearth, with cauſing the fluid copper-mat to enter into that aperture. The ſpecific gravity of copper-mat is variable, according to the different degrees of its roaſting. That of fuſed copper-mat varies from 4.66 after the ſecond roaſting, to 5.20 after the third roaſting.

But for the ſake of greater ſecurity, the leaſt weight only ſhould in this caſe be aſſumed as a ſtandard. The ſpecific gravity of lead, at a mean rate, is 11.35.

From theſe *data* it is obvious, that if the height of the *lead* in the outward hearth be *ſix* inches, the *copper-mat* in the interior furnace, to be equiponderant, muſt ſtand *fourteen inches and one half* high.—If then the copper-mat, riſing through the lead in the outward hearth, covers the ſurface of this laſt to the height of $\frac{1}{2}$ an inch, its column muſt likewiſe be proportionally higher in the furnace, and conſequently reach to 16 inches. It is therefore evident, that, when the lead riſes to 6 inches height in the outward hearth, the diſtance of the twyer, or nozzle of the bellows, muſt amount to at leaſt 18 or 20 inches from the loweſt point of the hearth.

According to the above ſtated ſpecific gravity of lead, one Rhinland cubic* foot of that metal weighs 750 lb. Cologne weight. When therefore 300 pounds of lead are employed in this operation, their volume will amount to 760 cubic Rhinland inches; and hence if the perpendicular height of theſe three centners of lead ſhall be 6 inches in the outward hearth, it follows, that the breadth of this hearth muſt be 9 inches, and its length a little more than 14 inches.

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* The Rhinland foot is reckoned at 12,396 Engliſh inches, and the mark of Cologne is 3612 grains, whence the pound will be 7224 grains, or about half an ounce more than our avoirdupois pound.---N.

In performing the proceſs; the furnace is previouſly heated to a ſufficient degree, and the lead afterwards introduced on the outer hearth; becauſe the heat is there ſufficiently intenſe to fuſe it. Its ſurface is ſecured againſt oxidation, by covering it with powdered charcoal. At the ſame time the furnace is charged with copper-mat, and the requiſite quantity of coals, and the fuſion effected by a clear fire. As the operation proceeds the lower part of the copper-mat iſſuing from the furnace through the aperture (*a*), and riſing through the body of the lead to its ſurface, is farther prevented from cooling, by means of ſome pieces of burning wood. The beſt and moſt uſeful proportion of the coal, to that of the copper-mat in this fuſion, muſt be determined by experience.

The copper-mat being now deprived of its portion of ſilver, and flowing into the fore-pot (vortiegel) from above the lead in the outer hearth, is once more roaſted, previous to extracting the copper. But it is to be obſerved, that by this method of diveſting it of its ſilver, it at the ſame time undergoes a farther roaſting; on which account another part of the expence is ſaved in my proceſs.

In the management of this proceſs, it is evident, that as much copper-mat as may be thought fit, can be decompoſed by the ſame individual quantity of lead; and accordingly, that this laſt may be impregnated with ſilver at pleaſure in various degrees. It is not adviſeable, however, to alloy the lead with too great a proportion of ſilver; becauſe a ſmall loſs of the compound metal would then produce a more conſiderable loſs of ſilver. But I muſt leave this to the judgment of the artiſt, and to future experience.

When the lead is to be run off, the receiver muſt be firſt duly heated, and then the tap hole opened. The whole of the lead is thus drawn off from the outer hearth of the furnace, and its ſurface covered with charcoal-duſt. Freſh lead may then be again conveyed, after ſtopping the tap-hole, into the outer hearth, and the operation continued as before.

Whenever the furnace requires to be cooled by the blaſt of the bellows, the *work lead*[*] is firſt entirely drawn off from the outer hearth, and alſo laded out of the draining hearth; after which, all the copper-mat that remains in the outward hearth, and in the furnace, is ſuffered to run into the draining hearth, now cleared of the lead. But this copper-mat, not being yet deprived of the ſilver it contains, muſt be added at the next charge of the furnace.

The lead, which is thus alloyed with ſilver, is at laſt ſubjected to cupellation in the uſual way. The *litharge*, which is obtained in this operation, the metal abſorbed in by the *teſt* (heerd) and the *firſt waſte*[*] (abzug) need not be reduced to the reguline ſtate by a ſeparate operation; becauſe this may be done at the ſame time, that another portion of copper-mat is cleared of its ſilver by fuſing them together with it. But as this perfection of any art can only be obtained by experience, and diſpatch in the manipulations can be acquired by practice alone, it will be found neceſſary, even in this inſtance, to perform repeated experiments, for the purpoſe of aſcertaining the beſt dimenſions of the parts of the furnace, the moſt ſuitable proportion of the quantity of coals to be employed, and the moſt advantageous direction of the bellows pipe; as well as the moſt convenient regulation of the blaſt, and the moſt ſuitable proportion of the copper-mat to the lead to be employed in this proceſs.

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[*] *Work-lead*, or lead freed from copper, and mixed with ſilver by the operation. See *Gren's Principles of Chemiſtry*, § 1478, 1486.---Tranſl.

The advantages afforded by my method, compared with thoſe reſulting from the proceſs of eliquation, are evident. No lead is here loſt by combuſtion, becauſe it is accurately covered by the copper-mat, and no intenſe heat is required, becauſe theſe ſubſtances are of eaſy fuſion. A much ſmaller quantity of lead is alſo ſufficient in this proceſs, than in that of eliquation; perhaps only a fifth part, or ſtill leſs; whence, upon the whole, the loſs or waſte of lead, including the cupellation, is conſiderably leſs. Beſides this, my proceſs requires but little expence in the building or apparatus, and the expences of ſmelting are by this means in part compenſated; ſeveral fires for roaſting the copper-mat being rendered unneceſſary, and thus ſaved. From this diminution of expences in my new method, ſuch copper-mats may likewiſe be deprived of their ſilver, as when worked into black copper are ſo poor in ſilver, as not to repay the charges of eliquation.

By theſe means, therefore, when the quantity of fuſed copper-mat in the furnace A has ſufficiently increaſed, it will reach the communicating aperture (*a*), after having driven before it all the lead into the outward-hearth. Laſtly, as ſoon as the copper-mat has advanced to beneath the ſurface of the lead in the outward-hearth B, it will riſe through the lead by the laws of hydroſtatics, and penetrating its maſs, will place itſelf on the ſurface of the lead, continually accumulating there, until having reached the proper height, it can run off through the indentation cut in the rim of the fore-hearth.

In this ſimple manner, therefore, any required quantity of copper-mat may, by repeated charging of the furnace, paſs through a determined quantity of lead; and may be thus deprived of its ſilver without loſs in burned lead, and the lead itſelf may at pleaſure be more or leſs impregnated with ſilver.

Experiments in the ſmall Way, to confirm the Succeſs of this Method.

In order to perform theſe operations in the ſmall way, as well to confirm the theory and practice, and at the ſame time to diſcover a method of making the neceſſary experiments which might enſure correſpondent performance in the large way, I proceeded in the following manner:

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[*] *Firſt waſte* is the droſs or ſcoria formed in the firſt part of the operation, when the cupelling is performed in the large way in the *refining furnace*. See *Gren's Principles of Chem.* §. 1486.---Tranſl.

A Heſſian crucible muſt be perforated at the bottom, and both its internal and external ſurfaces coated with charcoal-duſt and loam. This crucible is then placed in another calice-formed crucible (kelchtutte), or into a black-lead crucible, properly cut for that purpoſe, and likewiſe internally coated with powdered charcoal. In this it is faſtened by means of a wedge made of clay, in ſuch a manner that it cannot elevate itſelf during the experiment. A determinate quantity of lead, altogether free from ſilver, is then fuſed in the interior crucible, through the aperture of which in the bottom it flows into the exterior veſſel. The quantity of lead ſhould be ſufficient to occupy part of the inner crucible: And to protect it againſt oxydation, its ſurface muſt be covered with ſome powdered charcoal in both veſſels. Upon this ſome powdered copper-mat, that has undergone the ſecond or third roaſting, is introduced into the inner crucible, and melted by a fire ſufficiently ſtrong and briſk, ſo that it may flow thin. The putting in of freſh copper-mat is to be continued, till the interior crucible can hold no more, when in thin fuſion. The fluid copper-mat preſſes the fuſed lead in the inner crucible through its aperture at bottom into the outer, and at laſt itſelf follows, and riſes up through the lead, and giving out its portion of ſilver during its tranſition. That portion only of copper-mat which remains in the inner veſſel will continue to preſerve its ſilver. The apparatus is then ſuffered to become cold without agitation, and after breaking the veſſels, the whole of the lead and copper-mat reſting upon it in the outer crucible are to be collected, ſeparating them carefully from each other, and cleaning them from the adhering charcoal-duſt. Their weights are next to be exactly determined; after which the *lead* is aſſayed for ſilver, and the *copper-mat*, deprived of its ſilver, is aſſayed for copper. A calculation may be made from the reſults of the quantity of ſilver obtainable from a certain determinate quantity of copper.

If the copper-mat has not been duly roaſted, and, conſequently, ſtill contains too much ſulphur, it will diſſolve a portion of ſilver in this proceſs, and a loſs will be obſerved in the remaining lead, which will be greater in proportion as the ſulphur in the copper-mat is leſs ſaturated with copper. But if the copper-mat, by too much roaſting, has been brought below the point of ſaturation of the ſulphur with the copper, part of the copper will then ſeparate during the fuſion, and, mixing with the lead, will occaſion an increaſe of weight.

On making experiments with the copper-mat from *Rothenburg*, I found that a mixture of that which had ſuſtained the *third* roaſting heat, with an equal portion of that which had been ſubjected to the *ſecond* roaſting, was the beſt ſuited, and of ſuch a quality, that the effect was quite ſatisfactory.

Out of many different experiments, which I have made in order to put my method to trial, and which afforded very uniform reſults, I ſhall in this place give an account only of the laſt I have made.

Copper-mat from *Rothenburg*, mixed in equal parts of that of the ſecond with that of the third roaſting, was made to paſs, in the manner above explained, through 24 centners of lead. The lead was again recovered with an increaſe of weight, not yet fully amounting to 2 *per cent.* The *work-lead* contained in the centner *two* ounces, 1$\frac{17}{144}$ drachms of ſilver, and hence in the whole 51 *ounces*, and one and $\frac{1}{16}$ drachm. The whole quantity of the copper-mat, diveſted of its ſilver, in this operation, by means of lead, amounted to 9 centners of black copper; hence *one* centner of black copper at 110 lb. yielded 5 *ounces*, 5 and $\frac{65}{144}$ drachms of ſilver. When the copper, obtained from this copper-mat, was examined, it was found to contain no more than 3$\frac{11}{16}$ drachms of ſilver, and, therefore, leſs than is uſually the caſe with eliquated copper.

A more minute Explanation of this Proceſs, as performed in the large Way.

Two conditions are eſſentially requiſite to the management of this proceſs. (1) The copper-mat muſt have been roaſted in the due degree. (2) The melting-furnace muſt have been ſo conſtructed, that the copper-mat, while in fuſion, may be enabled to paſs through the body of the melted lead.

The neceſſity of the firſt condition is evident from what has been already ſaid; namely, that when the copper-mat ſtill contains too large a portion of ſulphur not ſaturated with that metal, part of the lead will be diſſolved by the ſulphur during the fuſion, and an irrecoverable loſs will be thus occaſioned. On the contrary, when the copper-mat has been too ſtrongly roaſted, it then no longer contains a quantity of ſulphur ſufficient to hold all the copper in ſolution. Part of the latter will, therefore, ſeparate, and unite with the lead. But it is better to fall into the laſt error than the firſt; becauſe the copper can be again recovered, when, as muſt be done of courſe, the ſcoriæ and litharge produced in the refining of the lead impregnated with copper, together with the metal imbibed by the aſhes of the teſt, are again reduced to the reguline ſtate; in which ſtate they are conveyed again into the furnace with a copper-mat leſs roaſted. The ſulphur of this laſt again ſeparates the copper from the reduced lead. But for the accompliſhment of this condition it is neceſſary, that by ſmall experiments, eaſy to be made, and in the manner indicated, it ſhould be firſt aſcertained, what degree of roaſting of the copper-mat may be the moſt ſuitable to the operation. And with this view the portion to be ſubjected to the trial ſhould be ſo managed, that an uniform degree of roaſting may be kept up. According to my experiments in the ſmall way, it ſeems, that copper-mat, which has been roaſted the third time in the uſual manner, and has once more paſſed by itſelf through the melting furnace, is apparently the beſt qualified for this purpoſe, if *pure* lead be made uſe of in diveſting it of its ſilver. For though this laſt ſhould happen to take up ſome of copper in the proceſs, yet the copper may be recovered from it, by adding copper-mat of the ſecond roaſting to the litharge, with the firſt waſte and metallic portion abſorbed by the teſt at the time when theſe products of the refinery are afterwards reduced.

We ſee, therefore, that my method ſaves: (1) The expences of building and maintaining the houſe appropriated to the proceſs of eliquation; (2) The wages expended on this account; (3) The additional charges for the fuel in the operations of eliquating, ſecond eliquating (darren)* and melting the lead with the black-copper, in which it is contained; (4) All the loſs of lead, occaſioned by its fuſion with the copper at its eliquation, its refining, and the ſcratch-work with the bruſh in cleaning; (5) Likewiſe the loſs of lead poor in ſilver, which is obtained by eliquation from copper, and ought not to be enriched with ſilver in that degree, as may be done according to my method. And if it ſhould be found adviſeable to *refine* the *work-lead* directly in the ſmelting houſe, there would then (6) be ſaved the charges of tranſporting the black copper to the houſe, in which it is to be eliquated.

To this may be added, that by my method of proceeding the quality of the copper is improved, which is always the chief object of the ſmelting houſe. The copper obtained by eliquation is well known to contain lead, and this proportion of lead imparts to it a noxious property, diminiſhes its ductility, and even that of the braſs, prepared from it. This contamination of the copper with lead cannot take place in my proceſs; becauſe by that management no lead can combine with the copper of the copper-mat made uſe of, when the ſulphur of this laſt has been ſufficiently diminiſhed in its quantity by the roaſting.

II.

Obſervations on the Proofs of the Huttonian Theory of the Earth, adduced by Sir James Hall, Bart. By R. KIRWAN, ESQ. *Communicated by the Author.*

(*Concluded from page* 102.)

EXPERIMENT THE FIRST.

GRUNSTEN, a compound of felſpar and hornblend intimately mixed with each other, was the ſubject of this experiment.

Its *colour*, black, or greeniſh black, intermixed with pale reddiſh brown; both the felſpar and hornblend imperfectly and confuſedly cryſtalized in minute grains; the *fracture* partly ſtriated and partly foliated. *Luſtre* moderate: its hardneſs 7, or almoſt 8. Gives an earthy ſmell when breathed upon, and frequently contains ſmall ſpecks of pyrites. Ibid. p. 7.

This ſubſtance he vitrified by a ſtrong heat and ſubſequent rapid cooling, p. 9. A fragment of the glaſs thus produced being introduced under a narrow muffle, and heated to 21. in one minute became ſo ſoft as to yield readily to the preſſure of an iron rod, but after a ſecond minute it became quite hard, though the temperature had been ſtationary. The ſubſtance thus hardened underwent a thorough change, it loſt its vitreous character, its fracture was like that of *porcelain* (that is even) and it was fuſible only in a heat of 31. page 11.

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* *Gren's Principles of Chem.* §. 1490.

In another experiment, *ibid.* he found this change to take place even before the glaſs was in perfect fuſion. For while both ends of a fragment of this glaſs were ſupported on reſts of clay, it was found not to ſink down between them until the heat was raiſed to 30. In another experiment he found the conſolidation, which he (improperly, as I think) calls cryſtalization, to take place even while the heat was gradually increaſed, and the ſubſtance ſtill ſo viſcid as to retain the original ſhape of the fragments.

In another experiment, where the glaſs was ſlowly cooled, its texture was found compleatly to reſemble that of whinſtone, the *fracture* was *rough*, *ſtony*, and *cryſtaline*, with a number of ſhining facettes interſperſed through the maſs, and a few cryſtals in the cavities produced by air bubbles, p. 8.

Theſe experiments may be conſidered in two points of view; firſt, with reſpect to phenomena of conſolidation in a heat either gradually increaſed *above*, or gradually diminiſhed *below* the heat neceſſary to ſoften the vitreous ſubſtance, the loſs of the vitreous character, and the ſtony appearance, aſſumed through ſlow refrigeration.

And, in the ſecond place, we may examine how far the phenomena here obſerved tend to countenance the Huttonian theory either of the formation of granite, trap, or baſalt, or other ſtony ſubſtances: in this reſpect only it concerns me to examine theſe experiments, yet I cannot forbear mentioning ſome few reflections on the firſt.

It has been obſerved by all thoſe who have attended to the formation of common glaſs (and is, indeed, evident from the fumes that float over its ſurface) that from the inſtant it enters into fuſion, it is in a conſtant ſtate of decompoſition, gradually becomes leſs fuſible, and increaſes in denſity; the ſubſtances that thus eſcape are, in this caſe, the ſaline, as Boſc D'Antic has ſhewn, and Macquer alſo aſſerts. See 1 Boſc D'Antic, 10, and 242, 213. and hence the loſs of weight which gas thus ſuffers, *ibid.* 220, and 4 Macquer 261. Macquer alſo obſerved, that glaſs kept too long in fuſion looſes its tranſparency, and becomes opaque, becauſe the flux evaporates. And he obſerves, that glaſſes formed of argill, lime, and gypſum, are particularly ſubject to this accident. Lavoiſier noticed the ſame phenomenon during the fuſion of felſpar even by oxygen air, namely, that the longer it was kept in fuſion, the more infuſible it became. Mem. Par. 1783, p. 577, which he imputed to the volatility of one of ſome or other of its ingredients. And he afterwards found occaſion to extend the ſame remark to ſteatites, and alſo to a mixture of equal parts of quartz and calcareous ſpar. This increaſed in fuſibility of certain ſubſtances by a gradually increaſed or continued heat, is not, therefore, a new diſcovery having been already noticed: but Sir James Hall has conſiderably enlarged it, by ſhewing that the ſtones he operated upon had re-aſſumed their ſtony appearance, after having been in a vitreous ſtate; this appearance, if I underſtand him rightly, they have aſſumed only in conſequence of ſlow cooling, and not merely by a heat either ſtationary or gradually increaſed; conſolidation only being the effect of ſuch treatment.

This conſolidation Sir James calls *cryſtalization*, a term which ſeems to me highly improper; for, according to every ſenſe in which this term has ever been employed, whether that operation was perfect or confuſed, it denotes at leaſt an union of particles previouſly diſperſed through a liquid medium, they muſt, therefore, be at liberty to move through this medium in order to coaleſce, and re-unite to each other (if both they and the medium itſelf coaleſce and conſolidate), this action is called *coagulation*, as happens in what was called *offa Helmontii*, and the jelly formed by the *liquor ſilicum*; but in Sir James's experiment we find the conſolidation to take place in a fragment of glaſs, which ſtill retained its ſolid ſtate, and, conſequently, the particles were not at liberty to move towards each other. This conſolidation muſt, therefore, evidently have ariſen from ſome internal change in the conſtitution of the glaſſes in which it was obſerved. What theſe changes may have been I ſhall now examine.

In the firſt place, it is highly probable that ſilex, argil, and lime, and ſlightly oxygenated calx of iron, whatever be their affinity to each other when duly proportioned, require, like all ſolids, to abſorb in their paſſage to a liquid ſtate a certain portion of latent heat; but when in fuſion, and the particles of each chemically united, they require a higher degree of heat to keep them in fuſion; their elective affinities promoting fuſion before the union, and impeding it after the union is formed; it is thus that iron and platina, metals ſeparately highly infuſible, contribute to each others fuſion; but when fuſed, become ſtill more infuſible, as appears by Rinm. §. 135. Sulphur and lead are ſeparately and eaſily fuſible, but when united, their fuſion becomes much more difficult. Again, Dr. Kennedy has diſcovered that all theſe whins contain 10 per cent. ſoda; and Vauquelin has lately diſcovered tartarin in felſpar: in the high heats to which theſe ſtones are expoſed in order to vitrify them, may it not be ſuppoſed that theſe ſalts are, in ſome meaſure, volatized, and the compounds thus rendered leſs fuſible? Though in an high heat rapidly produced, they may ſtill be fuſible as a ſmaller proportion of ſoda will in that circumſtance ſuffice to that effect.

The next circumſtance to be accounted for, is the ſaxification or *ſtony* appearance aſſumed by the vitrified ſtones when ſlowly cooled, by far the moſt curious fact, for which we are indebted to the ingenuity of Sir James. To account for this change, it is proper to remark, that though whins are ſaid to be vitrified in a high degree of heat, yet this is not rigorouſly true, for in that caſe they ſhould afford a tranſparent glaſs, whoſe fracture would be perfectly poliſhed with a ſtrong luſtre, as we ſee that of common glaſs, whereas, in truth, they melt only into an *enamel*, nearly approaching to the perfect vitreous ſtate: even the bottles made of them are nothing more; and hence their ſuperior hardneſs. Their ingredients, therefore, are not uniformly diffuſed through their whole maſs, but lie in the ſame order and poſition as before fuſion, and in effect they contain much more ſilex than can be compleatly vitrified by the ſmall proportion of lime and argill that enter into their compoſition, even though aſſiſted by the ſoda; and in the next place we muſt notice, that the affinity of ſoda to ſilex diminiſhes in the ſame ratio as the heat diminiſhes, and, conſequently, they ſeparate, if the heat be not ſo ſuddenly diminiſhed as to impede all motion. This is evident by what happens to common glaſs when ſlowly cooled down to the temperature of the atmoſphere, as came to paſs in the Glaſs Houſe at Leith, and conformably to this inſtance the ſaxification might take place even after a perfect vitrefaction;—that there are unions grounded on chymical affinity, which take place to a certain degree only at certain temperatures, and are in great meaſure looſened at a lower temperature, appears in the common inſtance of the ſolution of moſt ſalts in water, ſpirit of wine, or other menſtruums, greater in a high degree of heat than at a lower, and has alſo been noticed in the fuſion of gold in a maſs of ſilver, for if the ſilver be very gradually cooled, the gold will ſeparate from it, as Homberg obſerved, Mem. Par. 1713.

Now the affinity of ſilex to the alkali being looſened by a ſlight diminution of heat, the affinity of argill to the ſilex to which it united only as to a *compound* in the given temperature, is alſo neceſſarily looſened; that in the dry way argill unites to ſilex in temperatures below 150°, only in conſequence of the previous union of the ſilex to the alkali, is clearly deduced from this fact, that if the alkali be abſent, the union will not take place in temperatures below 150, whereas it takes place by Sir James's own experiment, at temperatures below 100, when the alkali is preſent; for he found the whins fuſible at 55°. It is true the whins contain lime alſo, but though the preſence of a certain proportion of lime contributes materially to the fuſibility of ſilex and argill, yet it would be ineffectual in degrees of heat below 120°, if an alkali were not preſent to aſſiſt it, as I know by experience.

The preſence of argill contributes alſo to the diminution of the affinity of the alkali to the ſiliceous ingredients, as the alkali ſeems to have nearly as ſtrong an affinity (ſome think ſtronger) to argill as to ſilex; hence it is, that all analyſts ſince Bergman's time employ an alkali to looſen the intimate union of ſilex and argill in precious ſtones.

Thoſe facts being duly conſidered, we ſhall not be ſurprized at ſeeing the cloſe vitreous texture deſtroyed by the ſlow cooling of melted whins, (all of which contain the above ingredients) and ſucceeded by the looſer texture of a mere ſtony ſubſtance. This is the only change that takes place, if we except the minute and indeterminate cryſtalizations that occur in the cavities formed by the expulſion of air, while the maſs was as yet ſoft; for the facettes interſperſed through the ſtone cannot be accounted cryſtals, but only the rudiments of cryſtalization. Theſe are formed at the inſtant the affinity of the alkali is leſſened, and the earths begin to aſſume their ſolid ſtate. The alkali being as yet liquid, allows the earthy particles to move through it, and to form theſe incipient cryſtalizations.

We are now to examine how far the ſtony ſtructure aſſumed through ſlow refrigeration, by ſtones previouſly fuſed, tends to afford any ſupport to the Huttonian theory. In my opinion it affords none at all; the utmoſt effect it can produce in an unprejudiced mind, is to render the origin of whins ambiguous, by making them aſſume the appearance of a Neptunian origin, when in fact they owe it to fuſion; but it is only an *appearance*, for natural whins are accompanied with circumſtances, and contain ſubſtances which contradict that appearance, and prove it to be deceitful. Beſides, theſe experiments have no relation whatſoever to granite, or calcareous maſſes, which form the bulk of the globe, and afford not the ſlighteſt indication of their origin; whins, though they abound in Scotland, and ſome other countries, are, in compariſon of the former, but thinly ſcattered over the ſurface of the globe. Some reſemblance betwixt them and lavas has been long noticed. I ſhall now briefly mention a few of the diſcriminating characters of the artificial and natural whins, which may in moſt caſes prevent us from confounding them, or aſcribing to them a common origin.

1. The *natural* whins, particularly Amygdaloides (vulgarly called Toadſtones) frequently contain calcareous ſpar and zeolyte; now as the former contains fixed air, and the latter a notable proportion of water, I hardly think Sir James, who profeſſes not to agree with Dr. Hutton in all points, will allow theſe to have been vitrified or fuſed.

2. The natural whins, according to Dr. Kennedy's ſtatement, loſe 5 per cent. of water, and other volatile matter, when heated to redneſs. It is not ſaid, whether the artificial loſe any part of their weight by ſuch treatment; but it is plain they would not, ſince even the lavas of Catania and Piedmonte, though of ancient date, loſt none, as Dr. Kennedy expreſsly notices, and has thus afforded an excellent criterion for diſtinguiſhing the long conteſted origination of thoſe ſubſtances.

3. As Sir James has neglected giving a *compleat* account of the external characters of the natural whins, which were the ſubject of his experiments, as alſo of the regenerated, or artificial whins derived from them; and as I have not myſelf ſeen them, it is difficult for me to compare them with each other, and would, indeed, be impoſſible, if ſome account of them had not been given by Mr. Pictet, in his valuable *Journal Britannique*, copied into the 5th Vol. of the new Rozier's Journal, p. 313. It is the reſult of the examination both of the natural and artificial whins by the Society of Natural Hiſtory at Geneva.

As to the grunſtein, No. 1. they remark, that it betrays not the leaſt mark of an igneous origin, but that the whins which Sir James produced from it, had every diſtinctive character of a lava, and even of a porous lava.

The Baſalt (or rather Trapp) on which the Caſtle of Edinburgh ſtands, is of a compact ſtructure; the artificial produced from it, Sir James tells us, ſo greatly reſembles it both in colour and texture, that it would be difficult, or perhaps impoſſible, to diſtinguiſh them, *but for a few minute air bubbles, diſtinguiſhable in the artificial.* Neptuniſts will, however, conſider this as a leading character of diſtinction. The mineralogiſts of Geneva add, that the colour of the artificial is deeper, and its hardneſs greater, than that of the natural. If the ſpecific gravity, and other characters of both were given, it is probable that other differences might be perceived. It is only in theſe characters that any difference can be expected, as the internal compoſition muſt be the ſame in both.

Of the remaining artificial whins I can give no account, their external characters having been omitted; I cannot, however, paſs over the general inferences that Sir James deduces from his experiments, namely, that "the arguments againſt the ſubterraneous fuſion of "whinſtone, derived from its ſtony character, ſeem now to be fully refuted," for not to repeat what has been already ſaid, that many of them contain ſubſtances whoſe exiſtence is incompatible with that hypotheſis, I muſt farther add, that the upright ſtate in which many of them exiſt, for inſtance the baſaltic pillars of Staffa, and of the Giant's Cauſeway, and of many other countries, the baſis they reſt on, ſometimes granite, ſometimes gneiſs, ſometimes coal or limeſtone, and the total abſence of all ſigns of the operation of fire, forbid us to entertain any doubt of their production in the moiſt way. Nay, the College of Dublin now poſſeſſes fragments of baſaltic pillars, in which marine ſhells are imbedded; if ſuch evidence can be reſiſted, it is in vain to ſeek for greater.

Sir James thinks the cauſe of the fluidity of lavas, which I formerly ſuggeſted, as ſtrange and inconceivable as that of Citizen Dolomieu. Not having had the happineſs of viewing thoſe ſtupendous torrents, I founded my opinion on the accounts given by the moſt accurate obſervers, and particularly of C. Dolomieu, who beheld, and carefully examined, every circumſtance relating to them for many years. This great obſerver has not thought my opinion ſo inconceivable, for he has ſince embraced it. "From the manner" (ſays he) "in which lavas flow, it cannot be doubted, but they carry with them a ſubſtance capable of maintaining their heat and fluidity, and contain a ſubſtance which burns in contact with the atmoſphere until it is conſumed. This ſubſtance, of which ſulphur is at leaſt one of the principal ingredients, if it be not the only one, bears a ſtrong reſemblance in its conſtitution to phoſphorus, being capable of two ſorts of combuſtion. This combuſtion ſeems capable of maintaining fluidity in a bed of lava, &c." 1 New Rozier's Journ. p. 119 & 120.

Sir James ſays, I have ſuppoſed ſubſtances that have left no trace of their exiſtence. Other obſervers, however, diſcovered theſe traces, as Dolomieu and Fabroni, in the paſſages I have already quoted. Mineralogy, Vol. I. p. 397, and 1 New Roz. p. 120, 121. It is not to be expected that volatile ſubſtances, ſuch as ſulphur and petrol, ſhould long remain. However, I acknowledge that the cauſe of the ſtony appearance which lavas after cooling exhibit, diſcovered by Sir James, appears to me at preſent by far the moſt probable; and that in this reſpect his diſcovery is of great importance to geology. But I perſiſt in thinking, his experiments afford no conformation of the high degrees of heat attributed to volcanos, and ſtill leſs to the many hypotheſes gratuitouſly heaped on each other by Dr. Hutton, or to the volcanic origin of whins or traps, for the reaſons already aſſigned.

III.

On the Genuineneſs and Purity of Drugs and Medicines. By Mr. FRED. ACCUM.

(Continued from page 36.)

METALS AND METALLIC SUBSTANCES.

Mercury. Quickſilver.—Ph. L.

THE property which mercury poſſeſſes of readily uniting with moſt of the metals, without ſuffering any material change in its obvious properties, induces fraudulent dealers to adulterate this article with lead, tin, &c. If theſe metals are preſent in any conſiderable quantity, the mercury will have a dull aſpect, and will not run freely into round globules, but when gently moved, it forms vermicular ſtriæ, ar tears. When agitated in a bottle, it ſoon becomes black, the lead and tin becoming oxidated, and may thus be ſeparated, as has been noticed in the Philoſophical Journal I. But as theſe metals are ſo eaſily detected, and can only be added in a comparatively ſmall quantity, the dealers in this article have recourſe to other practices. It is a fact, that biſmuth and zinc remarkably favor the union of lead with this metallic fluid, and that even tin may then be added in a conſiderable quantity. As it is impoſſible to free mercury from theſe metals completely by a mere agitation, recourſe muſt be had to diſtillation; and even then the mercury cannot be obtained perfectly pure, unleſs the proceſs be very carefully managed.

Mercury free from any admixture ſhould be totally volatile, when gently heated to the boiling point. In order to detect the preſence of lead, one part muſt be boiled for a few minutes with twelve parts of electric acid. The fluid is then to be decanted, and examined by means of ſulphuric acid. If the ſmaller quantity of lead be preſent, a few drops of this acid will render the fluid turbid, and a white precipitate will be ſeparated. One hundred grains of this precipitate, well waſhed and dried, contains 72 parts of metal, and 28 of acid; the quantity of lead contained in a given quantity of mercury, may thus be accurately aſcertained. But the minuteſt quantity of lead in the greateſt quantity of mercury, can only be detected by diſſolving the mercury in nitric acid, and then mixing this ſolution with water ſaturated with ſulphurated hydrogenous gas. Dark brown clouds will immediately appear, and a preeipitate of the ſame colour will be depoſited, if the fluid is ſuffered to reſt undiſturbed for a day or two, one part of lead may thus be ſeparated from 15260 parts of mercury. The admixture of biſmuth is detected in a ſimilar manner, by pouring the nitrous ſolution into diſtilled water; a white precipitate will appear if this metal be preſent. Tin is manifeſted in the uſual manner, by a weak nitro-muriatic ſolution of gold; and zinc, like the reſt of the metals, may be ſeparated by expoſing the mercury to heat.

Sweet

Sweet Muriate of Mercury. Calomel.—Ph. L.

Calomel ought to be perfectly ſaturated with mercury. Compleat ſaturation can only be known, by boiling for a few minutes one part of calomel, and $\frac{1}{32}$ part of muriate of ammoniac in 10 parts of diſtilled water. The fluid muſt then be filtrated and examined by means of carbonate of pot-aſh. If the calomel is well prepared, no change will take place on the addition of this re-agent; but if the preparation is imperfect, a precipitate will enſue, 47 parts of which indicate 48 of muriatic acid. 114 parts of calomel perfectly ſaturated with mercury, contain 97 of metal, and 17 of acid. It ſhould be perfectly inodorous and taſteleſs, and when rubbed in a ſtone mortar with ammoniac becomes intenſely black.

Red ſulphurated Oxide of Mercury. Red ſulphurated Quickſilver.—Ph. L.

The cinnabar of the ſhops is generally adulterated with red oxide of lead, in order to make it dry ſooner when uſed for oil painting, To diſcover this reprehenſible focus, one part of cinnabar is to be digeſted by heat with four of acetic acid. If lead is preſent, the acid will acquire a ſweetiſh taſte, and by letting fall into it a few drops of ſulphuric acid, ſulphate of lead will inſtantly be ſeparated. Genuine red ſulphurated oxide of mercury ſhould be entirely volatile in the fire, and conſequently leave nothing behind it after evaporation. A compleat decompoſition of its integrant parts ſhould be effected, by boiling one part of it in twelve of nitro-muriatic acid, (compoſed of three parts of nitric, and one of muriatic acid) and then treating the reſidue in a ſimilar manner, with a ſufficient quantity of ſoda, or pot-aſh, forced from carbonic acid. Cinnabar is likewiſe found adulterated with a mixture of chalk and dragon's blood, and cinnabar thus contaminated, is of an exceedingly fine crimſon colour. If it contains carbonate of lime, an efferveſcence will enſue on muriatic acid being added, and the earth will be taken up by the acid; on adding diſtilled water, and ſeparating the fluid, chryſtals of ſelenite will be obtained, on adding a little ſulphuric acid. The colouring matter may be compleatly extracted by digeſting the reſidue in ardent ſpirit, and ſubſequent evaporation. 100 parts of Engliſh cinnabar contain generally 20 parts of ſulphur, and 80 of mercury. Its ſpecific gravity is then 1000.

Black Oxide of Mercury. Quickſilver with Sulphur.—Ph. L.

This mixture of quickſilver and ſulphur, ought to be at leaſt ſo far completed, that no globules of mercury ſhould be perceptible through a good microſcope. Its colour is then of a beautiful black; it is impalpable to the touch, and totally volatile by heat. It is found ſophiſticated with ivory black. If this be the caſe, it will leave ſome aſhes behind when laid on a red hot iron, or on ignited coals. If the mixture of the mercury with the ſulphur has been well performed, the powder does not give a white colour to gold, when rubbed on it for any conſiderable time, nor can a ſeparation be eaſily effected by ſaturating it with a thick mucilage, &c. However, black oxide of mercury, which ſtands theſe teſts, will not be found in commerce, nor can it reaſonably be expected.

Nitrated Oxide of Mercury. Red Nitrated Quickſilver.—Ph. L.

Moſt of the red nitrated oxide of mercury met with in the ſhops is adulterated with red oxide of lead. This fraud may be diſcovered in the ſame manner, as in the red ſulphurated oxide of mercury. Genuine red nitrated oxide of mercury is totally volatile on being expoſed to a red heat, and ſoluble in nitric acid, without effervеſcence, but with a developement of heat. Sulphuric acid converts it into yellow oxide of mercury (Turpeth mineral); muriatic acid forms corroſive muriate, and when completely ſaturated, ſweet muriate of mercury, or calomel *.

White Oxide of Mercury. White Calx of Quickſilver.—Ph. L.

Inſtead of pure white oxide of mercury, we frequently meet with a mixture of white oxide of mercury, and white oxide of lead, to which, not ſeldom, a conſiderable quantity of chalk is intimately mixed. White oxide of lead is diſcovered by digeſting one part of the oxide with four of acetic acid, decanting the fluid, and adding to it a ſmall quantity of ſulphuret of ammoniac, or water impregnated with ſulphurated hydrogen. An almoſt black precipitate will be formed, which on the addition of ſulphuric acid will not be rediſſolved. The preſence of calcareous earth may be inveſtigated as directed before.

Genuine white oxide of mercury is of a ſnowy whiteneſs, taſteleſs, and inodorous; inſoluble in acids, ponderous; does not become black when rubbed with freſh prepared lime water, is totally volatiliſed by heat, and when accurately prepared, may thus be converted into calomel by mere ſublimation.

Yellow Oxide of Mercury. Vitriolated Quickſilver.—Ph. L.

Is ſeldom found adulterated. But from a careleſs and ſlovenly management during the proceſs for obtaining it, it often contains a conſiderable quantity of free ſulphuric acid. It then has a perceptible acid taſte, and diſturbs the ſolution of muriate of barytes. It ſhould be totally volatile by heat, and poſſeſs moſt of the characteriſtics of the preceding mercurial preparations.

Sweet Muriate of Mercury obtained by Precipitation. Mild Muriated Quickſilver.—Ph. L.

This preparation being preciſely the ſame as calomel, ought therefore to poſſeſs all its peculiar properties. But as it is ſpecifically lighter than calomel, it is found adulterated with ſtarch, or with white oxide of biſmuth. The firſt fraud may be detected by expoſing it in a cloſe veſſel to a red heat; as by this means the calomel will ſublime, and leave a black coal behind, and the latter ſophiſtication is detected by expoſing it to heat in an open veſſel, and reducing the reſidue by means of a little charcoal to the metallic ſtate, for farther inveſtigation.

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* This product, if obtained according to the rules of the Royal College of Phyſicians, muſt be contaminated with a certain quantity of corroſive ſublimate, on account of the preſcribed quantity of muriatic acid to be uſed. Did the guardians of health wiſh to unite a certain quantity of this deadly poiſon with the red oxide? or did they credit the notion, that the product would have a more ſparkling appearance, which in general is looked upon as the characteriſtic of excellency, &c.?

Red Oxide of Mercury. Calcined Quickſilver.—Ph. L.

Is ſeldom found adulterated. If well prepared it is totally volatile by fire. It is ſoluble in ſulphuric, nitric, and muriatic acid, but the acetic, oxalic, malic, and tartareous acids have no effect upon it.

Corroſive Muriate of Mercury. Muriated Quickſilver.—Ph. L.

It has been ſaid that this product has been found adulterated with arſenic. Though this may probably ſeldom be the caſe, yet when it is ſuſpected, one part of the corroſive ſublimate may be diſſolved in 24 parts of diſtilled water, and precipitated again by carefully adding a ſolution of carbonate of pot-aſh. The white precipitate thus afforded, is then ſeparated from the ſupernatant fluid. This fluid thus freed from all the mercury it contained, is then to be tried by means of the ammoniate of copper. A tranſparent blue liquid will be formed, if the corroſive muriate of mercury was free from arſenic; but a yellowiſh green precipitate will appear if arſenic was preſent. This precipitate collected, dried, and laid on ignited coals, will ſoon diffuſe the garlic ſmell peculiar to that metal. Two hundred and ſixty-ſeven parts of this precipitate, contain one hundred and ſixty-two parts of copper, and one hundred and ſixty-five of arſenic.

Sixty-nine parts of corroſive muriate of mercury contain, if well prepared, generally 50 parts of mercury, 17 of ſolid muriatic acid, and two of water of cryſtalization. One part is then ſoluble in ſixteen parts of cold water at 50°, in $3\frac{1}{2}$ of boiling water, in $2\frac{1}{2}$ of cold ardent ſpirit, or in a little more than its own weight of boiling ardent ſpirit.

Iron. Iron.—Ph. L.

The iron filings of the ſhops, which in general are procured from the gun-ſmiths, and other artificers, are never free from copper or braſs. If ſome of the filings be diſſolved in pure muriatic acid, and a poliſhed iron be immerſed into this ſaturated, and concentrated ſolution, the part of the metal in contact with the fluid, will ſoon become coppered, if the quantity of this metal was conſiderable. A ſmaller quantity of copper is manifeſted by digeſting the iron filings, in water impregnated with ammoniac, which will acquire a blueiſh hue, if the filings contain copper. But the beſt way to detect the minuteſt quantity of copper or braſs is, to diſſolve one part of the ſuſpected iron in three of nitric acid, and to decompoſe this ſolution by the addition of carbonate of pot-aſh, or ammoniac freed from carbonic acid. In the firſt caſe, a greeniſh precipitate, and in the latter, a blue ſolution will be obtained, particularly if the ammoniac is added in abundance.

The practice of purifying iron filings uſually made uſe of in the ſhops, by means of a magnet, is not ſo perfect as is generally expected. When other metals have been united with the iron, as is the caſe in various ſolders, the mere iron filings of the compound can not be ſeparated in this manner. And ſo likewiſe braſs, copper, and zinc filings, which adhere to thoſe of the iron, are likewiſe forced to obey the magnet, and render the operation ſo far ineffectual.

Sulphate of Iron. Vitriolated Iron.—Ph. L.

Should conſiſt entirely of iron united to ſulphuric acid. The vitriolated iron of the ſhops frequently contains copper, on account of its being in general prepared, by merely re-diſſolving the common green vitriol of commerce, which abounds with copper. The beſt way of detecting the preſence of copper, conſiſts in diſſolving one part of the ſulphate of iron, in three of diſtilled water, precipitating this ſolution with carbonate of pot-aſh, and then letting fall into it a little ammoniac. If the ſmalleſt quantity of copper is preſent, the fluid will acquire a ſapphire blue colour, but no ſuch effect will take place if the ſalt be free from copper.

The practice of obtaining this ſalt pure, by diſſolving the common green vitriol, and ſubſequent boiling with iron filings, or cryſtalization in an iron veſſel, is not ſo ſatisfactory as could be wiſhed. Sulphate of zinc, which is frequently found in the common ſulphate of iron of commerce, does not become ſeparated in this manner. A direct combination is requiſite for obtaining this ſalt perfectly pure.

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IV.

Extracts of Letters from H. Goodwyn, Esq. on the Unities of Weight and Meaſure beſt adapted to the Britiſh Empire; on the new Meaſures of France; with a Deſcription of an Engine for raiſing Water.

I APPREHEND it to be an indiſputable point that a correſponding unity of weight and meaſure is truly deſirable in this kingdom; and that an additional benefit, beyond what the French nation poſſeſs from their new metrical ſyſtem, would ariſe, if that unity correſponded alſo with the general method in which the comparative ſpecific gravities of bodies are expreſſed with us, namely, by an unit, or by one thouſandth part. Now, Sir, it appears to me that we are not only in poſſeſſion, but in the conſtant uſe of one, both for weight and meaſure, as invariable as that now eſtabliſhed in France. I allude to the foot meaſure and avoirdupoiſe, or (if I may be allowed the more appropriate expreſſion) decade ounce weight. When I add, what I am perſuaded, by your valuable publications, you well know, that the decade ounce weight, of pure rain, or diſtilled, water at 60° of heat, is generally allowed to be equal in bulk to the $\frac{1}{1000}$th part of the cubic foot; and alſo obſerve that by adding only 44.3511 parts out of 10000; or about $\frac{1}{225}$th part, to the content of our preſent *Wincheſter meaſure*, that *it* would then contain exactly 10 cubic feet. Theſe tranſitions from the preſent to ſuch an improved ſyſtem, would be "a conſummation devoutly to be wiſhed," and moſt ardently do I wiſh it to be legalized.

A ſtandard meaſure for the purpoſes of trade, in particular, as well as for others, that would uniformly give an accurate reſult, and could be eaſily made, examined and aſcertained, by common mechanics, which neither our preſent liquid or dry meaſures evidently can, would ſurely be an acquiſition of great value. Such an one, I humbly preſume, would be the following:—A ſquare pyramid, whoſe perpendicular height is exactly thrice the length of the ſide of the baſe: for ſuch an one, and every ſection of it, made by a plane parallel to its baſe, would, in the firſt inſtance poſſeſs, and in every ſubdiviſion, retain theſe remarkable properties*.

1ſt. Similar comparative dimenſions to thoſe above given, for the original pyramid, *i. e.* every ſmaller pyramid, formed by the above-mentioned parallel ſection, would have its perpendicular height thrice the length of the ſide of its baſe, and

2dly. The length of the ſide of each baſe will always indicate, or equal the cube root of the ſolid content of the pyramid *e. g.* If the length of the ſide of the baſe be 3. the ſolid content will be the cube of 3. viz. $3 \times 3 \times 3 = 27$.

Mr. Locke has ſomewhere in his writings ſtated it as wrong for one man to pull down the ſuperſtructure of another without building or erecting a better in its ſtead. I ſome time paſt took the liberty to point out to you an error in the comparative tables of Engliſh and French meaſures in your Journal; and upon Mr. Locke's principle I feel it juſt to give you my calculation of the general heads of comparative tables between ſuch a ſyſtem as is advanced in the former part of this paper, and that I apprehend to be now eſtabliſhed in France, premiſing, that I take the length of the mètre from your Journal, vol. III. page 283, at

pieds.	pou.	lignes.
3	0	11.296,

and I take the comparative length of the Engliſh, with the French foot, from data in the Connoiſſance des Temps for 1795, and from the Philoſophical Tranſactions for 1768, page 326.

By theſe references it will appear that the French foot is to the Engliſh

as 1. : 1.065752004, &c.

Conſequently,

French foot.		Engliſh decades, or 10ths of an Engliſh foot.
1.	=	10.65752004

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* I have been many years in the habit of uſing a pyramid meaſure to examine corn; and am perfectly convinced that ſuch a one will indicate a far more accurate reſult than can ariſe from the manner in which corn is meaſured by the buſhel.---G.

Comparative Tables, Engliſh with French.

LONG MEASURE.

Long decade.		Métre.	Métre.		Long decades.
1	=	0.03047983 ferè	1	=	32.808583358, &c. or inches 39.3703

SQUARE MEASURE.

Square decades.		Ares.	Ares.		Square decades:
1	=	0.0000092902 ferè	1	=	107640.3142 or ſqr. inches 155002.052448.

CUBE MEASURE.

Cube decades.		Litres.	Litre.		Cube decades.
1	=	0.028831637 ferè	1	=	35.3152622, &c. or cubic inches 61.0247727.

WEIGHTS.

Avoird. or decade oz.		Grammes.	Gramme.		Decade oz.
1	=	28.31637 ferè	1	=	0.03531526, &c. or grains or 15.45042625.

Long, Square, or Cube.	decades are reduced to	Long, Square, or Cube.	Engliſh inches by multiplying by	1.2 1.44 1.728

And decade ounces are reduced to grains,

containing,	7000. or 5760.	to the lb.	Avoird. Troy	by

multiplying the ounce by 437.5 = the number of grains in an avoirdupoiſe ounce.

Eaſt Smithfield, April 25, 1800.

Deſcription of an Engine for raiſing Water.

THE hydroſtatic machine here deſcribed was invented by me ſome years ago. It has been ſeen but by few: Mr. Watt, of Birmingham, Mr. Rennie, the civil engineer, Mr. More, the late ſecretary to the Arts and Sciences, Mr. George Adams, late of Fleet-ſtreet, who has made handſome mention of it in his Lectures. I have no doubt but you can readily point out many uſes to which it may be applicable.

May 17, 1800.

H. G.

A Section

A Section and Deſcription of a Machine that will raiſe a Body of Water to any Height, not exceeding the Height of a Column that will counterbalance the Preſſure of the Atmoſphere (ſay 30 *Feet) by the Deſcent of Part of the ſame Body of Water, through a ſomewhat greater Height, and aided by the Preſſure of the Atmoſphere.*

Let A	Plate VIII. figure 2, be a ſphere of copper, or other metal, about 1 foot 6 inches in diameter.
B	Another ſphere about 2 feet 6 inches in diameter.
C	A reſervoir of water kept conſtantly ſupplied.
D	A glaſs cap about 6 inches long, fixed on the top of A, for the purpoſe of ſeeing when the water begins to fill and has filled A.
E	The upper reſervoir into which A is to be emptied.
1,1,	A pipe, about 0.5 inches in diameter, fixed into the top of B, and riſing upwards to within about an inch of the top of D.
2	A pipe of the ſame diameter, a few feet longer than 1,1, fixed into the bottom of B, and extending downwards in a perpendicular or inclined direction.
3	A pipe of one inch and a half in diameter, fixed to bottom of A, and extending upwards through it, to within about two inches of the top of D. *Vide* fig. 3.
4,4,	A pipe about half an inch diameter fixed into the top of B, and extending upwards, through, and fixed to bottom of C.
5	A pipe of ſame diameter fixed to top of B, and terminating in and fixed to bottom of C.
6	A pipe of ſame diameter fixed into bottom of A.
7	A trumpet mouth pipe fixed to bottom of pipe 3, and extending downwards, to within about an inch of bottom of reſervoir C.
a, *b*, *c*, & *d*	Are cocks fixed to the pipes.

The ſpheres, pipes, cocks, and joints, muſt all be air tight.

In order to raiſe water from reſervoir C into reſervoir E, all the cocks being turned off, proceed thus: turn on the cocks *b* and *c*, in order to fill B, and when B is full, turn off the cocks *b* and *c*, and turn on the cock *d*. The water will then begin to run from the ſphere B, and will, by means of its communication through the pipe 1, 1, with the upper ſphere A, rarify the air therein, and the atmoſphere at the ſame time preſſing on the water in C, will cauſe it to riſe through the trumpet mouth at 7, of pipe 3, and by falling over the top of it at D to fill the ſphere A. When A is full, which may be ſeen through the glaſs cap D, turn off *d*, and turn on *a*, *b*, and *c*, by which means A will empty into the reſervoir E, and B will be repleniſhed for another operation. Then turn off the cocks *a*, *b*, and *c*, and turn on the cock *d*, and repeat the operation of raiſing the water into A. But if it be required to raiſe *one body* of water from reſervoir C into reſervoir E, by means of the deſcent of *another body* of water through B, a communication muſt be made into B, independant of the pipe 5 and cock *c*, viz. through a pipe, cock, and funnel (or reſervoir) repreſented by the dotted lines, communicating with B, near the pipe and cock 5 and *c*, and then they are to be uſed in lieu of pipe 5 and cock *c*.

V.

A Memoir, in which the Queſtion is examined, whether Azote be a ſimple or compound Body? *By* CHRISTOPHER GIRTANNER, *Doctor of Phyſic at Gottingen.*

(Concluded from page 140.)

I PROPOSE, in a particular work upon azote, to give a detail of the numerous experiments which I had made to diſcover its nature: I ſhall confine myſelf at preſent to give a general outline of theſe experiments, as well as of the inductions which I have thought might be drawn from them.

I think myſelf intitled, therefore, to affirm without ſelf deception, that azote gas is obtained.

1. When water is boiled in an earthen retort not glazed within, and the vapour is made to paſs through a tube of glaſs, or other material;

2. When water is boiled in a retort of glaſs containing clay or alumine, and the vapour is made to paſs through a tube of glaſs, or other material;

3. When water is boiled alone in a retort of glaſs, and the vapour is made to paſs through a tube of pipe clay;

4. When water is boiled alone in a glaſs retort, and the vapour is made to paſs through a glaſs tube containing clay or alumine;

5. When an earthen tube is filled with water, and this tube is included in a larger tube of glaſs with ſand between them, and this tube of glaſs in tube of iron alſo with ſand between them, and the whole is expoſed to ignition *;

6. When water is boiled in a glaſs retort containing lime, and the vapour is paſſed through a tube of glaſs or other material;

7. When water is boiled in a glaſs retort containing pounded quartz, or ſilex, and the vapour is paſſed through a tube of glaſs, or other material;

8. Experiment 1 ſucceeds equally when the earthen retort is coated *externally* with a metallic glaze:

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* This experiment, which I have not repeated, is given by Prieſtley, in his experiments and obſervations on different kinds of air. Birmingham, 1790, vol. ii. page 418.

9. Experiment 3 ſucceeds equally when the earthen tube is coated *externally* with a metallic glaze:

10. Experiment 4 ſucceeds equally when the tube is filled with lime, or pounded glaſs.

The vapours of water only without azote gas are obtained,

1. When water is boiled in a glaſs retort, and the ſteam is paſſed through tubes of glaſs or porcelain;

2. When water is boiled in an earthen retort, internally coated with a metallic glaze, and the vapours are paſſed through tubes of glaſs or porcelain;

3. When water is boiled in a glaſs retort filled with pounded glaſs, and the vapours are paſſed through the tubes of glaſs or porcelain:

GENERAL REMARKS.

In order to obtain azote gas in conſiderable quantity, the water muſt be gently evaporated by a very moderate heat, and care muſt be taken not to raiſe it.

It may be obſerved, that in all theſe experiments without exception, that as ſoon as the laſt drops of water is evaporated, the azote gas ceaſes to be produced, notwithſtanding the continuance of the heat.

RESULTS OF THE EXPERIMENTS.

Such are the facts, and the ſimple enumeration of experiments, independent of all ſyſtem, explanation, and theory. And it follows,

1. That Meſſrs. Wiegleb and Goettling were right in affirming, that water is changed into azote gas by the action of heat;

2. That they were wrong in maintaining that this change takes place always, and in all circumſtances, provided the vapours of water be in contact with ignited bodies;

3. That Mr. Wiegleb has not proved what he advanced, namely, that the change of water into gas is owing to caloric, and that water is the baſis of every gas whatever.

4. That the Dutch chemiſts were wrong in advancing, that the change of water into azote gas never happens in any caſe, and that the gas obtained is owing to the atmoſpheric air having paſſed through the retorts and the tubes;

5. That there are circumſtances in which water is changed into azote gas, and others in which it is not, and that it is eaſy to reconcile the two oppoſite parties.

The change of water into azote gas by the action of caloric and the earths being affirmed, it remains for us to diſcover the ſolution of this problem.

I have obſerved, as well as Ingenhouſz, Von Homboldt, and Van Mons, that the earths when moiſtened, have the property of abſorbing the oxigen of the atmoſphere at the ordinary temperature. I have alſo obſerved what Von Homboldt has not remarked, that they abſorb oxigen in leſs time, and greater quantity, when they are heated. I found by other experiments, that the earths alſo take oxigen from water, but that it is neceſſary for this purpoſe, that the temperature ſhould be more elevated than that of the atmoſphere.

Clay, loamy earth, or alumine, become charged with oxigen with the moſt avidity, and at a temperature far below that of boiling water. Lime requires a more elevated temperature, and does not abſorb ſo much oxigen. Silex requires to be ignited before it will abſorb oxigen, but then it ſeizes it rapidly enough. Baked clay alſo unites with oxigen, but requires a ſtill more elevated temperature. Glazed clay does not abſorb oxygen, becauſe the glaze being a metallic glaſs, has no action upon that ſubſtance.

Having made theſe obſervations, I did not find much difficulty in explaining the phenomena of the change of water into azote gas: Recollecting the hypotheſis which was thrown out by Mr. Mayer ſome years ago, on the nature of azote gas, which he ſuppoſes to be compoſed of oxigen and hydrogen, or that it is water changed into gas *, I adopted ſimilar ideas reſpecting the nature of this gas. Mr. Mayer having given his hypotheſis merely as a conjecture, unſupported by any chemical experiment whatever, I propoſed to ſupply what was wanting in the eſtabliſhment of his concluſion.

Admitting this hypotheſis, and reflecting on the ſingular property which the earths poſſeſs of abſorbing oxigen from water, I explained the before related experiments without difficulty. They are the conſequence of a double affinity. The oxigen of the water partly unites with the earth, and converts it into an earthy oxide; the reſt of the oxigen uniting with the hydrogen combines with the caloric, and forms azote gas; whence it follows, that *azote is water deprived of part of its oxigen.*

Several experiments preſent themſelves in ſupport of this aſſertion. I ſhall confine myſelf to mentioning ſome of them, and ſhall ſpeak of the others in a ſecond memoir, when I ſhall have repeated them.

1. Let tubes of porcelain be procured and tried, by paſſing the vapour of boiling water through them, which will be condenſed at the other extremity without the ſmalleſt particle of gas, excepting the air which is contained in the retort. After having in this manner aſcertained that the water is not changed into gas in theſe tubes, in order to obviate the objections of thoſe who imagine, that the external air paſſes through theſe tubes; let one of them be filled with tin filings and placed in a fire, taking care to keep it up: after which the vapours of water are to be paſſed through.

The pneumatic apparatus will be found to contain azote gas mixed with oxigen gas. The tin is changed into oxide, which oxide paſſes under the pneumatic apparatus with the azote gas, and the vapours of the water.

This experiment, which was made by Mr. Van Hauch, is eaſily explained according to my theory. The tin takes only part of the oxigen from the water. The reſt unites with hydrogen and forms azote.

2. When the vapours of water are paſſed over lead in the ſame manner as in the foregoing experiment, a ſimilar reſult is obtained according to Mr. Van Hauch. At the commencement of the operation, the oxide of lead paſſes into the inverted receiver along

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* Gren's Journal der Phyſik, vol. v. page 381.

with the gas, and the watery vapours; after which the metal itſelf appears in the form of an extremely fine powder. The proportion of azote gas to that of the oxigen obtained is as 64 to 36.

3. When the vapours of water are paſſed in the ſame manner through a tube filled with antimony, a mixture of azote and oxigen gas is obtained, in the proportion of 89 parts of the former to 11 of the latter.

4. A porcelain tube was filled with the black oxide of manganeſe. The tube was expoſed to a very ſtrong fire for two hours, till the oxide had intirely ceaſed to afford oxigen gas. The vapours of water were then paſſed over this oxide thus deprived of the greater part of its oxigen. Another product of oxigen gas of conſiderable purity, was firſt obtained, and afterwards azote gas.

This experiment of Mr. Van Hauch is very inſtructive. The manganeſe firſt abſorbed the hydrogen with avidity, and afterwards part of the oxigen of the water.

5. Through the ſame tube filled with the ſame manganeſe, as had ſerved for experiment 4, the vapours of water were paſſed. Azote gas was obtained. The fire was kept up for near three hours, and the diſengagement of azote continued as long as the vapours were paſſed through. It ceaſed when theſe vapours were ſtopped, but the diſengagement of azote began again as ſoon as the water was again made to boil, and the vapours began to paſs over the manganeſe. This experiment was repeated for ſix following days during three hours each day, and always with the ſame ſucceſs. When the operation was ended, the manganeſe was found adhering to the porcelain, ſo that it could not be ſeparated.

6. Dr. Pearſon, in his analyſis of water by the electric ſpark always obtained azote gas, beſides the two gaſes which compoſe water.

7. The ſame Dr. Pearſon, by burning a mixture of oxigen and hydrogen gaſes in a tube hermetically cloſed, obtained water and azote gas.

8. Dr. Prieſtley obſerved, that oxigen gas, which remains for a time in contact with the pureſt diſtilled water, becomes partly changed into azote. I verified this obſervation, and found as much as 0,1 of azote gas in the oxigen gas.

9. A mixture of hydrogen gas and nitrous gas, which remains for a time in contact with water, no longer burns, but is found to be changed into atmoſpheric air. I explain this fact by ſuppoſing, that the hydrogen has combined with part of the oxigen of the nitrous gas, or of the water, and has become changed into azote gas. This experiment, however, which was made by Mr. Link, did not ſucceed with me

10. Dr. Prieſtley found that hydrogen gas, which he had kept for a long time in contact with water, was intirely changed into azote gas. This experiment does not always ſucceed. It ſucceeded four times with Prieſtley. It is no doubt neceſſary that the water ſhould contain oxigen gas.

11. By burning together a mixture of eleven cubic inches of hydrogen gas, and one cubic inch of oxigen gas, azote gas is obtained. This experiment of Mr. Yelin did not ſucceed with me.

12. When the vapours of water are paſſed through a gun-barrel, which has already been uſed a number of times in this kind of experiment, and is intirely oxided within, hydrogen gas is no longer obtained, but azote; the iron being no longer capable of combining with all the oxigen which the water preſents to it. This experiment of Mr. Yeling has been confirmed to me by Mr. Mayer.

13. An experiment made by Mr. Lampadius, appears to me to afford very ſatisfactory proof that oxigen is contained in azote gas. He fuſed arſenic in the pureſt azote prepared by the combuſtion of phoſphorus. The metal was ſublimed, and after the experiment, he found that it was partly changed into oxide of arſenic.

14. When in the combuſtion of the two gaſes to produce water, the quantity of hydrogen is too great, a product of nitric acid is obtained.

15. The following experiment deſcribed by Scheele, appears to me to be a new proof that azote is nothing elſe but an oxide of hydrogen. Thus it is that this famous chemiſt expreſſes himſelf, in his treatiſe on Air and Fire, "I filled a bladder with air obtained " from iron filings, diſſolved in vitriolic acid, and I reſpired this air. After twenty in- " ſpirations I found myſelf obliged to ſtop. When I had recovered, I emptied my lungs " as much as poſſible, and again reſpired the inflammable air. After ten inſpirations I " could proceed no farther. The air upon examination was no longer inflammable, and " produced no cloud in lime water; in a word it was corrupted air" (azote gas).

(To be continued.)

VI.

Further Remarks on the Preparation of Pruſſiate of Pot-aſh—Method of purifying Cauſtic and carbonated Alcalis from Sulphate of Pot-aſh. By Mr. WILLIAM HENRY.

Mancheſter, June 16, 1800.

TO MR. NICHOLSON.

SIR,

IN your Journal for April laſt (p. 30), I communicated a new method of preparing the pruſſiate of pot-aſh. The only objection I am aware of, that can be urged againſt this proceſs, is the expenditure of a conſiderable quantity of pruſſiated barytes, a ſalt which it may not be in the power of every one to prepare. As the ſole advantage, obtained by its employment, is the decompoſition, by double affinity, of ſulphate of pot-aſh; it has ſince occurred to me, that a conſiderable ſaving of pruſſiated barytes would be gained, by firſt ſaturating the alcali, in the common way, with Pruſſian blue, and afterwards digeſting the impure pruſſiate of pot-aſh, thus obtained, with pruſſiate of barytes. To a ſolution of common pruſſiate of pot-aſh, which immediately precipitated muriate of barytes, I added,

therefore,

therefore, a ſmall quantity of the barytic pruſſiate, and applied a gentle heat for a few minutes. The ſolution, when filtered, no longer precipitated muriate of barytes (except after ſome time in the manner I have pointed out, p. 32.) The purer the alcali, employed in making the pruſſiate of pot-aſh, the leſs, it is obvious, will be the proportion of pruſſiated barytes required.

I have lately contrived a method of preparing pure and carbonated alkalis, which has many advantages over the common ones. Indeed the formation of a carbonated alcali, perfectly free from ſulphate of pot-aſh, is a problem of conſiderable difficulty, except by very expenſive proceſſes. Cryſtalization is incompetent to ſeparate the whole of the ſulphate. The lixivium of the aſhes of tartar, and the alcali prepared from nitre, I have never found free from ſulphate of pot-aſh. By ſaturating the alcali with acetous acid, and then adding acetite of barytes, a pure acetite of alcali may be obtained, which, when decompoſed by heat, affords a pure carbonate. But this mode is troubleſome and coſtly. I recommend the following, as a cheap and eaſy proceſs:

Render the alcali, whether vegetable, mineral, or volatile, perfectly cauſtic by quicklime; and to the clear ſolution add, by degrees, a warm ſolution of pure barytic earth, till the precipitation ceaſes. The barytes ſeizes the ſulphuric acid; and leaves the alcali pure, which may, if required, be afterwards ſaturated with carbonic acid, in any of the common modes. During its reſtoration to a mild ſtate, any barytes, that may remain in exceſs, is alſo precipitated. If the carbonic acid, employed for ſaturation, be obtained from carbonate of lime by ſulphuric acid, the gas ſhould be previouſly paſſed through a ſolution of carbonate of pot-aſh, in order to ſeparate any ſulphuric acid, that may happen to be attached to it. A carbonate of alcali thus prepared, when ſaturated with acetous acid, gives no ſigns of ſulphuric acid, on adding acetite of barytes.

Cauſtic alcali, however, purified in the above mode, ſtill retains a ſmall portion of lime; for, on examining the precipitate very attentively, I found no lime in its compoſition, which might perhaps have been expected in it, from ſome late experiments of M. M. Guyton, Kirwan, and Hatchett, on the affinity of earths in the humid way. If a cauſtic alcali be required, perfectly free from lime, as may ſometimes happen in nice experiments, the alcali may be deprived of its carbonic acid, entirely from the beginning, by pure barytes.

I am, Sir,

Your obedient humble Servant,

WILLIAM HENRY.

On a

VII.

On a New Fulminating Mercury. By EDWARD HOWARD, *Eſq. F. R. S*[*].

SECTION I.

THE mercurial preparations which fulminate, when mixed with ſulphur, and gradually expoſed to a gentle heat, are well known to chemiſts: they were diſcovered, and have been fully deſcribed, by Mr. Bayen[†].

M M. Brugnatelli and Van Mons have likewiſe produced fulminations by concuſſion, as well with nitrate of mercury and phoſphorus, as with phoſphorus and moſt other nitrates[‡]. Cinnabar likewiſe is amongſt the ſubſtances which, according to MM. Fourcroy and Vauquelin, detonate by concuſſion with oxymuriate of pot-aſh[§].

Mr. Ameilon had, according to Mr. Berthollet, obſerved, that the precipitate obtained from nitrate of mercury by oxalic acid, fuſes with a hiſſing noiſe[‖].

SECTION II.

But mercury, and moſt if not all its oxides, may, by treatment with nitric acid and alcohol, be converted into a whitiſh cryſtallized powder, poſſeſſing all the inflammable properties of gunpowder, as well as many peculiar to itſelf.

I was led to this diſcovery, by a late aſſertion, that hydrogen is the baſis of the muriatic acid: it induced me to attempt to combine different ſubſtances with hydrogen and oxygen. With this view, I mixed ſuch ſubſtances with alcohol and nitric acid, as I thought might

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[*] Philoſ. Tranſ. 1800, p. 204.

[†] *Opuſcules Chimiques de Bayen,* tom. i. p. 346, and note in p. 344.

[‡] *Annales de Chimie,* tom. xxvii. p. 74 and 79. (or this Journal, II. 468.)

[§] Ibid. tom. xxi. p. 238. (or this Journal, I. 168.)

[‖] This fact has been miſrepreſented, in the introduction to a work intitled The Chemical Principles of the Metallic Arts, by W. Richardſon, Surgeon, F.A.S. Sc. (page lvii.) The author, ſpeaking of the acid of ſorrel, ſays, " Klaproth of Berlin precipitated a nitrous ſolution of mercury with acid of wood-" ſorrel, neutralized with vegetable alkali. The white precipitate, well waſhed and dried, produced a ful-" minating noiſe, not inferior to that of fulminating gold. Acid of ſugar, perfectly neutralized by vege-" table alkali, produced the ſame precipitate, which, on expoſure to heat, exhibited the ſame fulminating " power." I muſt confeſs, I have not been able to produce any ſuch fulmination. Mr. Richardſon has moreover given this ſuppoſed diſcovery to Mr. Klaproth; whereas, Mr. Berthollet, when quoting the fact to which I ſuppoſe Mr. Richardſon intended to allude, obſerves, " Qu'on avoit déjà donné le nom d'argent " fulminant au précipité du nitrate d'argent par l'acide oxalique, dans lequel M. Klaproth avoit découvert " la propriété de fuſer avec vivacité lorſqu'on l'expoſe à la chaleur. M. Ameilon avoit auſſi, depuis " longtems, fait connoître que l'acide oxalique communiquoit cette propriété au mercure, quoique moins " fortement qu'à l'argent; mais cet effet (he continues) eſt fort éloigné de celui qu'on déſigne par la " fulmination." *Annales de Chimie,* tom. i. p. 57.

(by prediſpoſing affinity) favour, as well as attract, an acid combination, of the hydrogen of the one, and the oxygen of the other. The pure red oxide of mercury appeared not unfit for this purpoſe; it was therefore intermixed with alcohol, and upon both, nitric acid was affuſed. The acid did not act upon the alcohol ſo immediately as when theſe fluids are alone mixed together, but firſt gradually diſſolved the oxide: however, after ſome minutes had elapſed, a ſmell of ether was perceptible, and a white denſe ſmoke, much reſembling that from the *liquor fumans* of Libavius, was emitted with ebullition. The mixture then drew down a dark-coloured precipitate, which by degrees became nearly white. This precipitate I ſeparated by filtration: and, obſerving it to be cryſtallized in ſmall acicular cryſtals, of a ſaline taſte, and alſo finding a part of the mercury volatilized in the white fumes, I muſt acknowledge I was not altogether without hopes that muriatic acid had been formed, and united to the mercurial oxide. I therefore, for obvious reaſons, poured ſulphuric acid upon the dried cryſtalline maſs, when a violent efferveſcence enſued, and, to my great aſtoniſhment, an exploſion took place.

The ſingularity of this exploſion induced me to repeat the proceſs ſeveral times; and, finding that I always obtained the ſame kind of powder, I prepared a quantity of it, and was led to make the ſeries of experiments which I ſhall have the honour to relate in this paper.

SECTION III.

I firſt attempted to make the mercurial powder fulminate by concuſſion; and for that purpoſe laid about a grain of it upon a cold anvil, and ſtruck it with a hammer, likewiſe cold: it detonated ſlightly, not being, as I ſuppoſe, ſtruck with a flat blow; for, upon uſing 3 or 4 grains, a very ſtunning diſagreeable noiſe was produced, and the faces both of the hammer and the anvil were much indented.

Half a grain or a grain, if quite dry, is as much as ought to be uſed on ſuch an occaſion.

The ſhock of an electrical battery, ſent through 5 or 6 grains of the powder, produces a very ſimilar effect: it ſeems indeed, that a ſtrong electrical ſhock, generally acts on fulminating ſubſtances like the blow of a hammer. Meſſrs. Fourcroy and Vauquelin found this to be the caſe with all their mixtures of oxymuriate of potaſh *.

To aſcertain at what temperature the mercurial powder explodes, 2 or 3 grains of it were floated on oil, in a capſule of leaf tin; the bulb of a Fahrenheit's thermometer was made juſt to touch the ſurface of the oil, which was then gradually heated till the powder exploded, as the mercury of the thermometer reached the 368th degree.

SECTION IV.

Deſirous of comparing the ſtrength of the mercurial compound with that of gunpowder, I made the following experiment, in the preſence of my friend Mr. Abernethy.

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* *Annales de Chimie*, tom. xxi. p. 239.

Finding that the powder could be fired by flint and ſteel, without a diſagreeable noiſe, a common gunpowder proof, capable of containing eleven grains of fine gunpowder, was filled with it, and fired in the uſual way: the report was ſharp, but not loud. The perſon who held the inſtrument in his hand felt no recoil; but the exploſion laid open the upper part of the barrel, nearly from the touch-hole to the muzzle, and ſtruck off the hand of the regiſter, the ſurface of which was evenly indented, to the depth of 0,1 of an inch, as if it had received the impreſſion of a punch.

The inſtrument uſed in this experiment being familiarly known, it is therefore ſcarcely neceſſary to deſcribe it; ſuffice it to ſay, that it was of braſs, mounted with a ſpring regiſter, the moveable hand of which cloſed up the muzzle, to receive and graduate the violence of the exploſion. The barrel was half an inch in caliber, and nearly half an inch thick, except where a ſpring of the lock impaired half its thickneſs.

SECTION V.

A gun belonging to Mr. Keir, an ingenious artiſt of Camden-town, was next charged with 17 grains of the mercurial powder, and a leaden bullet. A block of wood was placed at about eight yards from the muzzle, to receive the ball, and the gun was fired by a fuſe. No recoil ſeemed to have taken place; as the barrel was not moved from its poſition, although it was in no ways confined. The report was feeble: the bullet, Mr. Keir conceived, from the impreſſion made upon the wood, had been projected with about half the force it would have been by an ordinary charge, or 68 grains, of the beſt gunpowder. We therefore recharged the gun with 34 grains of the mercurial powder: and, as the great ſtrength of the piece removed any apprehenſion of danger, Mr. Keir fired it from his ſhoulder, aiming at the ſame block of wood. The report was like the firſt in Section IV. ſharp, but not louder than might have been expected from a charge of gunpowder. Fortunately, Mr. Keir was not hurt, but the gun was burſt in an extraordinary manner. The breech was what is called a patent one, of the beſt forged iron, conſiſting of a chamber 0,4 of an inch thick all round, and 0,4 of an inch in caliber; it was torn open and flawed in many directions, and the gold touch-hole driven out. The barrel, into which the breech was ſcrewed, was 0,5 of an inch thick; it was ſplit by a ſingle crack three inches long, but this did not appear to me to be the immediate effect of the exploſion. I think the ſcrew of the breech, being ſuddenly enlarged, acted as a wedge upon the barrel. The ball miſſed the block of wood, and ſtruck againſt a wall, which had already been the receptacle of ſo many bullets, that we could not ſatisfy ourſelves about the impreſſion made by this laſt.

SECTION VI.

As it was pretty plain that no gun could confine a quantity of the mercurial powder ſufficient to project a bullet, with a greater force than an ordinary charge of gunpowder, I determined to try its comparative ſtrength in another way.

I procured two blocks of wood, very nearly of the ſame ſize and ſtrength, and bored them with the ſame inſtrument to the ſame depth. The one was charged with half an ounce of the beſt Dartford gunpowder, and the other with half an ounce of the mercurial powder; both were alike buried in ſand, and fired by a train communicating with the powders by a ſmall touch-hole. The block containing the gunpowder was ſimply ſplit into three pieces: that charged with the mercurial powder was burſt in every direction, and the parts immediately contiguous to the powder were abſolutely pounded, yet the whole hung together, whereas the block ſplit by the gunpowder had its parts fairly ſeparated. The ſand ſurrounding the gunpowder was undoubtedly the moſt diſturbed: in ſhort, the mercurial powder appeared to have acted with the greateſt energy, but only within certain limits.

SECTION VII.

The effects of the mercurial powder, in the laſt experiments, made me believe that it might be confined, during its exploſion, in the centre of a hollow glaſs globe. Having therefore provided ſuch a veſſel, 7 inches in diameter, and nearly half an inch thick, mounted with braſs caps, and a ſtop cock, (ſee Plate VII.) I placed 10 grains of the mercurial powder on very thin paper, laid an iron wire 149th of an inch thick acroſs the paper, through the midſt of the powder, and, cloſing the paper, tied it faſt at both extremities, with ſilk, to the wire. As the incloſed powder was now attached to the middle of the wire, each end of which was connected with the braſs caps, the packet of powder became, by this diſpoſition, fixed in the centre of the globe. Such a charge of an electrical battery was then ſent along the wire, as a preliminary experiment* had ſhewn me would, by making the wire red-hot, inflame the powder. The glaſs globe withſtood the exploſion, and of courſe retained whatever gaſes were generated: its interior was thinly coated with quickſilver in a very divided ſtate. A bent glaſs tube was now ſcrewed to the ſtop-cock of the braſs cap, which being introduced under a glaſs jar ſtanding in the mercurial bath, the ſtop-cock was opened. Three cubical inches of air ruſhed out, and a fourth was ſet at liberty when the apparatus was removed to the water-tub. The exploſion being repeated, and the air all received over water, the quantity did not vary. To avoid an error from change of temperature, the glaſs globe was, both before and after the exploſion, immerſed in water of the ſame temperature. It appears therefore, that the ten grains of powder, produced four cubical inches only of air.

To continue the compariſon between the mercurial powder and gunpowder, 10 grains of the beſt Dartford gunpowder were in a ſimilar manner ſet fire to in the glaſs globe: it remained entire. The whole of the powder did not explode, for ſome complete grains were to be obſerved adhering to the interior ſurface of the glaſs. Little need be ſaid of the nature of the gaſes generated during the combuſtion of gunpowder: they muſt have been, carbonic acid gas, ſulphureous acid gas, nitrogen gas, and (according to Lavoiſier *) perhaps hydrogen gas. As to the quantity of theſe gaſes, it is obvious that it could not be aſcertained; becauſe the two firſt were, at leaſt in part, ſpeedily abſorbed by the alkali of the nitre, left pure after the decompoſition of its nitric acid.

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* With Mr. Cuthbertſon's electrometer.

SECTION VIII.

From the experiments related in the 4th and 5th ſections, in which the gunpowder proof and the gun were burſt, it might be inferred, that the aſtoniſhing force of the mercurial powder is to be attributed to the rapidity of its combuſtion; and, a train of ſeveral inches in length being conſumed in a ſingle flaſh, it is evident that its combuſtion muſt be rapid. From the experiments of the 6th and 7th ſections, it is ſufficiently plain that this force is reſtrained to a narrow limit; both becauſe the block of wood charged with the mercurial powder was more ſhattered than that charged with the gunpowder, whilſt the ſand ſurrounding it was leaſt diſturbed; and likewiſe becauſe the glaſs globe withſtood the exploſion of 10 grains of the powder fixed in its centre: a charge I have twice found ſufficient to deſtroy old piſtol barrels, which were not injured by being fired when full of the beſt gunpowder. It alſo appears, from the laſt experiment, that 10 grains of the powder, produced by ignition four cubical inches only of air; and it is not to be ſuppoſed that the generation, however rapid, of four cubical inches of air, will alone account for the deſcribed force; neither can it be accounted for by the formation of a little water, which, as will hereafter be ſhewn, happens at the ſame moment: the quantity formed from 10 grains muſt be ſo trifling, that I cannot aſcribe much force to the expanſion of its vapour. The ſudden vaporization of a part of the mercury, ſeems to me a principal cauſe of this immenſe yet limited force; becauſe its limitation may then be explained, as it is well known that mercury eaſily parts with caloric, and requires a temperature of 600 degrees of Fahrenheit, to be maintained in the vaporous ſtate. That the mercury is really converted into vapour, by ignition of the powder, may be inferred from the thin coat of divided quickſilver, which, after the exploſion in the glaſs globe, covered its interior ſurface; and likewiſe from the quickſilver with which a tallow candle, or a piece of gold, may be evenly coated, by being held at a ſmall diſtance from the inflamed powder. Theſe facts certainly render it more than probable, although they do not demonſtrate, that the mercury is volatilized; becauſe it is not unlikely that many mercurial particles are mechanically impelled againſt the ſurface of the glaſs, the gold, and the tallow.

As to the force of dilated mercury, Mr. Baumé relates a remarkable inſtance of it, as follows.

“ Un alchymiſte ſe préſenta à Mr. Geoffroy, et l'aſſura qu'il avoit trouvé le moyen de “ fixer le mercure par une opération fort ſimple. Il fit conſtruire ſix boîtes rondes en fer

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* See Lavoiſier, *Traité elementaire*, p. 527.

" fort épais, qui entroient les unes dans les autres; la derniere étoit aſſujettie par deux " cercles de fer qui ſe croiſoient en angles droits. On avoit mis quelques livres de mercure dans la capacité de la première : on mit cet appareil dans un fourneau aſſez rempli " de charbon pour faire rougir à blanc les boîtes de fer; mais, lorſque la chaleur eut " pénétré ſuffiſamment le mercure, les boîtes creverent, avec une telle exploſion qu'il ſe " fit un bruit épouvantable: des morceaux de boîtes furent lancés avec tant de rapidité, " qu'il y en eut qui paſſerent au travers de deux planchers: d'autres firent ſur la muraille " des effets ſemblables à ceux des éclats de bombes *."

(*To be continued.*)

VIII.

Ancient Account of Parhelia ſeen in Cumberland. Communicated by Mr. H. SARGEANT.

To Mr. NICHOLSON.

SIR,

THE following is copied from a manuſcript written in the beginning of the 17th century, and now in the library belonging to the Grammar School of St. Bees. As it relates to a phænomenon which is extremely rare, and in this inſtance probably not elſewhere recorded, I ſend it to you for inſertion, if you think proper, in your valuable Journal, together with a copy of the figure (Pl. VIII. Fig. 4.) which accompanies it.

I am, Sir,

Your humble Servant,

Whitehaven, 1ſt of June 1800. H. SARGEANT.

Upon the 8th day of May, anno 1597, in Copland, (a diſtrict of Cumberland) was ſeene aboute the ſunne, beinge ſome two houres from ſettinge, and entringe into a thick cloude three parrhelii, or reſemblances of the ſunne, the brighteſt towards the north, the two other, one towards the ſouth, and the other towards the eaſte, and in a thinne whitiſh cloude, hardly able to be diſcerned, appeared like a rayne-bowe, a pretye diſtance from each of the ſaid parrhelii. Theſe towards the north and ſouth did ſeeme to be direct, ſtreight, without any compaſſinge or bowinge in towards the endes, but ſtoode like direct lynes from the ſouth towards the north. The one of them turned with his brightneſſe towards the other, but the rayne-bowe, which was eaſtward from the eaſte parrhelios, did bowe or compaſſe eaſtward directly from the ſunnwards, and from the parrhelios, which ſeemed contrary to the nature of all other rayne-bowes which ever I ſawe.

N. B. The writer is ſuppoſed to be the Rev. Mr. Copeland, a graduate of Oxford, about the year 1600.

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* *Chymie expérimentale et raiſonné,* tom. ii. p. 393. Paris, 8°, 1773.

Account

IX.

Account of the new Electrical or Galvanic Apparatus of Sig. ALEX. VOLTA, *and Experiments performed with the ſame.*—W. N.

FROM motives of delicacy to the inventor of the moſt curious and important combination hereafter to be deſcribed, I forbore giving an account of its conſtruction and effects in the laſt number of this Journal, though it has now been a ſubject of great attention among philoſophers for near two months. It appeared proper to avoid the publication of facts, originally flowing from the liberal communication of the worthy preſident of the Royal Society, until the paper of the inventor had been read to that learned body; and this could not be done till very lately, becauſe the latter part of his memoir did not arrive till long after the firſt four pages.

The Right Honourable Sir J. Banks, Bart. P. R. S. having favored my friend Anthony Carliſle, Eſq. with the peruſal and conſideration of theſe four pages at the latter end of laſt April, I had the pleaſure to look them over with him, immediately after which he conſtructed an inſtrument according to Sig. Volta's directions. The experiments made with this will form part of the preſent communication; but in the firſt place, I ſhall endeavour to relate the leading particulars of the communication made to the Royal Society, which no doubt will hereafter appear at large in their Tranſactions.

The portion of letter which firſt arrived from Sig. Volta, is dated from Como in the Milaneſe, March 20, 1800. This, together with the ſubſequent parts, contains a detailed account of the inſtrument, of which the following is one of the moſt convenient forms.

Take any number of plates of copper, or which is better of ſilver, and an equal number of tin, or which is much better, zinc, and a like number of diſcs, or pieces of card or leather, or cloth *, or any porous ſubſtance capable of retaining moiſture. Let theſe laſt be ſoaked in pure water, or which is better, ſalt and water, or alkaline lees. The ſilver or copper may be pieces of money †. Build up a pile of theſe pieces; namely, a piece of ſilver, a piece of zinc, and a piece of wet card: then another piece of ſilver, a piece of zinc, and a piece of wet card: and ſo forth, in the ſame order (or any other order, provided the pieces ſucceed each other in their turn) till the whole number intended to be made uſe of is builded up. The inſtrument is then completed.

In this ſtate it will afford a perpetual current of electricity, through any conductor communicating between its upper and lower plates; and if this conductor be an animal, it will receive an electrical ſhock as often as the touch is made, by which the circuit is completed.

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* Woollen or linen cloth appear to be more durable, and more ſpeedily ſoaked than card.

† Moſt of our philoſophers have uſed half crowns for the ſilver plates. The zinc may be bought at 8d. per lb. at the White Lion in Foſter Lane, and caſt in moulds of ſtone or chalk. A pound makes twenty thick pieces of the diameter of half a crown, or 1.3 inches diameter.

pleted. Thus if one hand be applied to the lower plate, and the other to the upper, the operator will receive a ſhock, and that as often as he pleaſes to lift his finger and put it down again.

This ſhock reſembles the weak charge of a battery of immenſe ſurface, and its intenſity is ſo low, that it cannot make its way through the dry ſkin. It is, therefore, neceſſary that a large ſurface of each hand ſhould be well wetted, and a piece of metal be graſped in each, in order to make the touch, or elſe that the two extremities of the pile ſhould communicate with ſeparate veſſels of water, in which the hands may be plunged.

The commotion is ſtronger the more numerous the pieces. Twenty pieces will give a ſhock in the arms, if the above precautions be attended to. One hundred pieces may be felt to the ſhoulders. The current of electricity acts on the animal ſyſtem while the circuit is complete, as well as during the inſtant of commotion, and the action is abominably painful at any place where the ſkin is broken.

That the energy of the apparatus is the effect of an electric ſtream or current, is proved by the condenſer with which Sig. V. aſcertained the kind of the electricity and obtained its ſpark. He finds the action ſtrongeſt, or moſt pungent, on wounds on the minus ſide of the apparatus, or where the wounds give out electricity, a fact alſo obſervable in the common electric ſpark.

The theory of the learned inventor, if I rightly apprehend him, is, that it is a property of ſuch bodies as differ in their power of conducting electricity, that when they are brought into contact they will occaſion a ſtream of the electric matter. So that if zinc and ſilver be made to communicate immediately by contact, there will be a place of good conducting energy; and if they be made to communicate mediately by means of water, there will be a place of inferior conducting energy: and wherever this happens there will be a ſtream or current produced in the general ſtock of electricity. This is not deduced as the conſequence of other more ſimple facts; but is laid down as a general or ſimple principle grounded on the phenomena.

As the current of electricity will be reſiſted by the different conductors, he remarks that the metals may touch in a ſingle point, or be ſoldered together; but that the humid ſurfaces muſt be more extended.

By many experiments, he finds that the conſequences are the ſame whether the zinc and ſilver touch each other, or whether the communication be made by ſeveral different metals, provided the water be in contact with the zinc and the ſilver only.

Where zinc is uſed, ſalt water is preferable to alkaline lees, but the contrary when tin is made uſe of inſtead of the zinc.

The effect is much increaſed by elevation of temperature.

He was ſurprized to find that the galvanic flaſh of light was no greater with this apparatus than with a pair of plates; but it was produced when the conductor of the circuit was applied to any part of the face, or even to the breaſt. The ſtrongeſt action was when the touching plate was held between the teeth, ſo as to lie upon the tongue. In this caſe

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the lips and tongue were convulſed, the flaſh appeared before the eyes, and the taſte was perceived in the mouth.

Two blunt probes were inſerted in the ears, and the ſhock paſſed through the head, after which the communication was kept up. A peculiar ſound, like crackling or boiling, was heard; but the author did not think it prudent to make this experiment repeatedly.

The ſenſe of ſmell could not be excited, becauſe, as Sig. V. remarks, this electricity cannot be made to diffuſe itſelf in the air.

As the diſcs become dry, and loſe their power, Sig. V. endeavoured to prevent this effect by incloſing the column in wax or pitch, and in this he has ſo far ſucceeded, that he has fitted up two columns of twenty pieces each, which have acted well for ſome weeks, and he hopes will for months.

The combination, which he thinks the moſt inſtructive, conſiſts of a row of glaſſes or cups (not of metal) containing warm water or brine. Into each of theſe is plunged a plate of zinc and another of ſilver, not touching each other. From theſe plates reſpectively proceed tails or prolongations, which communicate with or touch the plates of the outer glaſſes in ſuch a manner, that the zinc of the firſt cup communicates with the ſilver of the ſecond; the zinc of the ſecond with the ſilver of the third; the zinc of the third, &c. progreſſively and regularly through the whole row. The communication between the firſt and laſt glaſſes gives the ſhock, &c. The plates in the fluid are directed to be about an inch ſquare; but the contacts above the water may be as ſmall as the operator pleaſes.

Sig. Volta makes honourable mention of my conjectural theory of the torpedo *. After remarking that my inductions were the moſt probable that the exiſting theory of electricity could at that time afford, he proceeds to make various objections needleſs to be here detailed, and then offers his own new and ſtriking apparatus as more nearly reſembling the torpedinal organ. I need not anticipate the reader in the happy points of reſemblance between their ſtructure and effects.

Thus far I have followed this able philoſopher; who, to his former reſearches into the nature and laws of electricity, has now added a diſcovery which muſt for ever remove the doubt whether galvaniſm be an electrical phenomenon. But I cannot here look back without ſome ſurprize, and obſerve that the chemical phenomena of galvaniſm, which had been much ſo inſiſted on by Fabbroni †, more eſpecially the rapid oxidation of the zinc, ſhould conſtitute no part of his numerous obſervations.

On the 30th of April, Mr. Carliſle had provided a pile conſiſting of 17 half crowns, with a like number of pieces of zinc, and of paſteboard, ſoaked in ſalt water. Theſe were arranged in the order of ſilver, zinc card, &c. which order I ſhall denote by ſaying, that the ſilver was undermoſt, that is to ſay, under the zinc; and I make this remark becauſe ſome philoſophers have uſed the expreſſion that the ſilver was undermoſt when they uſed the order of ſilver, card zinc, &c. which, as the reader will eaſily perceive, is contrary to the order here ſpoken of. This is of no conſequence to the effect, though it is material to a clear

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* Philoſophical Journal, I. 358.

† Philoſophical Journal, IV. 120.

clear underſtanding of the terms we uſe. This pile gave us the ſhock as before deſcribed, and a very acute ſenſation wherever the ſkin was broken. Our firſt reſearch was directed to aſcertain that the ſhock we felt was really an electrical phenomenon. For this purpoſe the pile was placed upon Bennett's gold leaf electrometer, and a wire was then made to communicate from the top of the pile to the metallic ſtand or foot of the inſtrument. So that the circuit of the ſhock would have been through the leaves, if they had diverged. But no ſigns of electricity appeared. Recourſe was then had to the revolving doubler, deſcribed at page 95 of our preſent volume. The plate A was connected with the top of the electrometer and the ſilver end of the pile; and the plate B and ball were made to touch the top of the ſyſtem by an uninſulated braſs wire. The doubler had been previouſly cleared of electricity by twenty turns in connection with the earth. The negative divergence was produced in the electrometer. Repeated experiments of this kind ſhewed that the ſilver end was in the minus, and the zinc end in the plus ſtate.

In all theſe experiments it was obſerved, that the action of the inſtrument was freely tranſmitted through the uſual conductors of electricity, but ſtopped by glaſs and other non-conductors. Very early in this courſe, the contacts being made ſure by placing a drop of water upon the upper plate, Mr. Carliſle obſerved a diſengagement of gas round the touching wire. This gas, though very minute in quantity, evidently ſeemed to me to have the ſmell afforded by hydrogen when the wire of communication was ſteel. This, with ſome other facts, led me to propoſe to break the circuit by the ſubſtitution of a tube of water between two wires. On the 2d of May we, therefore, inſerted a braſs wire through each of two corks inſerted in a glaſs tube of half an inch internal diameter. The tube was filled with New river water, and the diſtance between the points of the wires in the water was one inch and three quarters. This compound diſcharger was applied ſo that the external ends of its wire were in contact with the two extreme plates of a pile of thirty-ſix half crowns with the correſpondent pieces of zinc and paſteboard. A fine ſtream of minute bubbles immediately began to flow from the point of the lower wire in the tube, which communicated with the ſilver, and the oppoſite point of the upper wire became tarniſhed, firſt deep orange, and then black. On reverſing the tube, the gas came from the other point, which was now loweſt, while the upper in its turn became tarniſhed and black. Reverſing the tube again, the phenomena again changed their order. In this ſtate the whole was left for two hours and a half. The upper wire gradually emitted whitiſh filmy clouds, which, towards the end of the proceſs, became of a pea green colour, and hung in perpendicular threads from the extreme half inch of the wire, the water being rendered ſemiopaque by what fell off, and in a great part lay, of a pale green, on the lower ſurface of the tube, which, in this diſpoſition of the apparatus, was inclined about forty degrees to the horizon. The lower wire of three quarters of an inch long, conſtantly emitted gas, except when another circuit, or complete wire, was applied to the apparatus; during which time the emiſſion of gas was ſuſpended. When this laſt mentioned wire was removed, the gas re-appeared as before, not inſtantly, but after the lapſe of four beats of a half ſecond clock ſtanding in the room. The product of gas, during the whole two hours and a half, was two-thirtieths of a cubic inch. It was then mixed with an equal quantity of common air, and exploded by the application of a lighted waxed thread.

It might ſeem almoſt unneceſſary to have reverſed the order of the pile in building up, as reverſing the tube muſt have anſwered exactly the ſame purpoſe. We choſe, however, to do this, and found that when the zinc was at the bottom, its effects were reverſed, that is to ſay, the gas ſtill came from the wire communicating with the ſilver, &c.

We had been led by our reaſoning on the firſt appearance of hydrogen to expect a decompoſition of the water; but it was with no little ſurprize that we found the hydrogen extricated at the contact with one wire, while the oxigen fixed itſelf in combination with the other wire at the diſtance of almoſt two inches. This new fact ſtill remains to be explained, and ſeems to point at ſome general law of the agency of electricity in chemical operations. As the diſtance between the wires formed a ſtriking feature in this reſult, it became deſirable to aſcertain whether it would take place to greater diſtances. When a tube three quarters of an inch in diameter, and thirty-ſix inches long, was made uſe of, the effect failed, though the very ſame wires, inſerted into a ſhorter tube, operated very briſkly. The ſolicitation of other objects of enquiry prevented trial being made of all the various intermediate diſtances; but from the general tenor of experiments, it appears to be eſtabliſhed, that this decompoſition is more effectual the leſs the diſtance between the wires, but that it ceaſes altogether when the wires come into contact.

May 6.—Mr. Carliſle repeated the experiment with copper wires and tincture of litmus. The oxidating wire, namely, from the zinc ſide, was the loweſt in the tube; it changed the tincture red in about ten minutes as high as the upper extremity of the wire. The other portion remained blue. Hence it ſeems either an acid was formed, or that a portion of the oxigen combined with the litmus, ſo as produce the effect of an acid.

It may be here offered as a general remark, that the electric pile with card, or with woollen cloth, continues in order for about two days, or ſcarcely three; that from a ſeries of glaſſes ſet up by Mr. Carliſle, as well as from the pile itſelf, it appears that the ſame proceſs of decompoſition of water is carried on between each pair of plates, the zinc being oxided on the wet face, and hydrogen given out; that the common ſalt is decompoſed, and exhibits an efflореſcence of ſoda round the edges of the pile, extruded, moſt probably, by the hydrogen: and that on account of the corroſion of the faces of the zinc, it is neceſſary to renew them previous to each conſtruction of the pile. This may be done by ſcraping or grinding. I found it moſt convenient to lay the piece in a hole in a board, and give it a ſtroke with a float file, or file of which the teeth are not croſſed. It might, perhaps, be leſs troubleſome to clean them with diluted muriatic acid; but this I have not tried.

As the ample field of phyſiological reſearch to which Mr. Carliſle's attention is directed, and the multiplicity of my own avocations, rendered it leſs convenient for us to purſue our enquiries together, I conſtructed an apparatus for my own uſe. Zinc was laminated to the twenty-fourth part of an inch in thickneſs, and pure ſilver to the one-thouſandth part of an inch, that is to ſay, as thin as our flatting mills can bring it.

Of theſe metals I made two ſets, namely, ſixteen pieces of ſilver of two inches in diameter, and ſixteen pieces of 1.8 inch diameter, with their correſpondent plates of zinc and wetted card. The ſmall pile was firſt prepared, and whether it were that theſe thin pieces were more diſpoſed to admit the water between the metallic faces of contact, or from whatever other cauſe it may have ariſen, it did not appear by any experiment, that the whole ſet, though ſo greatly exceeding the pile of half-crowns in ſurface, was capable of doing more in the decompoſition of water, or in communicating the ſhock. But this, with other facts, ſeems to ſhew, that the repetition of the ſeries is of more conſequence to this action, than the enlargement of ſurface; and alſo that the thickneſs of the plates, though it may be attended with convenience, moſt probably affords no addition to the force. I muſt alſo add, that I have no reaſon to recommend my pile, though at firſt ſight it ſeemed to poſſeſs cheapneſs and convenience. The plates of zinc are too thin to bear frequent cleaning or renewing after corroſion of the ſurface, and the ſilver, though it is ſcarcely acted on in this ſituation, is too thin to be conveniently wiped or handled.

The ſpontaneous electricity of the doubler preſented an objection to the ſtrict fidelity of its reſults; whence I thought it deſirable to give my pile a trial with the condenſer. The foot or ſtand of my electrometer is a braſs plate truly flat, and 3.8 inches in diameter. A piece of thin Perſian ſilk was tied ſmoothly upon the face of this plate, and it was then placed upon another braſs plate, upon which it was moved about horizontally, in order to accumulate electricity by friction; the electrometer itſelf being uſed as the handle by graſping the top. It was found that this treatment produced very weak ſigns of electricity when the electrometer was lifted up. The lower braſs plate was then placed on the top of the ſmall pile, and the condenſing electrometer placed upon it. A communication was then made, by means of a wire from the lower or ſilver end of the pile to the upper plate of the condenſer, or foot of the electrometer. In this ſituation it is evident, that the charge of the pile was employed in producing oppoſite ſtates of electricity in the condenſer, which would be ſhewn when the plates came to be ſeparated. The wire of communication being taken away, the electrometer was lifted, and the leaves diverged and ſtruck. It became neceſſary, therefore, to repeat the experiment, taking care to lift the electrometer more gradually. The divergence took place as before, and it was increaſed by preſenting excited ſealing wax towards the bottom of the electrometer. And as the top of the pile had by compenſation diminiſhed the ſame divergence, it is clear that the electricity of the top of the pile, *viz.* of the zinc, was contrary to that of ſealing wax; that is to ſay, the zinc was in the plus ſtate. After a number of repetitions of this experiment with the ſame invariable reſult, the pile was then carefully overſet, without diſturbing the relative arrangement of its parts; ſo that the zinc was now at the bottom, and the ſilver at the top. The electricity of the ſilver was then tried a number of times, by preciſely the ſame proceſs as before, and it exhibited an equal degree of intenſity, but it was minus or negative. In one of theſe experiments, I certainly ſaw the ſpark at the time of completing the circuit, and afterwards with the ſame pile, when I was expreſsly looking for it. But it is leſs neceſſary to dwell on theſe facts, as the ſtronger combinations have exhibited this effect with much greater perſpicuity.

The decompoſition of water, and oxidation of metallic wire, gave birth to a variety of ſpeculations and projects of experiments. Among others it became a queſtion, what would be the habitude of metals of difficult oxidation. Two wires of platina, one of which was round, and one fortieth of an inch in diameter, and the other nearly of the ſame maſs, but flatted to the breadth of one twenty-fifth of an inch, were inſerted into a ſhort tube of $\frac{1}{4}$ of an inch inſide diameter. When placed in the circuit, the ſilver ſide gave a plentiful ſtream of fine bubbles, and the zinc ſide alſo a ſtream leſs plentiful. No turbidneſs nor oxidation, nor tarniſh appeared, during the courſe of four hours continuance of this operation. It was natural to conjecture, that the larger ſtream from the ſilver ſide was hydrogen, and the ſmaller oxigen. Thick gold leaf was tried, with the ſame effects. A wire of braſs was then ſubſtituted inſtead of one of the ſlips of gold. When the braſs was on the minus, or ſilver ſide, the two gaſes were extricated for two hours, without oxidation as before; but when the braſs was, by reverſing the tube brought to the plus ſide, it became oxided in the ſame manner as if both the wires had been braſs. When the ſlips of gold were long ſubjected to this action, the extremity of the ſlip communicating with the zinc, acquired a coppery or purpleiſh tinge, which was deepeſt near the end. Whether this aroſe from oxidation of the gold, or of the copper, of which gold leaf contains about a ſeventieth part, cannot from this experiment be decided.

The ſimple decompoſition of water by platina wires without oxidation, offered a means of obtaining the gaſes ſeparate from each other. With this intention, Mr. Carliſle's pile of thirty-ſix was combined with my two ſets of ſixteen repetitions. His pile was built with the zinc uppermoſt, and mine in the reverſe order; ſo that by connecting the upper plates the whole conſtituted one range, and the communications could be made from the bottom of the one to the bottom of the other. The two platina wires were made to protrude out of two ſeparate tubes, each containing a little water, and through the oppoſite corks of each were paſſed copper wires of communication. Theſe tubes were ſlightly greaſed on the outſide to prevent their becoming damp; and in this ſtate the extremities, armed with the platina, were plunged in a ſhallow glaſs veſſel of water, in which two ſmall inverted veſſels, quite full of water, were ſo diſpoſed, that the platina of one tube was beneath one veſſel, and the platina of the other tube was beneath the other, the diſtance between their extremities being about two inches. The copper wires of theſe tubes reſpectively were made to communicate with the extremities of the intire pile of ſixty-eight ſets. A cloud of gas aroſe from each wire, but moſt from the ſilver, or minus ſide. Bubbles were extricated from all parts of the water, and adhered to the whole internal ſurface of the veſſels. The proceſs was continued for thirteen hours, after which the wires were diſengaged, and the gaſes decanted into ſeparate bottles. On meaſuring the quantities, which was done by weighing the bottles, it was found, that the quantities of water diſplaced by the gaſes, were reſpectively, 72 grains by the gas from the zinc ſide, and 142 grains by the gas from the ſilver ſide; ſo that the whole volume of gas was 1.17 cubic inches, or near an inch and a quarter. Theſe are nearly the proportions in bulk, of what are ſtated to be the component parts of water. The gas from the zinc ſide, being tried with one meaſure of nitrous gas, contracted to 1.25, and did not contract more by the addition of another meaſure; the gas from the ſilver ſide by the ſame treatment contracted to 1.6. The air of the room, on trial, contracted to 1.28. From the ſmallneſs of the quantity no attempt was made to detonate the air from the zinc ſide, but a portion of that from the ſilver ſide, being mixed with one third of atmoſpheric air, gave a loud detonation.

Upon the above it may be remarked, that it it does not ſeem probable that oxigen was afforded by both wires, but that they were mixed by the circumſtances of the experiment. For the gaſes being extricated in extremely minute bubbles beneath the inverted veſſels, cauſed a ſlow aſcending current conſiſting of water mixed with thoſe bubbles, many of which were undoubtedly too ſmall to be diſcerned. This aſcending current gave out as much of its gas at the top of the veſſel, as had time to conglomerate; but the extremely minute bubbles would return in the deſcending current, and be repeatedly carried up before this effect could take place. Such a continual circulation, or ſtream, the lower part of which paſſed down into the ſaucer, muſt at length have occaſioned the whole maſs of fluid to become replete with theſe minute bubbles, which would break at the open ſurface, and be loſt, or attach themſelves to the ſides of the veſſel, as was ſeen to be the caſe. What proportion may have thus diſappeared is uncertain; but it is highly probable, that one conſequence of the imperfections of our apparatus, was to occaſion both the inverted veſſels to become receptacles for the gaſes from both wires indiſcriminately; though moſt plentifully in each, from the wire immediately beneath its mouth. If this reaſoning may be admitted for the preſent, till the experiment is repeated in cloſed veſſels, it will be fair to reckon the whole diminution on both the quantities. The whole diminution was 1.15, whence it would follow, that the purity of the oxigen eſtimated in Prieſtley's manner, would be expreſſed by the number 0.85.

On account of the length of this communication, I ſhall at preſent forbear to enter into any conſiderations of theory, but ſhall conclude with a conciſe mention of the effects of a pile of one hundred half crowns, and a chemical incident, which appears to be the moſt remarkable of thoſe which I have yet obſerved.

The pile was ſet up with pieces of green woollen cloth ſoaked in ſalt water. It gave ſevere ſhocks, which were felt as high as the ſhoulders. The tranſition was much leſs ſenſible through a number of perſons, but it was very perceptible through nine. The ſpark was frequently viſible when the diſcharge was made in the dark, and a gleam of light was alſo, in ſome inſtances, ſeen about the middle of the column at the inſtant of the exploſion. The aſſiſtants were of opinion that they heard the ſpark.

The extrication of the gaſes was rapid and plentiful by means of this apparatus. When copper wires were uſed for the broken circuit, with muriatic acid diluted with 100 parts of water in the tube, no gas, nor the leaſt circulation of the fluid was perceived, when the diſtance of the wires was two inches. A ſhort tube, with two copper wires very near each other in common water, was made part of the circuit, and ſhewed by the uſual phænomena, that the ſtream of electricity was rapidly paſſing. The wires in the muriatic acid were then ſlided within a third of an inch of each other. For the ſake of brevity, I avoid enumerating the effects which took place during ſeveral hours, and ſimply ſtate; that the minus wire gave out ſome hydrogen during an hour, while the plus wire was corroded, and exhibited no oxide; but a depoſition of copper was formed round the minus, or lower wire, which began at its lower end: that no gas whatever appeared in this tube during two hours, though the depoſition was going on, and the ſmall tube ſhewed the continuance of the electric ſtream; and that the depoſition at the end of four hours formed a ramified metallic vegetation, nine or ten times the bulk of the wire it ſurrounded.

In this experiment it appeared, that the influence of electricity increaſing the oxidability of the upper wire, and affording naſcent hydrogen from the lower, cauſed the latter to act as the precipitant of a ſolution of one and the ſame metal.

We are in want of a meaſure of the intenſity of the action of theſe machines. Will this be derived from the quantities of water decompoſed, or of gas extricated under like circumſtances in given times? Or from any change of temperature? Or what other commenſurate incident?—Mr. Carliſle has not found that the water in the tube, while under this agency, did produce the ſlighteſt effect on a very ſmall and delicate thermometer.

X.

Some Experiments and Obſervations on Galvanic Electricity. By Mr. W. CRUICKSHANK, *Woolwich. Communicated by the Author.*

IN ſubjecting a number of fluids to the action of galvaniſm, ſeveral facts have been diſcovered, which to me, at leaſt, are perfectly new, and which appear to throw ſome light on the nature and powers of this new influence.

I ſhall, therefore, without any further apology, give a brief detail of ſome of the moſt important, hoping that they may prove acceptable to thoſe who are employed in the ſame purſuits.

I ſhall not give any particular account of the apparatus employed, being a pile, and not differing materially from that in uſe. I ſhall only juſt obſerve, that it conſiſted of plates of zinc and ſilver, of about 1.6 inches ſquare, and that the number of each employed in the following experiments varied from 40 to 100, according to the power required.

I found that a ſolution of the muriate of ammonia anſwered better for moiſtening the interpoſed papers than common water.

When the machine was in full action, ſparks, which were perfectly viſible in the day time, could be taken at pleaſure, by making a communication in the uſual way between the extremities of the pile, and a ſmall report or ſnap could be heard; the ſhock given at that time was very ſtrong, and a gold leaf electrometer, placed in the circle of communication, was very ſenſibly affected: theſe circumſtances, ſome of which I believe have been already aſcertained by Meſſrs. Nicholſon and Carliſle, ſhew the ſtrong reſemblance of this influence to electricity. Theſe gentlemen have likewiſe diſcovered that galvaniſm decompoſes water with much greater facility than electricity, but with phenomena ſomewhat different.

Experiment 1.—A quantity of common water was introduced into a glaſs tube, being confined at each end by corks, but perfectly at one by a cement of roſin and bees'-wax: pieces of ſilver wire were paſſed through the corks, and brought within an inch of each other in the fluid, their other extremities being at the ſame time connected with theſe of the machine or pile, one with the lower zinc plate, and the other with the upper ſilver plate. In future, to avoid circumlocution, I ſhall call the wire attached to the ſilver plate, the ſilver wire, and the other the zinc wire. The tube was then placed upright in a cup containing water, with the uncemented end downwards. As ſoon as the communication was made between the extremities of the pile by the wires, a quantity of ſmall air bubbles began to aſcend from the end of the wire connected with the ſilver, as obſerved by Meſſrs. Nicholſon and Carliſle; but a white cloud at the ſame time made its appearance at the one proceeding from the zinc, or the zinc wire. This cloud gradually increaſed, and aſſumed a darker colour, and at laſt it became purple, or even black. A very few air bubbles were likewiſe collected upon and aſcended from this wire, but when the machine was in full force, a conſiderable ſtream could be obſerved.

The gas was collected, and found to be a mixture of hydrogene and oxygene, in the proportion of three parts of the former to one of the latter. No great dependence, however, was placed upon this in point of accuracy. The zinc wire was found to be much corroded, and looked as if a conſiderable portion of it had been diſſolved. As the cloud which was formed around this wire became purple on expoſure to the light, I ſuſpected it might be luna cornea, or muriate of ſilver proceeding from the ſilver, which had been ſomehow diſſolved, and afterwards precipitated in this ſtate, by the muriatic ſalts in the common water. This led to the following experiments:

Experiment 2. The glaſs tube was now filled with diſtilled water, to which a little tincture of litmus was added, when the communication was made by the wires as in the former experiment, a quantity of gas aroſe from both wires, but in the greateſt quantity from that connected with the ſilver. In a few minutes a fine red line, extending ſome way upwards, was perceived at the extremity of the zinc wire; this increaſed, and in a ſhort time the whole fluid below the point of this wire became red; the fluid, however, above the ſilver wire, looked of a deeper blue than before, the ſlight tinge of purple being deſtroyed.

Experiment 3. I next filled the tube with diſtilled water, tinged with the tincture of Brazil wood; it was no ſooner placed in the circle of communication, than the fluid ſurrounding the ſilver wire, particularly towards its extremity, became purple, and this tinge increaſed ſo faſt, that the whole fluid ſurrounding this wire, and occupying the upper part of the tube, ſoon aſſumed as deep a colour, as could be produced by ammonia.

The portion of the fluid in contact with the zinc wire became very pale, and almoſt colourleſs, nor could the purple tinge extend below its upper extremity. From theſe experiments it would appear, that an acid, probably the nitrous, is produced at the wire proceeding from the zinc, and an alkali, probably ammonia, at that in contact with the ſilver. Theſe facts ſufficiently explain the action upon the ſilver wire, and the nature of the whitiſh cloud proceeding from it, and afterwards becoming purple. When lime-water was employed inſtead of common, or diſtilled water, the wire was likewiſe acted upon, but in a leſs degree, and the cloud had at firſt an olive colour, exactly reſembling the precipitate of ſilver by lime-water.

The quantity of ſilver diſſolved or corroded, if I may uſe the expreſſion, in theſe experiments, was very conſiderable, and where common, or diſtilled water had been employed, a ſmall portion of it remained in ſolution, which was diſcovered by the addition of the muriatic acid. Indeed a much larger quantity would probably have been ſuſpended, had it not been for the alkali generated at the ſame time, and which manifeſtly produced a precipitate at, or near, the upper extremity of the zinc wire, where after a certain time, a dark zone or ſtratum was always formed.

Experiment 4. It is a well known fact, that hydrogene gas when heated, or in its naſcent ſtate, reduces the calces of the metals; I expected, therefore, that by filling the glaſs tube with a metallic ſolution, I might be enabled to ſeparate the hydrogene from the oxigen gas, and thus procure the latter in its ſimple or pure ſtate. With this view the tube was filled with a ſolution of the acetite of lead, to which an exceſs of acid was added, to counteract the effects of the alkali. When the communication was made in the uſual way, no gas could be perceived, but after a minute or two, ſome fine metallic needles were perceived at the extremity of the wire connected with the ſilver. Theſe ſoon increaſed, and aſſumed the form of a feather, or rather, that of the cryſtals of the muriate of ammonia. The lead thus precipitated was perfectly in its metallic ſtate, and very brilliant; a little gas eſcaped from the wire connected with the zinc, and it was conſiderably corroded as uſual.

A ſolution of the ſulphate of copper was next employed, and with the ſame reſult, the copper being precipitated in its metallic form by the wire connected with the ſilver. In this inſtance the metal did not cryſtallize, but formed a kind of button at the end of the wire; which adhered ſo completely to the ſilver, that it was found impoſſible to ſeparate it.

The moſt beautiful precipitate, however, was that of ſilver from its ſolution in the nitrous acid. In this caſe, the metal ſhot into fine needle-like cryſtals articulated, or joined to each other, as in the Arbor Dianæ.

What became of the oxygene gas uſually produced in theſe experiments?

Experiment 5. A quantity of pure water mixed with diſtilled vinegar was introduced into the tube, and placed in the circle of communication; ſome gas was diſengaged from the ſilver wire, but no cloud appeared at the extremity of the zinc. After ſome time, however, a quantity of metallic ſilver was precipitated by the ſilver wire, and this precipitate at laſt became very copious; a perfectly ſimilar effect was produced, when the tube was filled with very dilute ſulphuric acid; in theſe caſes the precipitated ſilver had the appearance of ſhining ſcales, like that thrown down by copper in the uſual way. It may be proper to obſerve, that in all theſe precipitations and reductions, nothing but wires of pure ſilver were employed. The reſults in this laſt experiment were exactly what was expected; the vinegar prevented the alkali from precipitating the ſilver, diſſolved by the generated acid; in conſequence of which, when a ſufficient quantity of the metal was taken up, it was again thrown down by the ſilver wire in its metallic form.

Experiment 6. A ſolution of the muriate of ammonia being introduced into the tube, and expoſed to this influence, a little gas was diſengaged from the ſilver wire, while the zinc one was incruſted with a ſubſtance which ſoon became black, and was found to be luna cornea. The liquor which remained in the tube after the operation had been finiſhed, was highly alkaline, and ſmelled ſtrongly of ammonia; common ſalt was decompoſed in a ſimilar manner. This experiment accounts for the decompoſition of the muriate of ſoda and ammonia, which always takes place when the papers in the pile are moiſtened with a ſolution of theſe ſalts.

A ſolution of the nitrate of magneſia appeared to be likewiſe decompoſed by this proceſs, for after ſome time, a white powder reſembling magneſia, was precipitated on the ſurface of the ſilver wire; very little gas was diſengaged.

Experiment 7. In order to aſcertain how far this influence might be carried, provided the circle of communication was complete, two tubes were employed, and connected by a ſilver wire paſſing through corks as in the figure, where A, B, repreſent the two tubes, and C the connection wire; the tubes were filled with water and ſecured by corks; two other wires D and E, being then paſſed through theſe corks, the arc D was connected with the ſilver, and the other with the zinc, at the extremity of the pile. A quantity of gas as uſual was diſengaged at the extremity of the ſilver wire D in A, and the portion of the connecting wire C in the ſame tube was partly diſſolved, and as mentioned in experiment 1; but the other portion of the ſame wire in the tube B gave out gas, while the communicating zinc wire E was corroded. And I make no doubt that a ſimilar effect would be produced, if any number of tubes were connected in a ſimilar manner, by which means a large quantity of gas might be procured in a ſhort time.

Beſides ſilver wires, I likewiſe employed thoſe of copper or iron, and it did not appear that theſe were more corroded or acted upon than the ſilver; indeed, in ſome of the above experiments, not leſs than half, or three quarters of an inch of the wire was intirely conſumed. The copper wire connected with the zinc gives out a greeniſh blue ſubſtance reſembling the nitrate of copper with exceſs of the metal, or when part of the acid has been expelled by heat, &c. In examining the gas which was procured at different times, I always found it mixed with a little oxygen gas, but ſometimes this did not exceed ¼ of the whole in bulk; however, I paid but little attention to this part of the proceſs, for as my wires were always corroded, no concluſion with regard to the compoſition of water could be drawn from it.

At preſent I ſhall only further obſerve, that by making the galvanic influence paſs through a quantity of diſtilled water confined in a tube over mercury, for about 48 hours, a manifeſt diminution of the fluid was perceived.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Magnetic Dip at Prince of Wales's Iſland.

MR. George Thomas Staunton, in a letter to his father, Sir George Staunton, Bart. dated Prince of Wales's Iſland, Nov. 4, 1799, ſtates, that "our landing at this place has "enabled me to make uſe of the dipping needle, which I brought on ſhore, and found "that by the means of ſeveral obſervations, the ſouth pole of the needle dipped five de-"grees ten minutes."

◆

Infuſibility of Tungſten.

The ſpecific gravity of Tungſten has not been aſcertained with preciſion, on account of the extreme difficulty of fuſing this metallic ſubſtance. Guyton, in a fire urged by the blaſt of three pipes to 185 degrees of the pyrometer, obtained a well rounded piece of 35 grammes. But it broke in the vice, and exhibited a central portion, which was only agglutinated, and ſoon acquired a purple colour by expoſure to the air. He found that the well fuſed portion had no higher ſpecific gravity than 8.3406, and he concludes from the infuſibility and brittleneſs of this metal, that it affords little promiſe of utility in the arts, except in metallic alloys, or by virtue of the property which its oxide poſſeſſes, of affording fixed colours, or giving fixity to the colours of vegetables.

The *Geometrie deſcriptive* of Gaſpard Monge is publiſhed at Paris in quarto, with 25 plates, by Bernard, and likewiſe a new edition of his Statique.

The reference to a drawing in Mr. Cruickſhank's paper, in our preſent Number, relates to a ſketch which, on account of the late reception of the copy, could not be inſerted in the plate. It conſiſts ſimply of two tubes, forming what I have called the broken circuit in the manner mentioned in my experiment, where a tube with water was connected with another containing marine acid, and both ſet to work at the ſame time. It ſeems very probable, that a large quantity of gas might be thus extricated; but from the circumſtances of the laſt mentioned experiment, it ſeems apparent that the power of the apparatus in each individual tube will be leſs the greater the number.

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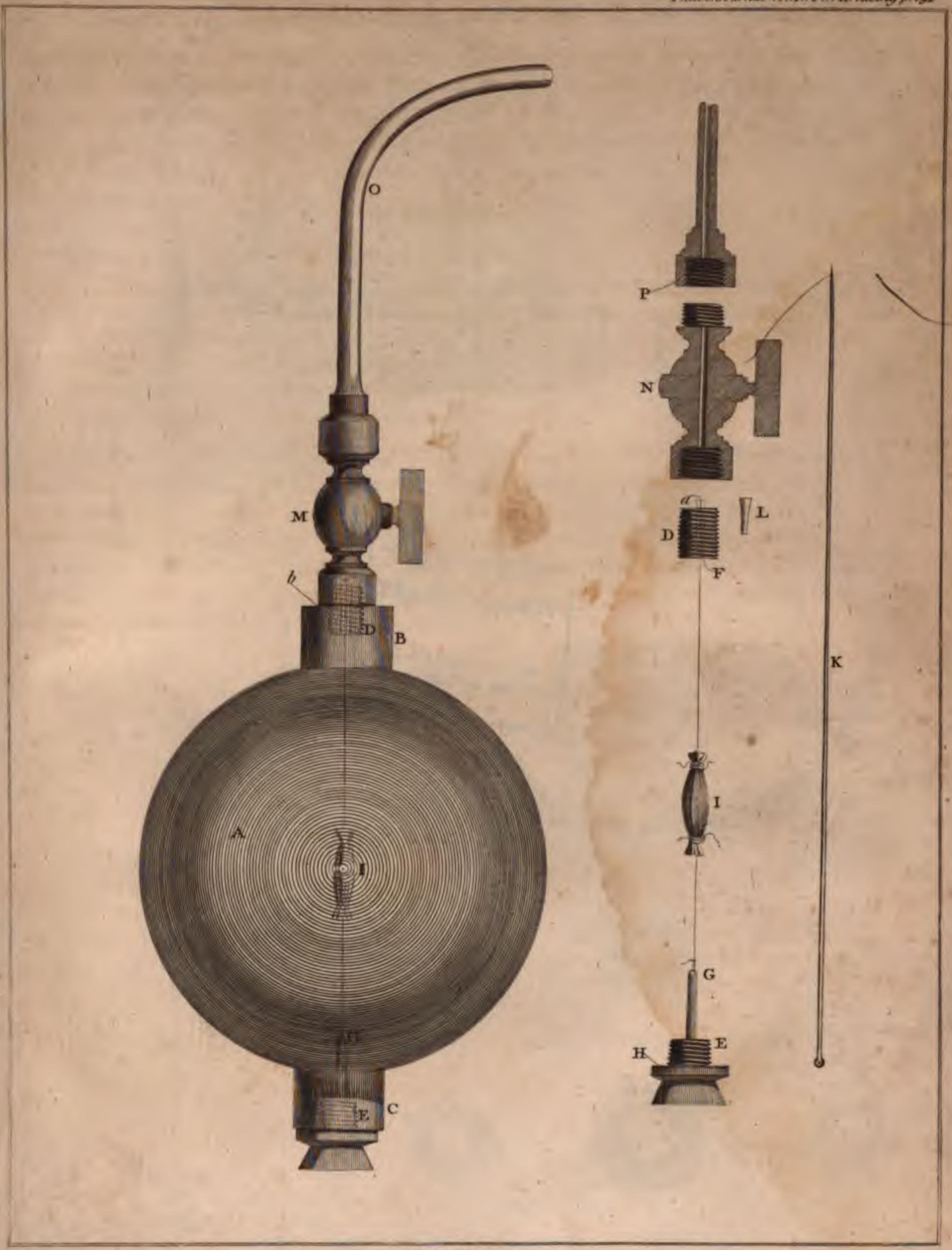

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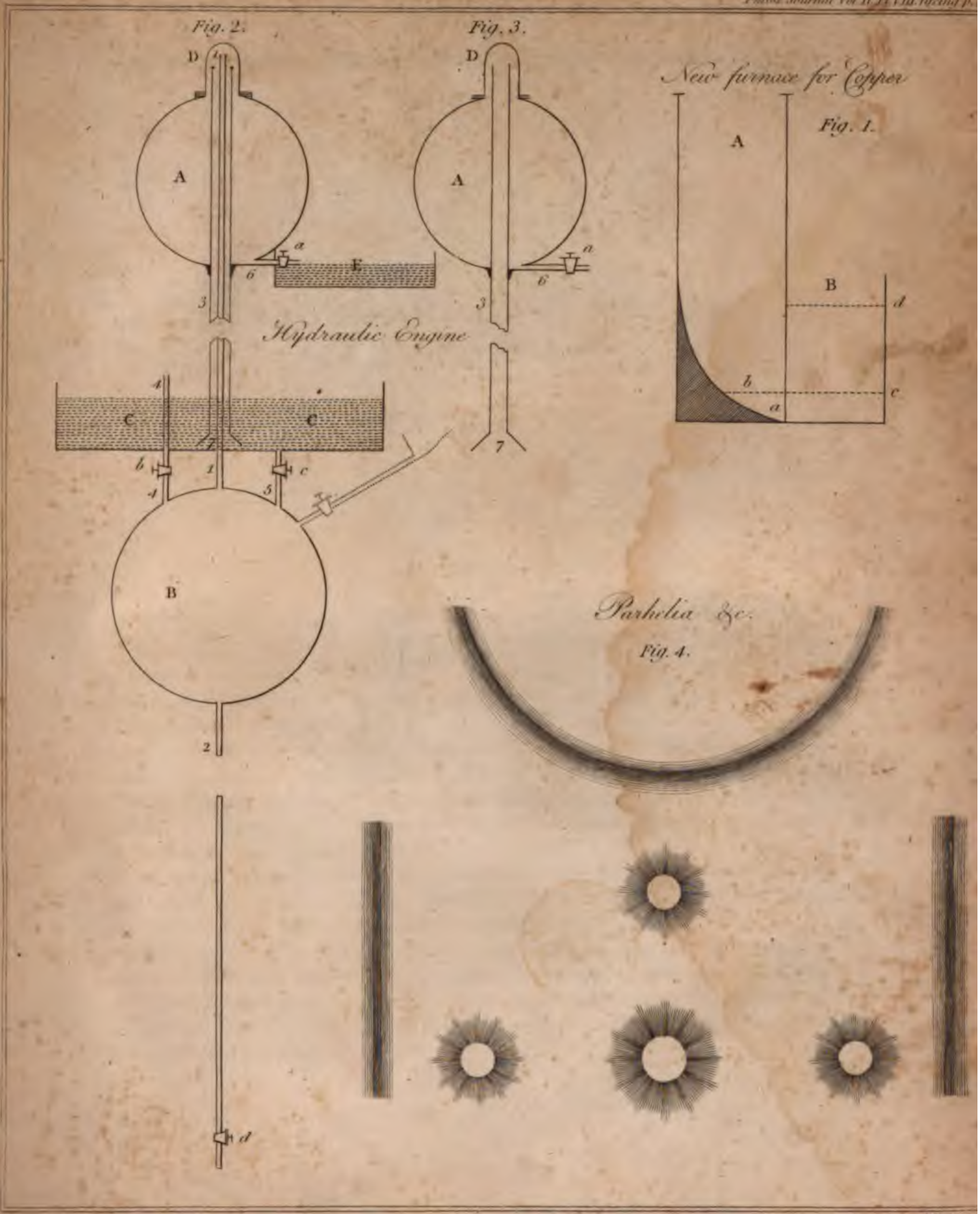

Hydraulic Engine

New furnace for Copper

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST 1800.

ARTICLE I.

Experiments on the Production of Air by the Freezing of Water. By JOSEPH PRIESTLEY, *LL.D. F.R.S. &c. &c. Communicated by the Author.*

IN 1793, when I was in England, I publiſhed an account of a courſe of experiments on *the generation of air from water*, and, after my arrival in this country, I publiſhed a ſequel to them. The reſult of the whole was that, after all air had, in every known method, been extracted from any quantity of water, whenever any portion of it was converted into vapour, a bubble of permanent air was formed, and this was always phlogiſticated. The proceſs with the torricellian vacuum I continued ſome years, and found the production of air equable to the laſt.

The neceſſary inference from theſe experiments is either that water is convertible into phlogiſticated air, or that it contains more of this air intimately combined with it than can, by this proceſs, be extracted in any reaſonable time.

Finding that no air is contained in ice that is free from viſible bubbles, I thought to aſcertain the truth of one of thoſe hypotheſes by expoſing to cold a quantity of water from which I had, by repeated proceſſes with the torricellian vacuum, expelled all the air that I poſſibly could; thinking that if it really contained no air, it would appear by the ice being perfectly ſolid, ſo that when it was melted, no air could be got from it. This experiment I repeated ſeveral times, but always found that, though the outſide of this ice was perfectly tranſparent, and free from air bubbles, the central parts were opaque; and though there were no diſtinct air bubbles there, yet when it was melted, a great number iſſued from it. The whole quantity of air, however, was not more than might have been produced from the ſame water in the other proceſſes in a reaſonable time; and in them the production of air had no limit.

Diſappointed in my expectation of getting by this means ice perfectly free from air (which, when a large quantity of water freezes very ſlowly it is eaſy to do, the air contained in it retiring from that which is frozen to that which remains fluid) I diſſolved ice that was perfectly tranſparent, and therefore free from air, in veſſels containing mercury, and expoſed it to froſt a ſecond time. But I always found that when the whole of it was frozen, the extreme parts were tranſparent, the central parts were opaque, and when diſſolved, yielded air. Though I repeated this proceſs ten or a dozen times with the ſame water, always letting out the air that was procured by freezing, preſently after it was extricated, and before it could have been re-abſorbed; yet on expoſing it to another freezing, I never failed to get more air, and the harder the froſt was, the more air I procured.

As there is an evaporation from ice no leſs than from water, the interſtices made by the cryſtalization of the water, when it is converted into ice, will ſoon be filled with vapour; and this vapour, like that which is formed by heat, becomes the baſis, I ſuppoſe, of a quantity of air. Since, however, the moſt tranſparent ice ſwims in water, this alſo muſt have interſtices, but they contain no air; being ſuch as exiſt in the moſt ſolid bodies in which (gold itſelf not excepted) the component particles are not in perfect contact, ſince they are reduced into leſs dimenſions by cold.

As the veſſels I made uſe of in theſe experiments were either cylindrical jars, or conical wine glaſſes, and, conſequently, the bubbles of air procured by freezing were expoſed to a conſiderable ſurface of water, and would in time (though not, I found, in the courſe of a day) have been abſorbed by the water more free from air, I procured glaſs veſſels of a conical form, terminating in narrow tubes, into which the air diſlodged from the ice might aſcend, and not be ſubject to be abſorbed. I was ſo fortunate as to have ſeveral veſſels of this form, and they completely anſwered my purpoſe for five or ſix proceſſes.

Theſe veſſels were firſt filled with mercury, and then I introduced into them a quantity of water freed from air by previous freezing; and when, after expoſure to froſt the ice was melted, the air diſlodged from the ice aſcended into the narrow tubes, and remained without any ſenſible diminution of bulk ſeveral days; and every time that the water was expoſed to the froſt, an addition was made to it. At length, however, though the veſſels were very ſtrong, and contained much mercury, which, by its tendency to deſcend, would give the water room to expand with the leſs danger of breaking the veſſel, none of them ſerved for more than the number of proceſſes above-mentioned.

After the breaking of my glaſs veſſels, I got other cylindrical ones made of *iron*, ſeven or eight inches in height, and near three inches wide at the bottom, the upper orifice being cloſed with a cork and cement, in the centre of which was a glaſs tube, the diameter of which was about the fifth of an inch. And as this glaſs tube was in the greateſt danger of breaking by the freezing of the water, and this had happened ſeveral times in the former experiments, notwithſtanding all my care to guard them from the froſt, I now made uſe of ſnow and ſalt to freeze the water in the iron veſſel only, placed in a veſſel of mercury, having been previouſly filled as the glaſs veſſels had been.

The water on which I now operated was about three ounces, and it had been made as free as poſſible from air by previous freezing. With this apparatus I repeated the proceſs of freezing nine times, without changing the water; and the laſt portion of air that I procured in this manner was as great as any of the preceding; ſo that there remained no reaſonable doubt, but that air might be produced from the ſame water, in this manner, *ad libitum*; and having got near two inches of air in the glaſs tube, I put an end to the experiment; and examining the air, I found it to be wholly *phlogiſticated*, not being affected by nitrous air, and having nothing inflammable in it.

During the proceſs of freezing, the air in the tube was generally compreſſed into one-fifth of its uſual bulk; but when I began to thaw the ice, which I did by means of hot water in the place of the freezing mixture, it ſoon expanded to its former dimenſions, and no ſenſible portion of it was abſorbed during the whole proceſs, which was about a week. Sometimes the violence of the preſſure, occaſioned by the expanſion of the water in freezing, would force a little water out of the veſſel between the cork and the glaſs tube, or the iron veſſel, which preſently became ice. This I always carefully removed, and applied freſh cement to the place, to prevent the introduction of any air from without, before I began to melt the ice. And that no external air had entered, was evident both from the manner in which the air was produced after the water recovered its fluidity, and from the quality of it when examined after the proceſs.

In the courſe of the experiments with the glaſs veſſels, a phenomenon occurred which was wholly unexpected by me, and which was very amuſing. Having left the veſſels filled in part with water, and in part with mercury, in the evening, I generally found them in the morning ſeemingly quite full of mercury, every part of the ice within the veſſel being covered with it. This muſt have been occaſioned by a vacuum having been formed between the glaſs and the ice, and into this ſpace mercury had been drawn up on the principle of the capillary tube. When this was not the caſe, the interſtices of the ice towards the centre were filled with thin laminæ of mercury, which alſo exhibited a curious appearance.

Sometimes, when there was no mercury between the glaſs and the ice, an interſtice was made between them when they were placed within the influence of the fire. In theſe circumſtances I have ſeen the mercury drawn up to the height of ſeveral inches. As this ſpace became enlarged by the increaſe of the heat, the laminæ of mercury were con-

tracted, till coming into the form of balls, too heavy to be ſupported; they fell down to the maſs of mercury in the baſon.

The moſt obvious inference from theſe experiments is, that water, when reduced by any means to the ſtate of vapour, is in part converted into phlogiſticated air, and that this is one of the means provided by nature to keep up the equilibrium of this conſtituent part of the atmoſphere; as the influence of light on growing vegetables is the means of recruiting the other parts of it, viz. dephlogiſticated air, and both of them are ſubject to abſorption and diminution in ſeveral natural proceſſes. Inflammable air, I have alſo ſhewn to be convertible into phlogiſticated air; ſo that this is another method of ſupplying the atmoſphere with this ingredient in its compoſition.

That water contains phlogiſton, I have ſhewn to be probable from ſeveral conſiderations, eſpecially that of its reſembling metals in their property of being conductors of electricity. For theſe ſubſtances certainly contain phlogiſton, if there be any ſuch thing. Mercury alſo becomes ſuper-phlogiſticated by agitation in water, and this without limit, and without changing the water or the mercury; and the remaining water contains no more oxygen than before. For the air expelled from it is not more pure, but conſiderably leſs ſo, and it is perfectly free from acidity.

I would further obſerve, that theſe experiments, which prove the converſion of water into phlogiſticated air, are inconſiſtent with the antiphlogiſtic theory, which makes water reſolvable into dephlogiſticated and inflammable air; but that they are highly favourable to the hypotheſis of water being the baſis of every kind of air, the difference between them depending upon the addition of ſome principles which we are not able to aſcertain by weight. Alſo, if any ſpecies of air be entitled to the appellation of *hydrogen*, it is not inflammable, but phlogiſticated air.

II.

On the Abſorbent Powers of different Earths. By Mr. JOHN LESLIE. *Communicated by the Author.*

IN completing my reſearches in hygrometry, I have been led to examine the habitudes of the earthy bodies with reſpect to moiſture. I had already diſcovered, that animal and vegetable matters, like the ſaline and deliqueſcent ſubſtances, attract humidity by a force altogether diſtinct from that of capillary abſorption, and attended with diminution of volume, evolution of heat, and the other concomitants which mark a real change of conſtitution. And while the empire of chemical agency ſeemed extending on every ſide, it was reaſonable to doubt if the earths themſelves continued entirely inert and paſſive under the atmoſpherical influences. My ſuſpicions were fully confirmed by experiment. *All the earths and ſtony bodies eminently attract moiſture from the air, and this with different*

degree:

degrees of force, modified alſo by temperature, which affects the meaſure of all combinations. The facts thus obtained, though not of a brilliant nature, appear highly inſtructive, and if proſecuted with attention, might throw ſome light on the obſcure theory of vegetation, and ſuggeſt uſeful improvements in agriculture and rural œconomy. I give this early notice of the little which I have done, as it ſeems to diverge from that line of inquiry which I had preſcribed to myſelf, and as I ſhall not at leaſt for the preſent, purſue it farther. But I earneſtly invite chemiſts and naturaliſts to reſume a ſubject which promiſes ſuch an eaſy and abundant harveſt.

My procedure was this:—The earths, or the ſtony matters groſsly pounded, were dried thoroughly before a good fire, and immediately introduced into phials, which were ſtopped up and ſet aſide to cool. The firſt object was to aſcertain, whether the degree of heat to which they were ſubjected in drying, would affect their abſorbent quality. I ſoon perceived, however, that the reſults were quite regular and uniform. In fact, the powder is dried in this caſe not by the action of the hot air on its ſurface, but by the heat penetrating the maſs, and communicating to the aqueous particles the diſpoſition to aſſume the vaporous ſtate, with the correſponding elaſticity of ſteam, which beyond the boiling point increaſes with moſt rapid progreſſion, and which enormous diſtenſive power ſoon overcomes the obſtinate adheſion to the earthy baſis. Any heat almoſt between the 100th and 300th degree of the centigrade ſcale, will bring the ſubſtances into the proper condition. This deſiccated powder is ſtrewed on the bottom of a ſaucer holding an hygrometer, over which a ſmall receiver is inverted. In a few minutes the inſtrument marks the higheſt degree of dryneſs produced. Here are a few of the principal reſults indicated by this mode, at the temperature of 16° centigrade.

Free ſtone	37°
Fine ſea ſand	40
Marble	47
Common clay	68
Sea ſand, lately cultivated	72
Sandy ſchiſt	75
Bog earth	77
Rotten whinſtone	78
Garden mould	80

It is remarkable that marble and quicklime produce exactly the ſame effect, and that in general no ſenſible difference is perceived between the pure earths and their carbonates. The great abſorbent power of the argillaceous, compared with the ſilicious bodies, alſo deſerves particular notice. But the cultivated ſoils poſſeſs that power in the moſt conſpicuous degree. Garden mould ſtands at the top of the ſcale, and even ſea ſand, which cauſes a dryneſs of 40° only is, by a few years' tillage, rendered capable of producing that of 72°. Nor is it the operation of manure, for the ſingle effect of this material is inferior to that of moſt of the earths. It ſeems highly probable, that the fertility of ſoils depends chiefly on their diſpoſition to imbibe moiſture. Manures perhaps act as ſtimulants merely, the carbonaceous matter of plants is derived from the atmoſphere, and the earth affords expanſion for the fibrils of the roots, and ſupplies them with the aqueous element. Rotten whinſtone conſtitutes the body of the ſoil in this fertile county, and it manifeſts among the ſtony compounds a ſuperior degree of the power of abſorption.

Torrefaction ſeems remarkably to diminiſh the faculty of the earthy ſubſtances to attract moiſture. Clay, roaſted in a ſtrong fire, from 68 gives only 35 degrees by the hygrometer, and after being urged in a blackſmith's forge, affords no more than 8; whinſtone, which in ordinary caſes has a power equal to 78°, ſhews only 23°, after expoſure to the fierce heat of a forge. Nor is the effect occaſioned by any partial or incipient vitrification, for ſandſtone, which has undergone a violent ignition, ſhews likewiſe a ſimilar change of property. If bodies therefore ſuffered no alteration of contexture from the impreſſions of the atmoſphere, geologiſts would be furniſhed with one certain criterion, to decide whether a foſſil owed its formation to the agency of fire or water. It is almoſt ſuperfluous to obſerve, how vague and inconcluſive are the arguments uſually employed by the contending ſects of Neptuniſts and Plutoniſts.

But although the mode of experimenting above deſcribed ſufficiently marks their diſcriminating qualities, it does not exhibit the full effect of the deſiccated powders; ſince, the hygrometer and the earth being included together, while the latter abſtracts moiſture from the air within the receiver, the former by its wetted ball continually imparts humidity, which notwithſtanding the extenſive ſurface of abſorption, muſt always enfeeble the performance in proportion to the time of action. The moſt accurate way of experimenting, is to throw the powders, either from the phials, or directly on its being withdrawn from the fire, into a large flattened glaſs veſſel with a narrow neck, which is corked up for ſeveral hours, and the condition of the included air may then be examined by letting down a ſmall hygrometer ſuſpended by a thread from the ſtopper. For want of more ſuitable apparatus, I uſed wine decanters, and having provided myſelf with ſpecimens of the primitive earths, I began with determining their relative abſorbent powers. The reſults at the ſame temperature, of 16° centigrade, were as follow:

Carbonate of ſtrontian	23°
Carbonate of barytes	32
Quartz	40
Marble	70
Carbonate of magneſia	75
Alumine	84

This furniſhes another proof that ſtrontianite is an independant earth, ſince it ſtands apart from the reſt. Quartz in ſmall rounded fragments, like pigeon's eggs, picked up on the beach, gave almoſt the ſame reſult as when reduced to a powder. In general, the only uſe of pounding the ſtones is to accelerate their performance. It might be preſumed, that the mixtures of thoſe earths would produce intermediate effects; yet equal parts of ſilex and alumine gave as much as the latter ſingly. But the *quantity* of abſorption muſt be diſtinguiſhed from its *intenſity*. Thus ſtrontianite, barytes, and ſilex, are quickly ſaturated with moiſture, while magneſia and alumine continue to imbibe it for a conſiderable time. The effects in mixture depend on thoſe qualities combined: after the ſilex has ceaſed to act, the alumine perſeveres, and through the intermediate agency of the air, diveſts the former of the minute portion of humidity which it had attracted. But the compounds of the ſimple earths are ſtill more remarkable in their affection to moiſture, ſince it ſurpaſſes that of their ingredients. Thus, ſea ſand gives 70°; whinſtone, 80°; though it conſiſts one half of ſilex, and the other half of alumine, and the oxide of iron * in nearly equal parts; and pipe clay, which contains a large proportion of ſilex, 85°. It is evident, that the abſorbent power of earths depends as much on their mechanical condition, as on the ſpecies of matter of whiah they are compoſed. Whatever tends to harden them diminiſhes their effect, and the contrary. Hence the reaſon why the action of fire impairs their deſiccating quality. Quartz, urged in a blackſmith's forge, gives only 19°; but the ſame powder, after being ſoaked a week in water, gave 35°; and moſt probably it would have in time recovered its whole original power. The proceſs by which nature gradually divides, ſoftens, and diſpoſes ſtony bodies to abſorb moiſture, is beautifully illuſtrated in the inſtance of whinſtone or baſalt. A piece of ſolid whinſtone gave 80 degrees by the hygrometer, another piece rotten and crumbling, gave 86°; but another portion of the ſame rock already reduced to mould, afforded 92 †. The ameliorating effects of culture is exemplified in ſea ſand: fine ſand cauſed a dryneſs of 70°; ſand collected from the paths of a ſheep walk near the beach 78°; the ſame ſand, lately brought into cultivation 85°. Still theſe effects are inferior to that of garden mould, which amounts to 95°, and to which decompoſed whinſtone approaches the neareſt. Comparing thoſe facts with the property lately diſcovered by Humboldt, that the earths attract the oxygenous portion of the air, we farther perceive the intimate concatenation of changes which maintain this mundane ſyſtem. The lap of nature is prepared for the reception and developement of the vegetable germs, exhibiting in a reciprocating ſeries of growth and decay, the ever renovating forms of organic bodies through endleſs ſucceſſions of cycles.

Sulphuric acid nearly doubles its deſiccating power at every 15 degrees of elevation of temperature, that at zero being 60°. The ſeveral earths appear to follow progreſſions not much different.

The relative effects of different ſoils may be aſcertained more eaſily, by altering the conſtruction of the hygrometer. Connect the two balls by a long bent tube paſſed through two corks, and cover and wet them both. In this ſtate they are introduced into the wine decanters containing the two earths to be examined; the riſe or depreſſion of the liquor will indicate which has the greateſt abſorbent power, and the meaſure of its exceſs. I conſtructed ſuch an inſtrument, but have not yet puſhed the inquiry to any extent.

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* The oxide of iron and zinc which I have tried, ſhewed leſs abſorbent powers than the earthy bodies.

† Thoſe ſpecimens were taken from a remarkable ſpot on the ſummit of Largo Lau, an elegant conical mountain, which riſes behind this place to the height of about 800 feet above the level of the ſea. The whinſtone parts into ſmall hexagonal columns, of which one of the ſides is ſometimes almoſt effaced.

Before I conclude, I muſt notify that the deſiccating quality of flannel may be employed for the graduation of the hygrometer, inſtead of that of the ſulphuric acid. A large piece of flannel dried well at a fire, laid between the leaves of a book to cool, and then placed in folds within a large receiver, will produce a dryneſs of 80° at the temperature of 16°. Thoſe, however, who wiſh to purchaſe the hygrometer or photometer, may apply to Mr. Cary, optician, in the Strand.

JOHN LESLIE.

Largo, Fifeſhire, June 9, 1800.

III.

On a New Fulminating Mercury. By EDWARD HOWARD, *Eſq. F.R.S.*

(Continued from page 178.)

HAD the alchemiſt propoſed to fix water by the ſame apparatus, the neſt of boxes muſt, I ſuppoſe, have likewiſe been ruptured; yet it does not follow that the exploſion would have been ſo tremendous: indeed it is probable that it would not, for if (as Mr. Kirwan remarked to me) ſubſtances which have the greateſt ſpecific gravity, have likewiſe the greateſt attraction of coheſion, the ſuppoſition that the vapour of mercury exceeds in expanſive force the vapour of water, would agree with a poſition of Sir Iſaac Newton, *that thoſe particles recede from one another with the greateſt force, and are moſt difficultly brought together, which upon contact cohere moſt ſtrongly* [*].

SECTION IX.

Before I attempt to inveſtigate the conſtituent principles of this powder, it will be proper to deſcribe the proceſs and manipulations which, from frequent trials, ſeem to me beſt calculated to produce it.

100 grains, or a greater proportional quantity, of quickſilver (not exceeding 500 grains [†]) are to be diſſolved, with heat, in a meaſured ounce and a half of nitric acid [‡]. This ſolution being poured cold *upon* two meaſured ounces of alcohol [§], *previouſly* introduced into

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[*] Newton's Optics, p. 372, 4th Ed. Lond. 1730.

[†] The reaſon of this limitation is not on account of any danger attending the proceſs; but becauſe the quantities of nitric acid and alcohol required for more than 500 grains, would excite a degree of heat detrimental to the preparation.

[‡] Of the ſpecific gravity of about 1,3.

[§] Of the ſpecific gravity of about ,849.

any convenient glaſs veſſel, a moderate heat is to be applied until an effervefcence is excited. A white fume then begins to undulate on the ſurface of the liquor; and the powder will be gradually precipitated, upon the ceſſation of action and re-action. The precipitate is to be immediately collected on a filter, well waſhed with diſtilled water, and carefully dried in a heat not much exceeding that of a water bath. The immediate edulcoration of the powder is material, becauſe it is liable to the re-action of the nitric acid; and, whilſt any of that acid adheres to it, it is very ſubject to the influence of light. Let it alſo be cautiouſly remembered, that the mercurial ſolution is to be poured *upon* the alcohol.

I have recommended quickſilver to be uſed in preference to an oxide, becauſe it ſeems to anſwer equally, and is leſs expenſive; otherwiſe, not only the pure red oxide, but the red nitrous oxide, and turpeth, may be ſubſtituted; neither does it ſeem eſſential to attend to the preciſe ſpecific gravity of the acid, or the alcohol. The rectified ſpirit of wine and the nitrous acid of commerce, never failed, with me, to produce a fulminating mercury. It is indeed true, that the powder prepared without attention, is produced in different quantities, varies in colour, and probably in ſtrength. From analogy, I am diſpoſed to think the whiteſt is the ſtrongeſt; for it is well known, that black precipitates of mercury approach the neareſt to the metallic ſtate. The variation in quantity is remarkable; the ſmalleſt quantity I ever obtained from 100 grains of quickſilver being 120 grains, and the largeſt 132 grains. Much depends on very minute circumſtances. The greateſt product ſeems to be obtained, when a veſſel is uſed which condenſes and cauſes moſt ether to return into the mother liquor; beſides which, care is to be had in applying the requiſite heat, that a ſpeedy, and not a violent action be effected. 100 grains of an oxide are not ſo productive as 100 grains of quickſilver.

As to the colour, it ſeems to incline to black, when the action of the acid on the alcohol is moſt violent, and *vice verſâ.*

SECTION X.

I need not obſerve, that the gaſes which were generated during the combuſtion of the powder in the glaſs globe, were neceſſarily mixed with atmoſpheric air; the facility with which the electric fluid paſſes through a vacuum, made ſuch a mixture unavoidable.

The cubical inch of gas received over water was not readily abſorbed by it: and, as it ſoon extinguiſhed a taper, without becoming red, or being itſelf inflamed, barytes water was let up to the three cubical inches received over mercury, when a carbonate of barytes was immediately precipitated.

The reſidue of ſeveral exploſions, after the carbonic acid had been ſeparated, was found, by the teſt of nitrous gas, to contain nitrogen or azotic gas; which does not proceed from any decompoſition of atmoſpheric air, becauſe the powder may be made to explode under the exhauſted receiver of an air-pump. It is therefore manifeſt, that the gaſes generated during the combuſtion of the fulminating mercury, conſiſt of carbonic acid and nitrogen gaſes.

SECTION XI.

The principal re-agents which decompoſe the mercurial powder, are the nitric, the ſulphuric, and the muriatic acids. The nitric changes the whole into nitrous gas, carbonic acid gas, acetous acid, and nitrate of mercury. I reſolved it into theſe different principles, by diſtilling it pneumatically with nitric acid: this acid, upon the application of heat, ſoon diſſolved the powder, and extricated a quantity of gas, which was found, by well known teſts, to be nitrous gas mixed with carbonic acid gas. The diſtillation was carried on until gas no longer came over. The liquor of the retort was then mixed with the liquor collected in the receiver, and the whole ſaturated with pot-aſh; which precipitated the mercury in a yellowiſh-brown powder, nearly as it would have done from a ſolution of nitrate of mercury. This precipitate was ſeparated by a filter, and the filtrated liquor evaporated to a dry ſalt, which was waſhed with alcohol. A portion of the ſalt being refuſed by this menſtruum, it was ſeparated by filtration, and recognized, by all its properties, to be nitrate of pot-aſh. The alcoholic liquor was likewiſe evaporated to a dry ſalt, which, upon the affuſion of a little concentrate ſulphuric acid, emitted acetous acid, contaminated with a feeble ſmell of nitrous acid, owing to the ſolubility of a ſmall portion of the nitre in the alcohol.

SECTION XII.

The ſulphuric acid acts upon the powder in a remarkable manner, as already has been noticed. A very concentrate acid produces an exploſion nearly at the inſtant of contact, on account, I preſume, of the ſudden and copious diſengagement of caloric from a portion of the powder which is decompoſed by the acid. An acid ſomewhat leſs concentrate likewiſe extricates a conſiderable quantity of caloric, with a good deal of gas; but, as it effects a complete decompoſition, it cauſes no exploſion. An acid diluted with an equal quantity of water, by the aid of a little heat, ſeparates the gas ſo much leſs rapidly, that it may with ſafety be collected in a pneumatic apparatus. But, whatever be the denſity of the acid, (provided no exploſion be produced,) there remains in the ſulphuric liquor, after the ſeparation of the gas, a white uninflammable and uncryſtalized powder, mixed with ſome minute globules of quickſilver.

To eſtimate the quantity, and obſerve the nature, of this uninflammable ſubſtance, I treated 100 grains of the fulminating mercury with ſulphuric acid a little diluted. The gas being ſeparated, I decanted off the liquor as it became clear, and freed the inſoluble powder from acid, by edulcoration with diſtilled water; after which, I dried it, and found it weighed only 84 grains; conſequently had loſt 16 grains of its original weight. Suſpecting, from the operation of the nitric acid in the former experiment, that theſe 84 grains (with the exception of the quickſilver globules) were oxalate of mercury, I digeſted them in nitrate of lime, and found my ſuſpicion juſt. The mercury of the oxalate united to the nitric acid, and the oxalic acid to the lime. A new inſoluble compound was formed;

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it weighed, when waſhed and dry, 48,5 grains. Carbonate of pot-aſh ſeparated the lime, and formed oxalate of pot-aſh, capable of precipitating lime-water, and muriate of lime; although it had been depurated from exceſs of alkali, and from carbonic acid, by a previous addition of acetous acid. That the mercury of the oxalate in the 84 grains, had united to the nitric acid of the nitrate of lime, was proved by dropping muriatic acid into the liquor from which the ſubſtance demonſtrated to be oxalate of lime had been ſeparated; for a copious precipitation of calomel inſtantly enſued.

The ſulphuric liquor, decanted from the oxalate of mercury, was now added to that with which it was edulcorated, and the whole ſaturated with carbonate of pot-aſh. As effervefcence ceaſed, a cloudineſs and precipitation followed; and the precipitate, being collected, waſhed, and dried, weighed 3,4 grains: it appeared to be a carbonate of mercury. Upon evaporating a portion of the ſaturated ſulphuric liquor, I found nothing but ſulphate of pot-aſh; nor had it any metallic taſte. There then remains, without allowing for the weight of the carbonic acid united to the 3,4 grains, a deficit from the 100 grains of mercurial powder, of 12,6 grains, which I aſcribe to the gas ſeparated by the action of the ſulphuric acid. To aſcertain the quantity, and examine the nature, of the gas ſo ſeparated, I introduced into a very ſmall tubulated retort, 50 grains of the mercurial powder, and poured upon it 3 drams, by meaſure, of ſulphuric acid, diluted with an equal quantity of water, and extricated the gas with the aſſiſtance of a gentle heat. I firſt received it over quickſilver, the ſurface of which, during the operation, partially covered itſelf with a little black powder*.

The gas, by different trials, amounted from 28 to 31 cubical inches; it at firſt appeared to be nothing but carbonic acid, as it precipitates barytes water, and extinguiſhed a taper, without being itſelf inflamed, or becoming red. But, upon letting up to it liquid cauſtic ammoniac, there was a reſidue of from 5 to 7 inches of a peculiar inflammable gas, which burnt with a greeniſh blue flame. When I made uſe of the water-tub, I obtained, from the ſame materials, from 25 to 27 inches only of gas, although the average quantity of the peculiar inflammable gas was likewiſe from 5 to 7 inches; therefore, the difference of the aggregate product, over the two fluids, muſt have ariſen from the abſorption, by the water, of a part of the carbonic acid in its naſcent ſtate. The variation of the quantity of the inflammable gas, when powder from the ſame parcel is uſed, ſeems to depend upon the acid being a little more or leſs dilute.

With reſpect to the nature of the peculiar inflammable gas, it is plain to me, from the reaſons I ſhall immediately adduce, that is is no other than the gas (in a pure ſtate) into which the nitrous etherized gas can be reſolved, by treatment with dilute ſulphuric acid.

The Dutch chemiſts have ſhewn †, that the nitrous etherized gas can be reſolved into nitrous gas, by expoſure to concentrate ſulphuric acid, and that, by uſing a dilute inſtead of a concentrate acid, a gas is obtained which enlarges the flame of a burning taper, ſo much like the gaſeous oxide of azote, that they miſtook it for that ſubſtance, until they diſcovered that it was permanent over water, refuſed to detonate with hydrogen, and that the fallacious appearance was owing to a mixture of nitrous gas with an inflammable gas.

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* I cannot account for this appearance.

† *Journal de Phyſique*, p. 250, October, 1794.

The inflammable gas ſeparated from the powder, anſwers to the deſcription of the gas which at firſt deceived the Dutch chemiſts; 1ſt, in being permanent over water; 2dly, refuſing to detonate with hydrogen; and, 3dly, having the appearance of the gaſeous oxide of azote, when mixed with nitrous gas.

The gas ſeparable by the ſame acid, from nitrous etherized gas, and from the mercurial powder, have therefore the ſame properties. Every chemiſt would thence conclude, that the nitrous etherized gas is a conſtituent part of the powder, had the inflammable and nitrous gas, inſtead of the inflammable and carbonic acid gas, been the mixed product extricated from it by dilute ſulphuric acid.

It however appears to me, that nitrous gas was really produced by the action of the dilute ſulphuric acid; and that, when produced, it united to an exceſs of oxygen preſent in the oxalate of mercury.

To explain how this change might happen, I muſt premiſe, that my experiments have ſhewn me, that oxalate of mercury can exiſt in two, if not in three ſtates.

1ſt, By the diſcovery of Mr. Ameilon already quoted, the precipitate obtained by oxalic acid, from nitrate of mercury, fuſes with a hiſſing noiſe. This precipitate is an oxalate of mercury, ſeemingly with exceſs of oxygen. Mercury diſſolved in ſulphuric acid and precipitated by oxalic acid, and alſo the pure red oxide of mercury digeſted with oxalic acid, give oxalates in the ſame ſtate.

2dly, Acetate of mercury precipitated by oxalic acid, although a true oxalate is formed, has no kind of inflammability. I conſider it as an oxalate with leſs oxygen than thoſe above-mentioned.

3dly, A ſolution of nitrate of mercury boiled with dulcified ſpirit of nitre, gives an oxalate more inflammable than any other: perhaps it contains moſt oxygen.

The oxalate of mercury remaining from the powder in the ſulphuric liquor, is not only always in the ſame ſtate as that precipitated from acetate of mercury, entirely devoid of inflammability, but contains globules of quickſilver; conſequently, it muſt have parted with even more than its exceſs of oxygen; and, if nitrous gas was preſent, it would of courſe ſeize at leaſt a portion of that oxygen. It is true, that globules of quickſilver may ſeem incompatible with nitrous acid; but the quantity of the one may not correſpond with that of the other, or the dilution of the acid may deſtroy its action.

As to the preſence of the carbonic acid, it muſt have ariſen either from a complete* decompoſition of a part of the oxalate; or, admitting the nitrous etherized gas to be a conſtituent principle of the powder, from a portion of the oxygen, not taken up by the nitrous gas, being united with the carbon of the etherized gas.

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* Inflammable oxalate of mercury, made to fuſe in a retort connected with the quickſilver tub, gives out carbonic acid gas.

SECTION XIII.

The muriatic acid digeſted with the mercurial powder, diſſolves a portion of it, without extricating any notable quantity of gas. The diſſolution evaporated to a dry ſalt, taſtes like corroſive ſublimate; and the portion which the acid does not take up, is left in the ſtate of an uninflammable oxalate.

SECTION XIV.

Theſe effects all tend to eſtabliſh the exiſtence of the nitrous etherized gas, as a conſtituent part of the powder; and likewiſe corroborate the explanation I have ventured to give, of the action of the ſulphuric acid. Moreover, a meaſured ounce and a half of nitrous acid, holding 100 grains of mercury in ſolution, and 2 meaſured ounces of alcohol, yield 90 cubical inches only of gas: whereas, without the intervention of mercury, they yield 210 inches. Upon the whole, I truſt it will be thought reaſonable to conclude, that the mercurial powder is compoſed of the nitrous etherized gas, and of oxalate of mercury with exceſs of oxygen.

1ſt, Becauſe the nitric acid converts the mercurial powder intirely into nitrous gas, carbonic acid gas, acetous acid, and nitrate of mercury.

2dly. Becauſe the dilute ſulphuric acid reſolves it into an uninflammable oxalate of mercury, and ſeparates from it a gas reſembling that into which the ſame acid reſolves the nitrous etherized gas.

3dly. Becauſe an uninflammable oxalate is likewiſe left, after the muriatic acid has converted a part of it into ſublimate.

4thly. Becauſe it cannot be formed by boiling nitrate of mercury in dulcified ſpirit of nitre; although a very inflammable oxalate is by this means produced.

5thly. Becauſe the difference of the product of gas, from the ſame meaſures of alcohol and nitrous acid, with and without mercury in ſolution, is not trifling; and,

6thly. Becauſe nitrogen gas was generated during its combuſtion in the glaſs globe.

Should my concluſions be thought warranted by the reaſons I have adduced, the theory of the combuſtion of the mercurial powder will be obvious to every chemiſt. The hydrogen of the oxalic acid, and of the etherized gas, is firſt united to the oxygen of the oxalate, forming water[*]; the carbon is ſaturated with oxygen, forming carbonic acid gas; and a part, if not the whole of the nitrogen of the etherized gas, is ſeparated in the ſtate of nitrogen gas; both of which laſt gaſes, it may be recollected, were after the exploſion preſent in the glaſs globe. The mercury is revived, and, I preſume, thrown into vapour; as may well be imagined, from the immenſe quantity of caloric extricated, by adding concentrate ſulphuric acid to the mercurial powder.

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[*] Drops of water were obſerved on the internal ſurface of the globe, the day after ſeveral exploſions had been produced in its centre.

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tracted, till coming into the form of balls, too heavy to be ſupported; they fell down to the maſs of mercury in the baſon.

The moſt obvious inference from theſe experiments is, that water, when reduced by any means to the ſtate of vapour, is in part converted into phlogiſticated air, and that this is one of the means provided by nature to keep up the equilibrium of this conſtituent part of the atmoſphere; as the influence of light on growing vegetables is the means of recruiting the other parts of it, viz. dephlogiſticated air, and both of them are ſubject to abſorption and diminution in ſeveral natural proceſſes. Inflammable air, I have alſo ſhewn to be convertible into phlogiſticated air; ſo that this is another method of ſupplying the atmoſphere with this ingredient in its compoſition.

That water contains phlogiſton, I have ſhewn to be probable from ſeveral conſiderations, eſpecially that of its reſembling metals in their property of being conductors of electricity. For theſe ſubſtances certainly contain phlogiſton, if there be any ſuch thing. Mercury alſo becomes ſuper-phlogiſticated by agitation in water, and this without limit, and without changing the water or the mercury; and the remaining water contains no more oxygen than before. For the air expelled from it is not more pure, but conſiderably leſs ſo, and it is perfectly free from acidity.

I would further obſerve, that theſe experiments, which prove the converſion of water into phlogiſticated air, are inconſiſtent with the antiphlogiſtic theory, which makes water reſolvable into dephlogiſticated and inflammable air; but that they are highly favourable to the hypotheſis of water being the baſis of every kind of air, the difference between them depending upon the addition of ſome principles which we are not able to aſcertain by weight. Alſo, if any ſpecies of air be entitled to the appellation of *hydrogen*, it is not inflammable, but phlogiſticated air.

II.

On the Abſorbent Powers of different Earths. By Mr. JOHN LESLIE. Communicated by the Author.

IN completing my reſearches in hygrometry, I have been led to examine the habitudes of the earthy bodies with reſpect to moiſture. I had already diſcovered, that animal and vegetable matters, like the ſaline and deliqueſcent ſubſtances, attract humidity by a force altogether diſtinct from that of capillary abſorption, and attended with diminution of volume, evolution of heat, and the other concomitants which mark a real change of conſtitution. And while the empire of chemical agency ſeemed extending on every ſide, it was reaſonable to doubt if the earths themſelves continued entirely inert and paſſive under the atmoſpherical influences. My ſuſpicions were fully confirmed by experiment. *All the earths and ſtony bodies eminently attract moiſture from the air, and this with different*

degree;

degrees of force, modified alſo by temperature, which affects the meaſure of all combinations. The facts thus obtained, though not of a brilliant nature, appear highly inſtructive, and if proſecuted with attention, might throw ſome light on the obſcure theory of vegetation, and ſuggeſt uſeful improvements in agriculture and rural œconomy. I give this early notice of the little which I have done, as it ſeems to diverge from that line of inquiry which I had preſcribed to myſelf, and as I ſhall not at leaſt for the preſent, purſue it farther. But I earneſtly invite chemiſts and naturaliſts to reſume a ſubject which promiſes ſuch an eaſy and abundant harveſt.

My procedure was this:—The earths, or the ſtony matters groſsly pounded, were dried thoroughly before a good fire, and immediately introduced into phials, which were ſtopped up and ſet aſide to cool. The firſt object was to aſcertain, whether the degree of heat to which they were ſubjected in drying, would affect their abſorbent quality. I ſoon perceived, however, that the reſults were quite regular and uniform. In fact, the powder is dried in this caſe not by the action of the hot air on its ſurface, but by the heat penetrating the maſs, and communicating to the aqueous particles the diſpoſition to aſſume the vaporous ſtate, with the correſponding elaſticity of ſteam, which beyond the boiling point increaſes with moſt rapid progreſſion, and which enormous diſtenſive power ſoon overcomes the obſtinate adheſion to the earthy baſis. Any heat almoſt between the 100th and 300th degree of the centigrade ſcale, will bring the ſubſtances into the proper condition. This deſiccated powder is ſtrewed on the bottom of a ſaucer holding an hygrometer, over which a ſmall receiver is inverted. In a few minutes the inſtrument marks the higheſt degree of dryneſs produced. Here are a few of the principal reſults indicated by this mode, at the temperature of 16° centigrade.

Free ſtone	37°
Fine ſea ſand	40
Marble	47
Common clay	68
Sea ſand, lately cultivated	72
Sandy ſchiſt	75
Bog earth	77
Rotten whinſtone	78
Garden mould	86

It is remarkable that marble and quicklime produce exactly the ſame effect, and that in general no ſenſible difference is perceived between the pure earths and their carbonates. The great abſorbent power of the argillaceous, compared with the ſilicious bodies, alſo deſerves particular notice. But the cultivated ſoils poſſeſs that power in the moſt conſpicuous degree. Garden mould ſtands at the top of the ſcale, and even ſea ſand, which cauſes a dryneſs of 40° only is, by a few years' tillage, rendered capable of producing that of 72°. Nor is it the operation of manure, for the ſingle effect of this material is inferior to that of moſt of the earths. It ſeems highly probable, that the fertility of ſoils depends chiefly on their

tracted, till coming into the form of balls, too heavy to be ſupported; they fell down to the maſs of mercury in the baſon.

The moſt obvious inference from theſe experiments is, that water, when reduced by any means to the ſtate of vapour, is in part converted into phlogiſticated air, and that this is one of the means provided by nature to keep up the equilibrium of this conſtituent part of the atmoſphere; as the influence of light on growing vegetables is the means of recruiting the other parts of it, viz. dephlogiſticated air, and both of them are ſubject to abſorption and diminution in ſeveral natural proceſſes. Inflammable air, I have alſo ſhewn to be convertible into phlogiſticated air; ſo that this is another method of ſupplying the atmoſphere with this ingredient in its compoſition.

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II.

On the Abſorbent Powers of different Earths. By Mr. JOHN LESLIE. *Communicated by the Author.*

IN completing my reſearches in hygrometry, I have been led to examine the habitudes of the earthy bodies with reſpect to moiſture. I had already diſcovered, that animal and vegetable matters, like the ſaline and deliqueſcent ſubſtances, attract humidity by a force altogether diſtinct from that of capillary abſorption, and attended with diminution of volume, evolution of heat, and the other concomitants which mark a real change of conſtitution. And while the empire of chemical agency ſeemed extending on every ſide, it was reaſonable to doubt if the earths themſelves continued entirely inert and paſſive under the atmoſpherical influences. My ſuſpicions were fully confirmed by experiment. *All the earths and ſtony bodies eminently attract moiſture from the air; and this with different degree:*

degrees of force, modified alſo by temperature, which affects the meaſure of all combinations. The facts thus obtained, though not of a brilliant nature, appear highly inſtructive, and if proſecuted with attention, might throw ſome light on the obſcure theory of vegetation, and ſuggeſt uſeful improvements in agriculture and rural œconomy. I give this early notice of the little which I have done, as it ſeems to diverge from that line of inquiry which I had preſcribed to myſelf, and as I ſhall not at leaſt for the preſent, purſue it farther. But I earneſtly invite chemiſts and naturaliſts to reſume a ſubject which promiſes ſuch an eaſy and abundant harveſt.

My procedure was this:—The earths, or the ſtony matters groſsly pounded, were dried thoroughly before a good fire, and immediately introduced into phials, which were ſtopped up and ſet aſide to cool. The firſt object was to aſcertain, whether the degree of heat to which they were ſubjected in drying, would affect their abſorbent quality. I ſoon perceived, however, that the reſults were quite regular and uniform. In fact, the powder is dried in this caſe not by the action of the hot air on its ſurface, but by the heat penetrating the maſs, and communicating to the aqueous particles the diſpoſition to aſſume the vaporous ſtate, with the correſponding elaſticity of ſteam, which beyond the boiling point increaſes with moſt rapid progreſſion, and which enormous diſtenſive power ſoon overcomes the obſtinate adheſion to the earthy baſis. Any heat almoſt between the 100th and 300th degree of the centigrade ſcale, will bring the ſubſtances into the proper condition. This deſiccated powder is ſtrewed on the bottom of a ſaucer holding an hygrometer, over which a ſmall receiver is inverted. In a few minutes the inſtrument marks the higheſt degree of dryneſs produced. Here are a few of the principal reſults indicated by this mode, at the temperature of 16° centigrade.

Free ſtone	37°
Fine ſea ſand	40
Marble	47
Common clay	68
Sea ſand, lately cultivated	72
Sandy ſchiſt	75
Bog earth	77
Rotten whinſtone	78
Garden mould	80

It is remarkable that marble and quicklime produce exactly the ſame effect, and that in general no ſenſible difference is perceived between the pure earths and their carbonates. The great abſorbent power of the argillaceous, compared with the ſilicious bodies, alſo deſerves particular notice. But the cultivated ſoils poſſeſs that power in the moſt conſpicuous degree. Garden mould ſtands at the top of the ſcale, and even ſea ſand, which cauſes a dryneſs of 40° only is, by a few years' tillage, rendered capable of producing that of 72°. Nor is it the operation of manure, for the ſingle effect of this material is inferior to that of moſt of the earths. It ſeems highly probable, th[illegible]tility of ſoils depends chiefly on their

One of the firſt requiſites in the analyſis of a compound body, ſhould be its complete ſeparation from all other ſubſtances, which, by their preſence, may tend to introduce uncertainty into the reſults of the proceſſes that are employed. But it is ſeldom that a ſimplicity of ſubſtance can be obtained in the ſubjects of chemical reſearch; for, agreeably to a known law of affinity, the laſt portions of any ſubſtance are ſeparated with peculiar difficulty, the force of attraction appearing to increaſe, as we recede from the point of ſaturation. In a liquid ſtate, the muriatic acid is a totally unfit ſubject for analytic experiment; for, in the ſtrongeſt ſtate under which it can be procured, it ſtill contains a large proportion of water. This watery portion, beſides the complexity which it introduces into the reſults of experiments, prevents any combuſtible ſubſtance that may be applied, from acting on the truly acid part; becauſe that claſs of bodies, having leſs difficulty in attracting oxygen from water than from the acid, will neceſſarily take it from the former ſource. The ſtate of gas, therefore, is the only one in which the muriatic acid can become a proper object of analyſis.

In the ſeries of experiments on this gas, which I am now about to deſcribe, I employed the electric fluid, as an agent much preferable to artificial heat. This mode of operating enables us to confine accurately the gaſes ſubmitted to experiment; the phænomena that occur during the proceſs, may be diſtinctly obſerved; and the compariſon of the products, with the original gaſes, may be inſtituted with great exactneſs. The action of the electric fluid itſelf, as a decomponent, is extremely powerful; for it is capable of ſeparating from each other, the conſtituent parts of water, of the nitric and ſulphuric acids, of the volatile alkali, of nitrous gas, and of ſeveral other bodies, whoſe components are ſtrongly united. I began, therefore, with examining attentively the effects of the electric muriatic acid gas, without admixture *.

SECTION I.

On the Effects of Electricity on Muriatic Acid Gas.

When ſtrong electrical ſhocks were paſſed through a portion of muriatic acid gas, confined in a glaſs tube over mercury, the following appearances took place. The bulk of the gas, after 20 or 30 ſhocks, was conſiderably diminiſhed; and a white depoſit appeared on the inner ſurface of the tube, which conſiderably obſcured its tranſparency. In ſome inſtances, both the contraction and depoſit were much more remarkable than in others. The gas which iſſued from muriate of ſoda, ſoon after the affuſion of ſulphuric acid, and while the charge was yet warm, exhibited theſe appearances in an eminent degree.

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* The gaſes ſubmitted to the action of electricity, in the following experiments, were confined in ſtraight glaſs tubes of various diameters, armed at the ſealed end with a conductor of gold, or of platina, but generally of the latter metal. The ſhocks were as ſtrong as could be given without breaking the tubes, which, notwithſtanding every precaution, were often ſhattered by the force of the exploſion. Each meaſure of gas is equal to the bulk occupied by a grain of mercury.

Of this gas, 307 meaſures were reduced, by 20 ſhocks, to 227, or were contracted nearly one-fourth. Gas from the ſame materials, after they had continued working for ſome hours, was diminiſhed, by ſimilar treatment, only about a twelfth. Theſe effects, therefore, it ſeemed probable, depended in ſome meaſure on the preſence of moiſture; and I accordingly found, that muriatic acid gas, after more than a week's expoſure to muriate of lime, brought into contact with it immediately after cooling from a ſtate of fuſion, was ſcarcely diminiſhed at all; and that the depoſit, though it ſtill occurred, was leſs copious in quantity. This depoſit was not, like corroſive ſublimate, ſoluble in water; but had every property of the leſs ſaturated ſalt, calomel.

The mercury by which the muriatic acid was confined, was therefore evidently oxidated; and, to the combination of a part of the gas with the oxide thus produced, the diminution of bulk was doubtleſs to be aſcribed. But it was uncertain from whence this oxygen was derived. It might either reſult from the decompoſition of the acid gas, or of the water chemically combined with it. The following experiments were therefore made, to determine this point.

Experiment 1. Through 1457 meaſures of muriatic acid gas, 300 electrical ſhocks were paſſed. There remained, after the admiſſion of water, 100 meaſures of permanent gas, (or not quite 7 from each hundred of the original gas,) which, on trial, appeared to be purely hydrogenous.

Exper. 2. Of the gas, dried by muriate of lime, 176 meaſures received 120 ſhocks. The reſidue of hydrogenous gas amounted to 11 meaſures, or rather more than 6 *per cent.*

Theſe experiments, and other ſimilar ones, made on comparative portions of muriatic acid gas, in its recent ſtate, and after expoſure to muriate of lime, convinced me that it was impoſſible, by this method, wholly to deprive the muriatic gas of water. The recent gas, however, when electrified in ſmaller quantity than in experiment 1, gave a larger proportion of hydrogenous gas; which ſhews, that ſome portion of its moiſture was removed, by expoſure to muriate of lime. In order, if poſſible, to procure the gas perfectly dry, another mode of preparing it was reſorted to. Alum and common ſalt were firſt well calcined, ſeparately, to expel their water of cryſtallization, and, being then mixed, were diſtilled together in an earthern retort. The gas proceeding from theſe materials, was received over dry mercury; but, though only the laſt portion that came over was reſerved for experiment, it ſtill, after the uſual electrization, afforded a product of hydrogenous gas.

In the courſe of the preceding experiments, I obſerved that the diminution of the muriatic acid gas ſtopped always at a certain point, beyond which it could not be carried by continuing the ſhocks. Gas alſo, which had been thus treated, when transferred to another tube, and again electrified, did not exhibit any further depoſit. It became intereſting, therefore, to know, whether the production of hydrogenous gas had a ſimilar limitation; becauſe, the deciſion of this queſtion would go far towards aſcertaining its ſource. If the evolved hydrogenous gas aroſe from the decompoſition of the acid, it might be expected to be produced, as long as any acid remained undecompoſed. But, if water were the origin of this gas, it would ceaſe to be evolved, when the whole of the water contained in the gas had been reſolved into its conſtituent principles.

Experiments 3 and 4. Into two ſeparate tubes, I paſſed known quantities of muriatic acid gas. Through the one portion, 200 diſcharges were taken; and through the other, 400. On comparing the quantities of hydrogenous gas produced, it proved to bear exactly the ſame proportion, in each tube, to the gas originally ſubmitted to experiment. Hence it may be inferred, that the hydrogenous gas, evolved by electrifying the muriatic acid, has its origin, not from the acid, but from the water which is intimately attached to it. The agency of the electric fluid appears alſo, from the following experiments, to be exerted, not only in diſuniting the elements of water, but in promoting the union of the evolved oxygen with muriatic acid.

Exper. 5. A mixture of common air and muriatic acid gas, in the proportion of 143 of the former to 116 of the latter, was rapidly diminiſhed by electrical ſhocks; 30 of which reduced the whole to 111 *. The remainder conſiſted of muriatic acid and azote gaſes, with a ſmall proportion of oxygenous gas. The depoſit formed on the tube was of the ſame kind as before, but much more abundant.

Exper. 6. The ſame appearances were occaſioned, much more remarkably, by electrifying muriatic acid with oxygenous gas: and the contraction continued, till the mercury roſe ſo as to touch the extremity of the platina conductor. At each exploſion, a denſe white cloud was ſeen in the tube, which ſoon ſettled on its inner ſurface, and was of exactly the ſame chemical compoſition as the one already deſcribed. Nitrous gas and muriatic gas, when electrified together, underwent a ſimilar change.

In order to aſcertain whether the mercury by which the gaſes were confined, in the above experiments, had any influence on their reſults, they were repeated in an inſtrument made, purpoſely for the occaſion, by Mr. Cuthbertſon, of London. It conſiſted of a glaſs tube, ground at each end, with the view of receiving two ſtoppers, each perforated with platina wire, which projected into the cavity of the tube. When the ſtoppers were in their places, the extremities of the wires were at the diſtance of about half an inch; and, by properly diſpoſing the apparatus, electrical ſhocks might be paſſed, through any gas or mixture of gaſes, with the contact only of glaſs and platina.

Exper. 7. In this tube I electrified the muriatic acid gas, and then admitted to it an infuſion of litmus. The ſudden deſtruction of its colour evinced the formation of oxygenated muriatic acid. Not the ſmalleſt depoſit appeared on the tube.

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* This experiment ſuggeſts an additional reaſon, to that already given, for the greater diminution of the firſt, than of the ſubſequent portions of the muriatic acid gas; for the former may be preſumed to have been much more adulterated than the latter, with the atmoſpherical air of the veſſels.

Experiments 8 and 9. The ſame phænomenon took place, when an infuſion of litmus was brought into contact with a mixture of common air and muriatic acid, and of oxygenous gas and muriatic acid, after electrization in this inſtrument; oxygenated muriatic acid being produced in both caſes.

The above facts prove, that the combination of oxygen with muriatic acid, in theſe experiments, is not occaſioned by a pre-diſpoſing affinity in the mercury to combine with oxygenated muriatic acid; but that the electric fluid ſerves actually as an intermedium, in combining the muriatic acid with oxygen.

From the relation of theſe experiments it appears, that not the ſmalleſt progreſs had been made by them, towards the decompoſition of the muriatic acid. I reſolved, therefore, to attempt its analyſis, in a ſimilar manner, with the aid of combuſtible gaſes.

SECTION II.

Effects of electrifying the Muriatic Acid Gas with inflammable Subſtances.

In a memoir read before the Royal Society, and inſerted in their Tranſactions for 1797, I have ſhewn, that when electrical ſhocks are paſſed repeatedly through a confined portion of carbonated hydrogenous gas, the water held in ſolution by the gas, is decompoſed by the carbon, which forms a conſtituent part of it; that carbonic acid is formed; and an addition made, of hydrogenous gas. Hence, the bulk of the carbonated hydrogen gas is conſiderably enlarged by this proceſs; which ſhews, by its reſults, that the affinity of carbon for oxygen, is rendered much more powerful and efficient by the electric fluid. I have ſince found, that other oxygenated ſubſtances are decompoſed, by electrifying them with carbonated hydrogen gas. Nitrous gas, for example, is ſpeedily deſtroyed by this proceſs, and carbonic acid and azotic gaſes are obtained.

Every attempt to decompoſe the muriatic acid, muſt be founded on the preſumption that it is an oxygenated ſubſtance; and thoſe bodies promiſe to be the moſt ſucceſsful agents, that poſſeſs a ſtrong affinity for oxygen. Now, of all known bodies, charcoal moſt ſtrongly attracts oxygen; and I have, therefore, repeatedly attempted the deſtruction of this acid, by paſſing it over red-hot charcoal. But, in a ſeries of experiments, which I made ſome time ſince, with this view, in conjunction, with Mr. Rupp, we ſoon found reaſon to be diſſatisfied with the difficulty and uncertainty of this proceſs. An immenſe production of hydrogenous gas took place; but it was not eaſy to determine whether it had its origin from real acid, or from water. Our experiments, however, though inſufficient to furniſh deciſive proof, induced us to believe that it had the latter origin.

It next occurred to me, that the comparative affinities of the muriatic radical, whatever it may be, and of charcoal, for oxygen, would be elegantly and ſatisfactorily aſcertained, by electrifying together the carbonated hydrogen and muriatic gaſes. If the muriatic acid be capable of decompoſition by carbon, it might be expected to be deſtroyed by this proceſs; and the exact quantity of acid decompoſed, and the nature and quantity of the products, would thus be eaſily determined. I electrified, therefore, the muriatic acid and carbonated hydrogen gaſes, with the moſt ſcrupulous attention to the phænomena and reſults. That the electric fluid might not be miſapplied, in decompoſing the water of the carbonated hydrogen gas, it was kept more than a week, before uſe, over quick lime, introduced to it while yet hot.

Exper. 10. Of this carbonated hydrogenous gas, 186 meaſures were expanded, by 130 ſhocks, to 211; that is the gas was increaſed about 1/8 its bulk.

Exper. 11. Of the ſame gas, 84 meaſures were mixed with 116 of muriatic acid gas, dried by muriate of lime. By 120 ſhocks, the mixture was a little dilated. After the admiſſion of a drop or two of water, there remained 91 meaſures; *i. e.* the addition of permanent gas was 7 meaſures, or about as much as might have been expected from the muriatic gas alone.

Exper. 12. Eighty-three meaſures of dry carbonated hydrogenous gas, with 89 of muriatic acid gas, received 200 ſhocks. The permanent reſidue, after the admiſſion of water, was 101 meaſures: the addition, therefore, amounted to 18. Of the added 18, 6 may be accounted for by the decompoſition of the water of the muriatic gas, and 10 by that of the carbonated hydrogenous gas. There remain, therefore, only 2 meaſures that can be ſuppoſed to be produced from the muriatic acid gas; a quantity too ſmall to afford grounds for ſuppoſing them to ariſe from decompoſed acid.

Exper. 13. Dry carbonated hydrogenous gas 132 meaſures,
mixed with dry muriatic gas 108
240
by 200 ſhocks, expanded to 268

Part of this gas was then transferred to another tube, and the proportion of permanent gas aſcertained. Through the remainder, 150 additional ſhocks were paſſed, before the amount of the gas thus evolved was determined. In both, it bore exactly the ſame proportion to the original gas; which ſhews, that by continuing the electrization, no further effects were produced.

(To be continued.)

V.

Eudiometric Obſervations. By Citizen BERTHOLLET *.

AS the air we breath is known to be compoſed of oxigen and azote gas, the proportions of theſe two gaſes, and the changes that may take place in the atmoſphere, have been conſtant objects of enquiry. But the beſt method of acquiring that knowledge, or the reſult on which the greateſt reliance may be placed, is not yet determined.

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* Extracted from the Memoirs on Egypt, and inſerted in the Annales de Chimie, tom. XXXIV. p. 73.

The property of nitrous gas to abſorb oxigen gas was firſt made uſe of; but it was then thought ſufficient to compare the diminutions produced in the air with which the experiment was made, and the purity of any air was conſidered to be proportioned to the diminution it ſuffered.

Inquiries were afterwards directed to aſcertain the real quantity of oxigen gas that combined with the nitrous gas; in order to determine by the abſorption which took place in the two gaſes when mixed together, the relative proportions of oxigen and azote gas contained in the air of the atmoſphere.

But the nitrous gas does not always afford conſtant reſults, unleſs by carefully obſerving the ſame manipulations. Ingenhouſz has long ſince ſhewn when the proportion of the oxigen gas is to be determined, there is no eſtabliſhed law by which the diminution attributed to the oxigen gas can be meaſured, or that of the nitrous gas which becomes concentrated with it.

I am informed by an extract from the bulletin of the Philomathic Society, that M. Humboldt has endeavoured, by ſome very ingenious experiments, to obviate the uncertainty which prevails with regard to the differences found in the nitrous gas; and that he propoſes, as a ſtrict method of determining by this gas the exact proportion of the oxigen gas, ſeveral corrections of the quantities; but I ſhall prove by experiments, upon which I am ſtill employed with Citizen Champy the younger, that this method is founded on ſuppoſitions which are inadmiſſible *.

The proof by hydrogen gas, for which we are indebted to Volta, has much preciſion, eſpecially when it is made upon oxigen gas; but it requires a complicated apparatus, and the hydrogen gas may differ according to the quantity of carbon it holds in ſolution, which may cauſe the reſult to vary conſiderably. Neverthleſs, this method may be conſidered as ſufficiently exact when the different airs are merely to be compared, and the ſame hydrogen gas is uſed to make this compariſon. But the ſame accuracy cannot be had when the abſolute quantity of oxigen is to be determined. The proportions by weight, of the oxigen and hydrogen that enter into the compoſition of water, are known with enough preciſion; but the relative ſpecific gravities of the two gaſes are not well enough determined; and they alter too much, according to the difference of the hydrogen gas, to admit of judging exactly of the diminution which takes place by the combuſtion which ſhould be attributed to the oxigen and to the hydrogen gas, and determine by that means the quantity of oxigen gas that is contained in the air with which the experiment is made.

The liquid ſulphuret of alkali offers a double advantage in giving at the ſame time the comparative ſtate of the different airs which are examined, and the proportion of the oxigen gas they contains. For all the diminution muſt here be attributed to the oxigen,

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* Theſe experiments were not finiſhed when I left Cairo, and I did not bring the notes of ſeveral reſults; for which reaſon I have been obliged to repeat them, and ſhall ſoon communicate them to the world.---B.

which is divided between the oxigen and nitrous, or the hydrogen gas in the preceding experiments. There is no neceſſity for any other corrections than thoſe which the difference of temperature and the preſſure of the atmoſphere require between the inſtant that the experiment is began, and that when the diminution of the air is meaſured.

It cannot be ſuppoſed that the abſorption of the oxigen would be incomplete when water ſufficiently charged with ſulphuret of alkali is uſed; for there is a great difference between the power which ſulphuret exerts on the oxigen gas, and the weak action the azote in the gaſeous ſtate may employ on the oxigen. And if any diminution be found in the azote which is entirely ſeparated by this means, by afterwards mixing it with nitrous gas, I ſhall ſhew that this diminution ought not to be attributed to the oxigen.

It muſt be admitted, that it cannot be ſaid that the real volume of the azote gas is that which was obtained; becauſe azote may hold ſulphuret in ſolution, or very probably the ſulphurated hydrogen which always exiſts in liquid ſulphuret; and it certainly has the ſmell, though this ſmell diſappears on waſhing it in a little water without any ſenſible diminution of volume, ſo that the difference of volume which takes place in this ſolution muſt be extremely ſmall.

There can be no apprehenſion that the azote ſhould be abſorbed by the ſulphuret, for if this abſorption took place it would continue. But the quantity of azote remains conſtantly the ſame after the oxigen is abſorbed.

It is therefore, poſſible to determine the proportion of oxigen which is found in any quantity of air, by means of the liquid ſulphuret, with all the preciſion that can reaſonably be expected in chemiſtry.

The inconvenience of this method is, that the ſulphuret acts ſlowly, and requires longer time, and particularly a low temperature; beſides it gives no certain indication when the abſorption is compleated, except the ceſſation of this diminution, which requires ſtill more time to be aſcertained.

Guyton has propoſed to uſe the dry ſulphuret, by applying the heat of a candle to an apparatus he has deſcribed. I have not tried this method, but it appears to me, that ſo ſmall a portion of matter may not come into good contact with ſo large a volume of air, by which means the abſorption of all the oxigen may be rendered uncertain. The experiment does not preſent any indication that proves its certainty.

I have recommended the ſlow combuſtion of phoſphorus. I firſt place a cylinder of phoſphorus fixed on a ſtick of glaſs in an upright veſſel, in which the air ſubjected to experiment is contained under water. If the temperature of the room be high, the veſſel is immerſed in water, in order that the phoſphorus may not melt. For the evaporation of the ſurface of the water keeps it at a temperature ſome degrees below that of the atmoſphere. Thus, during the experiments that I made here, the thermometer remained at about 36° degrees of the thermometer centigrade, and the bath in which the experiments were made was at ſix degrees below this temperature. Immediately after the phoſphorus is introduced into the air, a cloud is formed, which deſcends and mixes with the water.

When the operation is finiſhed, this cloud is not longer to be perceived. It is luminous in the dark, and as ſoon as it has diſappeared, the abſorption no longer proceeds even during the ſpace of ſeveral days. So that by this means a certain indication of the concluſion of the experiment is obtained; and if it be made in a narrow tube, it requires no more than two hours at the temperature I have mentioned *.

The air with which the experiment was made, is meaſured in a graduated tube, and when the operation is concluded, the remaining gas is meaſured in the ſame tube with the uſual precautions that the changes of temperature and preſſure of the atmoſphere require.

The diminution obtained by phoſphorus is found to be conſiderably leſs than that obtained by the ſulphuret; but the proportions are always the ſame, becauſe the phoſphorus being diſſolved in azote, as I have proved, takes the gaſeous form, as do all ſubſtances which are diſſolved in a gas. The volume of azote is thus augmented, and ſeveral experiments have ſhewn that this increaſe is about a fortieth part.

The difference that appears in the diminution between the phoſphorus and the ſulphuret cannot be cauſed by the one ſeparating the oxigen leſs exactly than the other; for the phoſphorus acts ſo ſtrongly on oxigen when it is diſſolved in azote, that it is ſufficient to paſs the phoſphorated azote through water to render it luminous, and make it burn with the oxigen it meets.

It appears to me that ſulphurated hydrogen, which is ſoluble in the azote, is capable of precipitating moſt of the phoſphorus. For if the phoſphorated azote be placed over the ſulphuret of alkali, its bulk is diminiſhed; but the total diminution is not ſo great as if the air had been firſt placed on the ſulphuret. The phoſphorated azote, which has been thus diminiſhed by ſulphuret, is no longer capable of being rendered luminous by coming in contact with oxigen gas.

If phoſphorus be introduced into the azote which is in contact with the ſulphuret, it produces no ſenſible effect; but if the azote be waſhed by being paſſed through clear water, it will be ſufficient to render the water immediately luminous, ſo that the water retains the greateſt part of the ſulphurated hydrogen, and gives a very ſmall quantity of oxigen to the azote gas, which by this operation is made capable of diſſolving phoſphorus, and of acting on the ſmalleſt quantity of oxigen gas.

I have obſerved, that the azote which has been expoſed with the ſulphuret, does not apparently loſe or acquire any thing in its bulk, when it has been paſſed through the water, and the combuſtion that takes place when phoſphorus is introduced is ſo ſmall, that it can ſcarcely be appreciated.

I, therefore, conclude, from all the preceding facts, that the method with phoſphorus is at the ſame time the moſt convenient and the moſt accurate, and that the time is not inconvenient;

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* Theſe experiments were repeated at a temperature from ſix to ten degrees of the thermometer centigrade, and it only required ſix or eight hours for the operation to be concluded. M. Humboldt mentions experiments which has laſted ſeveral days, after which the gas ſtill reddened with the nitrous gas. He muſt have tried very different methods to have produced ſo great a difference in the reſults.

inconvenient; that it gives the moſt poſitive reſults with regard to comparing the airs, and requires only one correction of a fortieth part of the volume of the remaining gas, when the proportion of the oxigen gas is to be determined with the ſame accuracy as may be obtained by the ſulphuret, which appears to me to be the moſt exact approximation that can be procured by any means known *.

Several experiments made by Citizen Champy and myſelf in the laboratory of the Inſtitute of Egypt, with the ſulphuret of alkali and phoſphorus, in which we applied the corrections ſuited to the changes of temperature, as well as for the expanſion of the azote by the phoſphorus, have proved, that the proportion of oxigen gas in atmoſpheric air in that country was at leaſt twenty two parts out of an hundred. We have not found in a great number of experiments any difference exceeding one hundredth part; and it is more natural to attribute this ſmall variation to the inevitable imperfections attendant on all chemical proceſſes, than to any real change in the ſtate of the atmoſphere.

Multiplied experiments I have made at Paris by the ſame methods, appear to me to prove, that the proportion of oxigen is there very nearly the ſame as in Egypt. I muſt, however, remark, that I have not ſo accurate a recollection of the reſults, as to affirm a perfect equality. It is, therefore, neceſſary to repeat theſe experiments at Paris, in order to eſtabliſh the compariſon beyond all doubt †.

Several celebrated chemiſts and natural philoſophers give the oxigen gas a conſiderably higher proportion than that which I have determined; they pretend that they have found conſiderable variations in the air of different places and at different times. Humboldt has recently made the proportions of oxigen gas to vary from twenty-three to twenty-nine centimes. But I have not perceived theſe variations at the diſtance of Cairo from Paris,

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* Mr. Humboldt pretends, that he has formed a ternary combination of phoſphurus azote and oxigen (Ann. de Chim. 30 Thermidor, An. 6.) His opinion is founded on ſome experiments, in which he has obſerved that the phoſphorus produces unequal diminutions in the atmoſpheric air, expoſed to its action in tubes of equal ſize, and under the ſame circumſtances. The difference between the two tubes was from 115 to 156. It is on this alone that he eſtabliſhes the exiſtence of oxided phoſphuret of azote. I have ſeen no appearance of this difference in the numerous experiments I have made at Paris and at Cairo at two different periods.

† The experiments made at Paris gave about one two-hundreth part more of reſidue. They coincide perfectly with thoſe which I had formerly made. It appears to me likely that this ſmall difference ariſes from the air at Cairo being very dry. At the time when I operated, the air, probably, became ſaturated with water, and acquired by that means a ſlight dilation rather than a difference between the conſtituent parts of the two atmoſpheres. But the reſults prove that the proportion of oxigen gas might be determined at a little above twenty-two parts out of an hundred.

Experience will eaſily decide between the opinion of Mr. Humboldt and mine. It is only required to examine, whether it be true that phoſphorus and the liquid ſulphuret produce a uniform and conſtant reſult, as I affirm. I do not ſpeak of the vivid combuſtion of phoſphorus, with regard to which there are other conſiderations to be made. When the experiment of the ſlow action of the phoſphorus is made, the cylinder of phoſphorus ought to occupy a large part of the height of the tube in which the experiment is made, in order that all the atmoſpheric air may be ſubjected to its action.

at very remote periods, and in very different climates. This difference may be attributed to the nitrous gas, which is uſually made uſe of. It is allowed, that experiments of this kind cannot be compared, if any alteration happens in the water made uſe of; or in the nitrous gas; or in the dimenſions of the tube; or in the manipulations. Corrections founded on ſuppoſitions, very far from being juſtified by experiment, are very ſedulouſly applied, and regular methods are rejected, to which no exception has ever been made but what has been overthrown by actual obſervation. Perhaps they are found too ſimple in Europe.

And, indeed, how can it be imagined that the atmoſphere, which is continually agitated by motions that ſuddenly remove its parts, and which renew and alter its ſituations, can vary conſiderably between one village and another. There is, however, an exception to be made for thoſe places which are elevated above the level of the ſea. The difference of ſpecific gravity between oxigen and azote gas, which in the elaſtic ſtate exert a very weak mutual action, explain that difference which has been found in their proportions.

VI.

Analyſis of a new Variety of Lead Ore. By RICHARD CHENEVIX, *Esq. Communicated by the Author.*

THE mutual elucidation which kindred ſciences receive from each other, muſt ever increaſe as the means of inveſtigation become more accurate and extenſive, and in no inſtance can this mutual connection and improvement be more ſtrikingly diſplayed, than when the influence of newly diſcovered facts extends itſelf through the whole dominion of natural knowledge.

When Sir Iſaac Newton, by comparing the refracting power of water with that of other bodies, made the bold inference that it conſiſted partly of inflammable matter, and that the diamond poſſeſſing that power in a ſtill higher degree, muſt be ranked among combuſtible ſubſtances, it was little ſuſpected that chemiſtry would prove this not to be the vague doubt of an imagination miſled by a favourite hypotheſis, but the firm aſſertion of genius guided by ſtrong analogical conviction. The progreſs made ſince that period affords numerous examples of ſuch happy coincidences; and it cannot fail to be highly ſatisfactory, when two ſciences, tending to the ſame point by different means, concur in affording ſimilar reſults. The Abbé Hauy, on compariſon of the cryſtalline form of the emerald and the beril, after Monſ. Vauquelin had juſt diſcovered his new earth in the latter, did not heſitate to pronounce his ſtrong ſuſpicion of its being a conſtituent part of both; and analyſis proved the validity of his opinion. I am happy that I have it in my power to furniſh another example of the accuracy of thoſe principles, to which he has ſo particularly devoted his attention; and to call in the aid of chemiſtry to ſhew with what certitude the penetrating eye of the able cryſtallographer can, from external ſtructure, arrive at an intimate knowledge of pure mineralogical ſubſtances.

In the valuable collection of the Right Honourable Charles Greville, there are many different varieties of carbonated lead, acknowledged and claſſed as ſuch from their external characters: one, however, which, as far as I can learn, does not exiſt elſewhere, excited ſome doubt in the mind of Monſ. le Comte de Bournon, from its aſſuming a peculiar modification of form, not hitherto remarked. He offered it to me for chemical examination; obſerving at the ſame time, that though thoroughly convinced it was carbonated lead, he was equally ſure it muſt be in ſome particular ſtate of combination, either in the proportion of its known principles, or by containing ſome ſubſtance not found in the other varieties. I undertook the analyſis, and to render the approximation more ſtriking, he has favoured me with the following accurate mineralogical deſcription.

Mineralogical Characters, in which it chiefly differs from ſimple Carbonated Lead, by Monſ. le Comte de Bournon.

This variety of lead ore differs from ſimple carbonated lead.

1. It is much ſofter than carbonated lead, making no impreſſion whatever on it, and being eaſily ſcratched and worn by rubbing againſt it.

2. Its ſpecific gravity is much leſs, being 60.651. Many experiments on pure cryſtals of carbonated lead give a ſpecific gravity of 72.357. Many authors fix it at 44,63, but they have undoubtedly taken the heavy ſpar for it.

3. Its fracture is glaſſy, and much like that of the precious ſtones.

4. Its form is different in many reſpects. Like moſt other natural oxides of lead, its lamellæ appear to be rectangular; but their union, which very ſeldom forms a perfect cube in the carbonated lead, produces that form more frequently in this ore. The various laws of decreaſe, which theſe lamellæ undergo, in order to produce that cube (which is very frequently lengthened, or flattened) as well as the different cryſtalline modifications, are alſo very different.

Sometimes the decreaſe is at the angles, ſo as to replace thoſe of the cube or parallelipipedon by planes of greater or leſs extent, and perpendicular to the axis of the cube. Sometimes, if we conſider the cube or parallelipipedon as a priſm, we ſhall find that the longitudinal edges of the priſm are replaced by planes perpendicular to thoſe edges.

Very frequently, the cryſtal being ſtill conſidered as a priſm, the edges of the terminating faces are replaced by planes inclined to the faces of the priſm, ſo as to form an angle of 148°, and another angle of 122°, with the terminating faces. The cryſtallization under that form has ſometimes been of ſufficient duration, to allow at the two extremities of the cryſtal a tetraedal pyramid, ſeldom complete, but which ſometimes preſerves no marks of terminating faces, but a ſmall plane replacing the ſummit of the pyramid; when complete, the oppoſite faces join at the ſummit in an angle of 64°.

At other times, the cryſtal having undergone the preceding modification, ſo as to leave a great part of the cubical faces, is ſubject to another, which replaces each of the longitudinal priſmatic edges by two planes, inclined upon the priſmatic faces, with which they form an angle of 161° 30′, then a tetraedral pyramid, more or leſs truncated at the ſummit, ſeems placed on each of the longitudinal faces (the cryſtals ſtill conſidered as priſmatic) the faces of which, taken two by two, are differently inclined; two of them tend to meet at the ſummit in an angle of 143°, the two others in an angle of 116°. Such are the different modifications of this ore, to which there is nothing ſimilar among the ſimple carbonated leads.

All theſe varieties are extremely beautiful and perfect in their cryſtallization; they are generally of a light ſtraw colour, though ſometimes of a perfectly colourleſs and clear tranſparent white, with a luſtre far more brilliant than the ſimple carbonated leads.

Its topographical hiſtory I ſhall tranſcribe in the words of Mr. Greville. "About "fourteen or fifteen years ago, I purchaſed a piece of this mineral at Matlock, in Derby-"ſhire; the ſhopkeeper not knowing from what mine it came, or unwilling to inform me, "I left a commiſſion for him to ſend me all he could procure, and repeated my order for "ſeveral years ſucceſſively: I traced ſome ſpecimens, which had been ſold, and recovered "them. The largeſt cryſtal, Dr. Darwin, of Derby, gave up to me. I ſuppoſed, from "my enquiries, that it came from the diſtrict of mines intended to be unwatered by the "Cromford level, as I was aſſured of my having no chance of any more till that level was "compleated; and ſince that period the water has continued in a degree, that many "mines which were then open, are now drowned."

The cryſtals are upon a large facetted galena, and commonly from half an inch to an inch in length. The largeſt mentioned in Mr. Greville's note, is an inch and a half long, an inch broad, and half an inch thick, of the fineſt tranſparent yellow. This ore, however, does not appear to be an excluſive production of this country; for there are two ſpecimens in Mr. Greville's collection, which are in much ſmaller and perfectly white cryſtals, which ſeem perfectly the ſame; but they are not of this country, and, as I believe, come from the Hartz; no doubt it will be found elſewhere, when the attention of naturaliſts ſhall have been more particularly directed to that object.

ANALYSIS.

One hundred parts, choſen from a cryſtal perfectly tranſparent, regular, and pure, were reduced to powder; during this operation they frequently caked, and offered an elaſtic reſiſtance, not unlike muriate of ammonia in the ſame circumſtances; they were thrown into a known weight of nitrous acid; a ſudden efferveſcence took place, and the diminution was = 00,6, and the elaſtic fluid was afterwards proved to be carbonic acid. As ſoon as the efferveſcence had ſubſided, there remained at bottom a white powder, inſoluble without the aſſiſtance of heat; but after a ſlight ebullition, this reſiduum was reduced to 00,1, which was ſulphate of barytes, and quite foreign to the intimate nature of the ore. Into the diſſolution was poured cauſtic ammonia, in preference to a fixed alkali, becauſe the oxide of lead is ſoluble in this laſt. A precipitate fell down, and the remaining liquor, filtered and neutralized, was ſubmitted to the following teſts:

Nitrate of lime afforded no precipitate: the preſence, therefore, of the phoſphoric, arſenic, or molybdic acids, was entirely precluded.

Nitrate of barytes afforded no precipitate, therefore no ſulphuric acid was preſent.

Nitrate of lead, a ſlight precipitate, but quickly ſoluble by the addition of water. Theſe appearances convinced me that none of thoſe acids, hitherto found to mineralize lead ores were to be expected in the ſpecimen now to be examined; and the properties of the precipitate cauſed by nitrate of lead, agreeing nearly with thoſe of muriate of lead, I dropped in ſome nitrate of ſilver: a very thick and abundant precipitate took place, which had all the characteriſtics *, not to be miſtaken, of muriate of ſilver. It weighed 42, which, according to Bergman, contain 7 of muriatic acid; but according to Klaproth, in his analyſis of the molybdate of lead, 7 of muriatic acid ſaturate about 45 of oxide of lead; and again, according to Bergman, 6 of carbonic acid ſaturate 34 of oxide of lead, which would have left a deficit of 8. The experiment was, therefore, repeated; and being aware that ammonia could no more decompoſe the whole of the muriate of lead, than could any other alkali, the nitrate of ſilver was dropped immediately into the nitric diſſolution of the ore; and upon repeating this eſſay twice, 48 of muriate of ſilver were obtained each time, containing 8 of muriatic acid; which in combination form 59 of muriate of lead; the lead afterwards ſubmitted to various proofs, was found to be perfectly pure, and the proportions may be eſtabliſhed as follows:

Oxide of lead - - -	51	Muriate of lead - -	59
Muriatic acid - - -	8		
Oxide of lead - - -	34	Carbonate of lead -	40
Carbonic acid - - -	6		
	99		99

With about one of heavy ſpar, as before mentioned.

It does not, however, appear probable, that theſe two ſalts of lead exiſt ſeparately in the ore; its perfect cryſtallization and tranſparency ſufficiently indicate it to be in the ſtate of a triple ſalt, or carbonated muriate of lead, combined on the above proportions, and in the following order:

Muriatic and	acids	8	Oxide of lead 85
Carbonic		6	
		14	99

The very great ſcarcity of this ore prevented me from ſubmitting it to more experiments; however, upon expoſing a certain quantity of it to a red heat, a thick vapour aroſe,

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* I need mention no other than this; muriate is the only ſalt of ſilver which is not ſoluble in almoſt every acid, and is inconteſtibly the moſt delicate and abſolute re-agent in chemiſtry; as well to demonſtrate the preſence, as to determine the quantity either of ſilver or muriatic acid.

which manifeſted no ſmell of any kind, and could not, therefore, be ſuſpected to contain either arſenic or antimony: I ſuppoſe it to be muriate of lead, as notwithſtanding its great fuſibility, and its ſuppoſed fixity, it is, in fact, in ſome degree volatile, as I have repeatedly obſerved on other occaſions.

No more than twenty-two parts of water are neceſſary to diſſolve one of muriate of lead, and the diſſolving power is much encreaſed by the preſence of an acid, yet we ſee in the caſe before us it could reſiſt the action of a much greater quantity, even when aided by a conſiderable portion of nitric acid in exceſs; ſuch examples are not rare in natural ſubſtances, compared with the ſlight aggregation of thoſe which art can produce.

The exiſtence of a natural muriate of lead has been ſuſpected more than once. Ferber examined a ſpecimen found at Meis and Bleyſtadt, in Bohemia, which he imagined to contain muriatic acid; but the Baron de Born aſſures, that Klaproth, upon an accurate analyſis, had determined thoſe ſame grey hexaedral cryſtals, which Ferber had procured, to be merely phoſphate of lead, a variety then well known. Monſ. Sage had likewiſe aſſerted as much; but the ſpecimens which he had tried, being further ſubmitted to inveſtigation by Mr. Laborie, and by ſome members of the Royal Academy of Paris, were found to be totally void of muriatic acid, and are now univerſally acknowledged as pure carbonated leads.

The authorities here mentioned are too reſpectable for me to diſcuſs their merits. I ſhall only add, that after the experiments above deſcribed, and which were performed in the preſence of Monſ. de Bournon, he no longer refuſes his aſſent to this new ſpecies of lead ore, now firſt proved, if not diſcovered, but has added it to thoſe varieties already admitted in the ſyſtem of mineralogy.

VII.

Experiments on the Chemical Effects of Galvanic Electricity. By Mr. WILLIAM HENRY.

To Mr. NICHOLSON.

SIR,

Mancheſter, July 20, 1800.

IN addition to the intereſting facts, reſpecting the chemical action of galvaniſm, which appeared in the laſt Number of your Journal, I beg leave to communicate the following experiments. The apparatus, which I employed, needs no further deſcription than that it conſiſted of half crowns and ſimilar pieces of zinc, varying in number as occaſion required, with pieces of woollen cloth interpoſed, ſoaked in a ſaturated ſolution of common ſalt. I tried muriate of lime, as a ſubſtitute for common ſalt, but without any better effect.

1. To the hiſtory of the phenomena, attending the decompoſition of water by this new influence, which have been already accurately deſcribed by yourſelf, and by Meſſrs. Carliſle and Cruickſhank, I have little to add. If the water be confined by mercury, in a tube with a conductor ſealed hermetically into one end, a production of gas only takes place, when the conductor communicates with the ſilver ſide of the apparatus; and if the order be inverted, *i. e.* if the mercury be connected with the ſilver end, and the conductor at the ſealed extremity of the tube with the zinc end, of the pile, no gas whatever is produced, though the agitation of the ſurface of the mercury in the tube ſhews that the influence is ſtill tranſmitted. On paſſing up a wire of any metal through the quickſilver, ſo that its extremity may riſe into the water, gas then begins to aſcend copiouſly from the lower wire. Does not this ſhew, that pointed bodies are more effectual in decompoſing water than bodies with rounded ſurfaces?

It may be worth while to obſerve, that the change of the colour of litmus from blue to red, occurs in diſtilled water, even after long boiling. From Dr. Pearſon's experiments*, however, it appears that air is ſeparated by electric ſhocks from water, which has been expoſed under a receiver, exhauſted by a powerful air pump.

2. Concentrated ſulphuric acid was ſubmitted to this influence, in a glaſs tube, furniſhed with two platina conductors, the open end of the tube being immerſed in a cupful of the ſame acid. Gas was produced in great plenty, one half of which was abſorbed by ſulphuret of potaſh. The remainder was hydrogenous gas, from the decompoſition of water, which even the ſtrongeſt ſulphuric acid neceſſarily contains. But as the oxygenous gas was ſufficient to have ſaturated twice the quantity of hydrogen gas evolved, one half of the former muſt have had another origin than water, and may he aſcribed to the decompoſition of the acid. Indeed, during the paſſage of the electricity, a white cloud was obſerved to ſurround the wire from which the gas aſcended, and which was probably diſoxygenated ſulphur.

3. Perfectly pure and colourleſs nitric acid was rapidly decompoſed in a ſimilar apparatus. The acid aſſumed a ſtraw colour, and gas was obtained, conſiſting of oxygenous and azotic gaſes, in the proportion of 530 of the former to 151 of the latter.

4. From liquid muriatic acid, 424 parts of gas were evolved, of which 144 were oxygenous, and 280 hydrogenous. Theſe gaſes had, doubtleſs, their origin from the decompoſition of water. The quantity of the acid being conſiderable, in proportion to the gaſes produced, it did not exhibit the marks of having become oxygenated during the experiment.

5. A tube was prepared with platina conductors, covered with wax, and being filled with a ſaturated ſolution of oxygenated muriatic acid in water, the circuit was completed through it, between the two ends of the pile. The gas evolved was a mixture of oxygenous

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* Phil. Tranſ. 1797, or this Journal, vol. I.

and hydrogenous, in the proportion of 136 to 118. Now the 118 parts of hydrogen gas require 59 by meaſure of oxygen gas for ſaturation. The remaining 77 parts of oxygen gas aroſe from the de-oxygenation of the acid.

6. As no certain concluſions reſpecting the compoſition of the muriatic acid can be drawn from experiments, inſtituted on it when condenſed by water, I was eager to try the effects of this new and powerful agent on the acid in a gaſeous form. But it was neceſſary to aſcertain previouſly, whether the galvanic influence be capable of paſſing through aëriform bodies. An argument to the contrary was furniſhed by the inefficiency of this influence in inflaming mixtures of oxygen and hydrogen gaſes; for when the gaſes evolved from decompoſed water, have diſplaced the whole of the liquid above the extremity of the upper wire, the evolved gaſes do not take fire, as they do in the experiment of Dr. Deiman. Another fact, ſhewing the non-tranſmiſſion of this influence through air, is that when a little air is let up into an inverted tube filled with water, and furniſhed with two conductors, ſo as to diſplace the water a little below the extremity of the upper wire, no gas aſcends, though the lower wire be connected with the ſilver baſe of the pile. I found, alſo, that a diviſion in a piece of tin-foil paſted on glaſs, even when ſo ſmall as not to be diſcerned without a magnifier, interrupted the paſſage of the galvanic fluid. But this non-conducting property might poſſibly be peculiar to common air, and it was deſirable, therefore, that the tranſmitting power of other gaſes ſhould be aſcertained.

One of the moſt delicate teſts I know of the effect of electricity on gaſes is furniſhed by the phoſphorated hydrogen gas, which, by common electrical ſhocks, or even ſparks, is expanded with great rapidity. I expoſed, therefore, a portion of this gas over mercury, in a tube furniſhed at one end with a platina conductor, which was connected with the ſilver end of the pile, while the mercury that confined the gas was connected with the zinc end. But after ſtanding ſeveral hours, no change had enſued. In a ſimilar manner I expoſed a mixture of muriatic acid and oxygenous gaſes, which common electrical diſcharges rapidly diminiſh, without any contraction of bulk, or change of properties *. Neither was any permanent gas evolved by a ſimilar treatment of muriatic acid gas alone. The deficiency of the property of tranſmiſſion through gaſes limits conſiderably the uſe of galvaniſm as a chemical agent, and has totally overturned my project of attempting, by its intervention, the analyſis of muriatic acid.

7. Finding it impracticable to tranſmit the influence through ammoniacal gas, I expoſed to its action a portion of water perfectly ſaturated with this alcali. The reſult, which has been confirmed by frequent repetitions, ſurprized me not a little. No oxygenous gas was produced; for the evolved gas was not diminiſhed by ſulphuret of potaſh, neither did it inflame on paſſing through it an electric ſpark. On firing it with oxygen gas,

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* I was miſled at firſt to believe that a contraction had taken place, becauſe the ſealing wax, with which the platina wires were covered, had abſorbed a portion of muriatic acid gas.

gas, it was diminiſhed very greatly, and a ſolution of ſulphuret of potaſh, after having abſtracted the exceſs of oxygen gas, left only a ſmall bubble of azotic gas, which may be traced to the oxygen gas employed to effect the combuſtion.

8. A ſolution of cauſtic vegetable alcali, being expoſed in a ſimilar manner, the gas produced was pure hydrogen gas. During the proceſs, the ſurface of the mercury became covered with a blackiſh film, eſpecially at its edges where it was in contact with the glaſs tube. The black matter, however, was confined to the ſurface of the mercury, and was not generated at the ſame part of the tube at which gas was produced, viz. in contact with the platina wire. It ſtill occurred even on uſing mercury which had been carefully diſtilled for the purpoſe.

The two laſt facts are the moſt curious that have occurred in the courſe of my experiments. In experiment 7, the volatile alcali was certainly decompoſed, for had the hydrogen gas proceeded from the decompoſition of water only, oxygenous gas would alſo have been obtained. Into what new combination does the azote, in this caſe, enter? It is not improbable that at the ſame inſtant both water and ammoniac are decompoſed; that the hydrogen of both is converted into gas; and that the oxygen of the water, uniting with the azote of the alcali, compoſes nitric acid, which, combining with the ammoniac, produces nitrate of ammonia. The deſtruction of the vegetable alcali is not leſs certain. From the 8th experiment we derive a ſufficient proof that it contains hydrogen. Azote is probably another of its conſtituents, but is prevented from appearing in the gaſeous ſtate by its union with oxygen, evolved, at the ſame inſtant, by the decompoſition of a portion of water. Theſe ſuggeſtions admit of being verified by a careful examination of the alcalis, after expoſure to the influence of galvaniſm; and a third component of the vegetable alcali will perhaps be found in the black precipitate above deſcribed. I have not been inattentive, even hitherto, to theſe points; but the minuteneſs of the quantities ſubmitted to experiment prevents me from ſpeaking deciſively, and I chooſe rather to reſerve myſelf for the reſults of experiments now going on, than to incur the riſk of being forced to retract a haſty aſſertion. In the mean time, I ſend you theſe particulars of unfiniſhed experiments, becauſe thoſe who are practically engaged in inveſtigating the properties of galvaniſm may derive, from a communication with each other through the medium of your Journal, ſome of the advantages of perſonal intercourſe and cooperation.

I am reſpectfully,
SIR,
Your obedient
humble ſervant,

WILLIAM HENRY.

Deſcription

VIII.

Deſcription of a new Inſtrument for repeating the Examination of the Strata which have been penetrated by the common boring Inſtrument uſed in Mine Works. By A. Baillet, Inſpector of Mines, and Profeſſor at the Mineral School at Paris [*].

AFTER remarking the great utility of boring, in order to aſcertain the exiſtence of mineral ſubſtances, particularly coal at great depths, the author obſerves, that it would be of great advantage if this operation could be eaſily repeated or verified. "It often "happens," ſays he, "that ſome doubts remain concerning the nature, the thickneſs, or "the poſition, of the principal mineral ſtrata through which the borer has paſſed. This "uncertainty increaſes when there is reaſon to ſuſpect the integrity of the perſons em-"ployed; in which caſe there is but one method of removing the uncertainty, viz. to bore "the ſame excavation a ſecond time with a tool of a larger diameter, and endeavour to "confirm the reſults of the firſt operation by obſerving thoſe of the ſecond." The ſlowneſs, the expence, and the other inconveniences of this method, may eaſily be imagined. To avoid theſe the author propoſes the uſe of an inſtrument, which he calls a verifier (verificateur), which is of uſe to take at any required depth in the hole or rectiform, a ſample of the ground which exiſts at that ſpot. This inſtrument, repreſented at fig. 1, plate 9, is compoſed of two principal pieces; a ſuperior piece *a b*, in which there is a cylindrical cavity opening below; and another inferior piece *c d*, inſerted in this cavity, and terminating in a cone.

Theſe two pieces are connected together by means of two keys or pins *e e*, which paſs through the upper piece, and occupy a circular groove *e f* in the lower piece.

The upper piece contains two knives *g g*, let into its thickneſs, and fixed at their upper extremity by a ſcrew *h*. A gutter or groove *j*, of an helical form, is made from the lower part of each blade, for the purpoſe of conveying the fragments and powder ſcraped off by theſe knives into the cup or cavity of the lower piece *k*.

When the apparatus is addreſſed, the backs of the two knives touch each other at *l*, and the point of the cone *m* lies in the angle formed by the ſlopes of the two knives.

The extremity *s* of the inſtrument terminates in a ſcrew, which is not repreſented in the preſent drawing, and the other extremity *r* likewiſe terminates in a ſcrew; ſo that by means of theſe the inſtrument may be lodged in any part of an hole already bored. That is to ſay, a rod of any required length may be fixed below it by means of the ſcrew *s*, and the other rods neceſſary to lower it down, may be ſcrewed on above.

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[*] Communicated to the Philomathic Society, and inſerted in their Bulletin, No. 39, in the 8th republican year.

The author remarks, 1, that the two pieces of the inſtrument being connected together, it can eaſily be let down into any hole, and drawn up again; 2, that if the lower piece reſts on a fixed ſupport, the upper piece is capable of deſcending through a quantity determined by the ſpace left in the excavation, which receives the two keys in *e e*; and that during this deſcent, the knives will be forced aſunder by the cone of the lower piece forcing itſelf between their backs; 3, that the upper piece is capable of turning upon the lower, in which caſe the knives will cut away the ground, and the fragments will fall into the groove, and be conducted into the cup beneath; 4, laſtly, that if the inſtrument be drawn up again, the two knives will retire into their cells, either by the effect of their own elaſticity, or by that of a ſpring properly placed for that purpoſe, or by the re-action of the ground itſelf, which preſſes them as they riſe.

The author afterwards enters more at large into the uſes of his inſtrument, which the reporter of the ſociety conciſely ſtates as being effected by adapting to the inſtrument a ſeries of rods ſucceſſively lowered into the cavity, and of which the length is ſuch that the knives of the inſtrument ſhall be found at the exact height of the ſtratum intended to be examined when the loweſt rod touches the bottom. Other rods are of courſe to be added above, in order to conduct and manage the inſtrument; in this ſituation of things nothing more is required than to turn the apparatus in the ſame manner as a borer, and when it is preſumed that the knives have detached a ſufficient quantity, the inſtrument is drawn up.

The author terminates his memoir by ſeveral eſſential obſervations. The firſt relates to the neceſſity of not leaving the upper rods to the action of their own weight, which would produce the great inconvenience of ſeparating the knives too much at firſt, and endangering the upper rods by too great reſiſtance. The rods may eaſily be lowered properly, and by degrees, by employing the frame and ſcrew *a*, fig. 3, through which the upper rod paſſes.

The ſecond obſervation relates to the different methods of adjuſting the verifier to the preciſe height where the action is intended to take place. For this purpoſe a rod of the proper length may be forged, in order that it may be fixed to one or more of the common rods; or otherwiſe the operator may provide himſelf with two rods of one decimetre, one of two decimetres, and another of five decimetres, which ſyſtem will give all the lengths from decimetre to decimetre, from 1 to 9, &c.

The third and laſt obſervation relates to the depth of the circular cut which the two knives will make. Under like circumſtances a cavity of twice the dimenſions may be made with one knife inſtead of two. The ſingle knife muſt have the form repreſented in fig. 2.

IX.

General Principles and Conſtruction of a Sub-marine Veſſel, communicated by D. BUSHNELL, *of Connecticut, the Inventor, in a Letter of October, 1787, to Thomas Jefferſon, then Miniſter Plenipotentiary of the United States at Paris.*[*]

THE external ſhape of the ſub-marine veſſel bore ſome reſemblance to two upper tortoiſe ſhells of equal ſize, joined together; the place of entrance into the veſſel being repreſented by the opening made by the ſwell of the ſhells, at the head of the animal. The inſide was capable of containing the operator, and air, ſufficient to ſupport him thirty minutes without receiving freſh air. At the bottom oppoſite to the entrance was fixed a quantity of lead for ballaſt. At one edge which was directly before the operator, who ſat upright, was an oar for rowing forward or backward. At the other edge, was a rudder for ſteering. An aperture, at the bottom, with its valve, was deſigned to admit water, for the purpoſe of deſcending; and two braſs forcing pumps ſerved to eject the water within, when neceſſary for aſcending. At the top, there was likewiſe an oar for aſcending or deſcending, or continuing at any particular depth—A water gauge or barometer, determined the depth of deſcent, a compaſs directed the courſe, and a ventilator within, ſupplied the veſſel with freſh air, when on the ſurface.

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[*] Tranſactions of the American Philoſophical Society. IV. 303.

This is the only modern inſtance I am acquainted with of the purſuit of ſub-marine navigation. The ſubject was largely and pleaſantly deſcanted upon by Merſennus in his *Tractatus de Magnetis Proprietatibus,* and Biſhop Wilkins has given a chapter at ſome length on the ſame ſubject, in his *Mathematical Magick* (ed. 1648) where he affirms, that Cornelius Dreble had proved, beyond all queſtion, that the contrivance is feaſible by the experiments he made in England. The chapter of Wilkins is entertaining for a ſort of viſionary facility with which he removes the difficulties and enumerates the benefits of theſe ſub-marine enterprizes. For letting out and taking in ſuch things as the nature of the voyage may require, he recommends bags, or flexible tubes, ſomewhat reſembling the ſcupper bags of ſhips. The progreſſive motion may, he obſerves, be produced by fins or oars, which will operate with eaſe when the veſſel is truly equipoiſed, and if ſwiftneſs ſhould not be obtained, he ſuppoſes, the obſervations and diſcoveries to be made at the bottom of the ſea would abundantly recompence for that defect. The greateſt difficulty, in his apprehenſion, would be in the neceſſity of renovating the air for reſpiration and combuſtion; for remedying which, beſides the probability that cuſtom may render men capable of living in air of inferior purity, he has ſeveral philoſophical views and projects. The conveniencies and advantages he enumerates, are, 1. Privacy; as a man may thus go to any part of the world inviſibly, without being diſcovered or prevented. 2. Safety; from the uncertainty of tides and tempeſts which vex the ſurface; from pirates and robbers; and from the ices which ſo much endanger other voyages towards the poles. 3. It may be of uſe to undermine and blow up a navy of enemies: 4. Or to relieve a blockaded place: 5. And as the proſpect enlarges in the mind of our author, he proceeds to contemplate the unſpeakable benefit of ſub-marine diſcoveries. Experiments on the aſcent and deſcent of ſubmerged bodies; the exploration of the deep caverns and paſſages of the waters of the ocean; obſervations on the nature and kinds of fiſhes, with the allurements, artifices, and treacheries, which may ſucceſsfully be practiced during ſo familiar a reſidence in their territories; the food and oil they may afford; the probability of freſh ſprings for a ſupply of water at the bottom of the ſea; the facility of recovering ſub-marine treaſures whether loſt or naturally produced beneath the ocean; and laſt of all he adds, that

> “All kinds of arts and manufactures may be exerciſed in this veſſel. The obſervations made by it may be both written, and (if need were) printed here likewiſe. Several colonies may thus inhabit, having their children born and bred up without the knowledge of land, *who could not chuſe but be amazed with ſtrange conceits upon the diſcovery of this upper world.*”

The entrance into the veſſel was elliptical, and ſo ſmall as barely to admit a perſon. This entrance was ſurrounded with a broad elliptical iron band, the lower edge of which was let into the wood of which the body of the veſſel was made, in ſuch a manner, as to give its utmoſt ſupport to the body of the veſſel againſt the preſſure of the water. Above the upper edge of this iron band, there was a braſs crown, or cover, reſembling a hat with its crown and brim, which ſhut water tight upon the iron band: the crown was hung to the iron band with hinges ſo as to turn over ſidewiſe, when opened. To make it perfectly ſecure when ſhut, it might be ſcrewed down upon the band by the operator, or by a perſon without.

There were in the braſs crown, three round doors, one directly in front, and one on each ſide, large enough to put the hand through—when open they admitted freſh air; their ſhutters were ground perfectly tight into their places with emery, hung with hinges and ſecured in their places when ſhut. There were likewiſe ſeveral ſmall glaſs windows in the crown, for looking through, and for admitting light in the day time, with covers to ſecure them. There were two air pipes in the crown. A ventilator within drew freſh air through one of the air pipes, and diſcharged it into the lower part of the veſſel; the freſh air introduced by the ventilator, expelled the impure light air through the other air pipe. Both air pipes were ſo conſtructed, that they ſhut themſelves whenever the water roſe near their tops, ſo that no water could enter through them, and opened themſelves immediately after they roſe above the water.

The veſſel was chiefly ballaſted with lead fixed to its bottom; when this was not ſufficient, a quantity was placed within, more or leſs, according to the weight of the operator; its ballaſt made it ſo ſtiff, that there was no danger of overſetting. The veſſel with all its appendages, and the operator, was of ſufficient weight to ſettle it very low in the water. About two hundred pounds of the lead, at the bottom, for ballaſt, could be let down forty or fifty feet below the veſſel; this enabled the operator to riſe inſtantly to the ſurface of the water, in caſe of accident.

When the operator would deſcend, he placed his foot upon the top of a braſs valve, depreſſing it, by which he opened a large aperture in the bottom of the veſſel, through which the water entered at his pleaſure; when he had admitted a ſufficient quantity, he deſcended very gradually; if he admitted too much, he ejected as much as was neceſſary to obtain an equilibrium, by the two braſs forcing pumps, which were placed at each hand. Whenever the veſſel leaked, or he would aſcend to the ſurface, he alſo made uſe of theſe forcing pumps. When the ſkilful operator had obtained an equilibrium, he could row upward, or downward, or continue at any particular depth, with an oar, placed near the top of the veſſel, formed upon the principle of the ſcrew, the axis of the oar entering the veſſel; by turning the oar one way he raiſed the veſſel, by turning it the other way he depreſſed it.

A glaſs tube eighteen inches long, and one inch in diameter, ſtanding upright, its upper end cloſed, and its lower end, which was open, ſcrewed into a braſs pipe, through which the external water had a paſſage into the glaſs tube, ſerved as a water-gauge or barometer. There was a piece of cork with phoſphorus on it, put into the water-gauge. When the veſſel deſcended the water roſe in the water-gauge, condenſing the air within, and bearing the cork, with its phoſphorus, on its ſurface. By the light of the phoſphorus, the aſcent of the water in the gauge was rendered viſible, and the depth of the veſſel under water aſcertained by a graduated line.

An oar, formed upon the principle of the ſcrew, was fixed in the forepart of the veſſel; its axis entered the veſſel, and being turned one way, rowed the veſſel forward, but being turned the other way rowed it backward; it was made to be turned by the hand or foot.

A rudder, hung to the hinder part of the veſſel, commanded it with the greateſt eaſe. The rudder was made very elaſtic, and might be uſed for rowing forward. Its tiller was within the veſſel, at the operator's right hand, fixed, at a right angle, on an iron rod, which paſſed through the ſide of the veſſel; the rod had a crank on its outſide end, which commanded the rudder, by means of a rod extending from the end of the crank to a kind of tiller, fixed upon the left hand of the rudder. Raiſing and depreſſing the firſt mentioned tiller turned the rudder as the caſe required

A compaſs marked with phoſphorus directed the courſe, both above and under the water; and a line and lead ſounded the depth when neceſſary.

The internal ſhape of the veſſel, in every poſſible ſection of it, verged towards an ellipſis, as near as the deſign would allow, but every horizontal ſection, although elliptical, yet as near to a circle, as could be admitted. The body of the veſſel was made exceedingly ſtrong; and to ſtrengthen it as much as poſſible, a firm piece of wood was framed, parallel to the conjugate diameter, to prevent the ſides from yielding to the great preſſure of the incumbent water, in a deep immerſion. This piece of wood was alſo a ſeat for the operator.

Every opening was well ſecured. The pumps had two ſets of valves. The aperture at the bottom, for admitting water, was covered with a plate, perforated full of holes to receive the water, and prevent any thing from choaking the paſſage, or ſtopping the valve from ſhutting. The braſs valve might likewiſe be forced into its place with a ſcrew, if neceſſary. The air pipes had a kind of hollow ſphere, fixed round the top of each, to ſecure the air-pipe valves from injury: theſe hollow ſpheres were perforated full of holes for the paſſage of the air through the pipes: within the air-pipes were ſhutters to ſecure them, ſhould any accident happen to the pipes, or the valves on their tops.

Wherever the external apparatus paſſed through the body of the veſſel, the joints were round, and formed by braſs pipes, which were driven into the wood of the veſſel, the holes through the pipes were very exactly made, and the iron rods, which paſſed through them, were turned in a lathe to fit them; the joints were alſo kept full of oil, to prevent ruſt and leaking. Particular attention was given to bring every part, neceſſary for performing the operations, both within and without the veſſel, before the operator, and as conveniently as could be deviſed; ſo that every thing might be found in the dark, except the water-gauge and the compaſs, which were viſible by the light of the phoſphorus, and nothing required the operator to turn to the right hand, or to the left, to perform any thing neceſſary.

No. 2.

Deſcription of a magazine and its appendages, deſigned to be conveyed by the ſub-marine veſſel to the bottom of a ſhip.

In the forepart of the brim of the crown of the ſub-marine veſſel, was a ſocket, and an iron tube, paſſing through the ſocket; the tube ſtood upright, and could ſlide up and down in the ſocket, ſix inches: at the top of the tube, was a wood ſcrew (A) fixed by means of a rod, which paſſed through the tube, and ſcrewed the wood ſcrew faſt upon the top of the tube: by puſhing the wood ſcrew up againſt the bottom of a ſhip, and turning it at the ſame time, it would enter the planks; driving would alſo anſwer the ſame purpoſe; when the wood ſcrew was firmly fixed, it could be caſt off by unſcrewing the rod, which faſtened it upon the top of the tube.

Behind the ſub-marine veſſel, was a place, above the rudder, for carrying a large powder magazine, this was made of two pieces of oak timber, large enough when hollowed out to contain one hundred and fifty pounds of powder, with the apparatus uſed in firing it, and was ſecured in its place by a ſcrew, turned by the operator. A ſtrong piece of rope extended from the magazine to the wood ſcrew (A) above mentioned, and was faſtened to both. When the wood ſcrew was fixed, and to be caſt off from its tube, the magazine was to be caſt off likewiſe by unſcrewing it, leaving it hanging to the wood ſcrew; it was lighter than the water, that it might riſe up againſt the object, to which the wood ſcrew and itſelf were faſtened.

Within the magazine was an apparatus, conſtructed to run any propoſed length of time, under twelve hours; when it had run out its time, it unpinioned a ſtrong lock reſembling a gun lock, which gave fire to the powder. This apparatus was ſo pinioned, that it could not poſſibly move, till, by caſting off the magazine from the veſſel, it was ſet in motion.

The ſkilful operator could ſwim ſo low on the ſurface of the water, as to approach very near a ſhip, in the night, without fear of being diſcovered, and might, if he choſe, approach the ſtem or ſtern above water, with very little danger. He could ſink very quickly, keep at any depth he pleaſed, and row a great diſtance in any direction he deſired, without coming to the ſurface, and when he roſe to the ſurface, he could ſoon obtain a freſh ſupply of air, when, if neceſſary, he might deſcend again, and purſue his courſe.

No. 3.

Experiments made to prove the Nature and Uſe of a Sub-marine Veſſel.

The firſt experiment I made, was with about two ounces of gun-powder, which I exploded 4 feet under water, to prove to ſome of the firſt perſonages in Connecticut, that powder would take fire under water.

The ſecond experiment was made with two pounds of powder, incloſed in a wooden bottle, and fixed under a hogſhead, with a two inch oak plank between the hogſhead and the powder; the hogſhead was loaded with ſtones as deep as it could ſwim; a wooden pipe deſcending through the lower head of the hogſhead, and through the plank, into the powder contained in the bottle, was primed with powder. A match put to the priming, exploded the powder, which produced a very great effect, rending the plank into pieces; demoliſhing the hogſhead; and caſting the ſtones and the ruins of the hogſhead, with a body of water, many feet into the air, to the aſtoniſhment of the ſpectators. This experiment was likewiſe made for the ſatisfaction of the gentlemen above-mentioned.

I afterwards made many experiments of a ſimilar nature, ſome of them with large quantities of powder; they produced very violent exploſions, much more than ſufficient for any purpoſe I had in view.

In the firſt eſſays with the ſub-marine veſſel, I took care to prove its ſtrength to ſuſtain the great preſſure of the incumbent water, when ſunk deep, before I truſted any perſon to deſcend much below the ſurface: and I never ſuffered any perſon to go under water, without having a ſtrong piece of rigging made faſt to it, until I found him well acquainted with the operations neceſſary for his ſafety. After that, I made him deſcend and continue at particular depths, without riſing or ſinking, row by the compaſs, approach a veſſel, go under her, and fix the *wood-ſcrew* mentioned in No. 2, and marked A, into her bottom, &c. until I thought him ſufficiently expert to put my deſign into execution.

I found, agreeably to my expectations, that it required many trials to make a perſon of common ingenuity, a ſkilful operator: the firſt I employed, was very ingenious, and made himſelf maſter of the buſineſs, but was taken ſick in the campaign of 1776, at New-York, before he had an opportunity to make uſe of his ſkill, and never recovered his health ſufficiently, afterwards.

Experiments made with a Sub-marine Veſſel.

After various attempts to find an operator to my wiſh, I ſent one who appeared more expert than the reſt, from New-York, to a 50 gun ſhip lying not far from Governor's Iſland. He went under the ſhip, and attempted to fix the wooden ſcrew into her bottom, but ſtruck, as he ſuppoſes, a bar of iron, which paſſes from the rudder hinge, and is ſpiked under the ſhip's quarter. Had he moved a few inches, which he might have done, without rowing, I have no doubt but he would have found wood where he might have

fixed

fixed the ſcrew; or if the ſhip were ſheathed with copper, he might eaſily have pierced it: but not being well ſkilled in the management of the veſſel, in attempting to move to another place, he loſt the ſhip: after ſeeking her in vain, for ſome time, he rowed ſome diſtance, and roſe to the ſurface of the water, but found day light had advanced ſo far, that he durſt not renew the attempt. He ſays that he could eaſily have faſtened the magazine under the ſtem of the ſhip, above water, as he rowed up to the ſtern, and touched it before he deſcended. Had he faſtened it there, the exploſion of one hundred and fifty pounds of powder, (the quantity contained in the magazine), muſt have been fatal to the ſhip. In his return from the ſhip to New-York, he paſſed near Governor's Iſland, and thought he was diſcovered by the enemy, on the iſland; being in haſte to avoid the danger he feared, he caſt off the magazine, as he imagined it retarded him in the ſwell, which was very conſiderable. After the magazine had been caſt off one hour, the time the internal apparatus was ſet to run, it blew up with great violence.

Afterwards, there were two attempts made in Hudſon's river, above the city, but they effected nothing. One of them was by the afore-mentioned perſon. In going towards the ſhip, he loſt ſight of her, and went a great diſtance beyond her: when he at length found her, the tide ran ſo ſtrong, that as he deſcended under water, for the ſhip's bottom—it ſwept him away. Soon after this, the enemy went up the river, and purſued the boat which had the ſub-marine veſſel on board—and ſunk it with their ſhot. Though I afterwards recovered the veſſel, I found it impoſſible, at that time, to proſecute the deſign any farther. I had been in a bad ſtate of health, from the beginning of my undertaking, and was now very unwell; the ſituation of public affairs was ſuch, that I deſpaired of obtaining the public attention, and the aſſiſtance neceſſary. I was unable to ſupport myſelf, and the perſons I muſt have employed, had I proceeded. Beſides, I found it abſolutely neceſſary, that the operators ſhould acquire more ſkill in the management of the veſſel, before I could expect ſucceſs; which would have taken up ſome time, and made no ſmall additional expence. I therefore gave over the purſuit for that time, and waited for a more favorable opportunity, which never arrived.

Other Experiments made with a Deſign to fire Shipping.

In the year 1777, I made an attempt from a whale-boat, againſt the Cerberus frigate, then lying at anchor between Connecticut river and New London, by drawing a machine againſt her ſide, by means of a line. The machine was loaded with powder, to be exploded by a gun-lock, which was to be unpinioned by an apparatus, to be turned by being brought along ſide of the frigate. This machine fell in with a ſchooner at anchor, aſtern of the frigate, and concealed from my ſight. By ſome means or other, it was fired, and demoliſhed the ſchooner and three men—and blew the only one left alive, overboard, who was taken up very much hurt.

After this, I fixed ſeveral kegs, under water, charged with powder, to explode upon touching any thing, as they floated along with the tide: I ſet them afloat in the Delaware, above the Engliſh ſhipping at Philadelphia, in December, 1777. I was unacquainted with the river, and obliged to depend upon a gentleman very imperfectly acquainted with that part of it, as I afterwards found. We went as near the ſhipping as he durſt venture; I believe the darkneſs of the night greatly deceived him, as it did me. We ſet them adriſt, to fall with the ebb, upon the ſhipping. Had we been within ſixty rods, I believe they muſt have fallen in with them immediately, as I deſigned; but as I afterwards found, they were ſet adrift much too far diſtant, and did not arrive, until after being detained ſome time by froſt, they advanced in the day time, in a diſperſed ſituation, and under great diſadvantages. One of them blew up a boat, with ſeveral perſons in it, who imprudently handled it too freely, and thus gave the Britiſh that alarm, which brought on *the battle of the kegs.*

The above veſſel, magazine, &c. were projected in the year 1771, but not completed, until the year 1775.

D. BUSHNELL.

X.

Queſtion reſpecting the Purification of Copper for alloying Gold. By X. Y.

To Mr. NICHOLSON.

SIR,

Newcaſtle-upon-Tyne, Feb. 18, 1800.

I BEG leave, through the medium of your valuable publication, to requeſt the favour of any of your correſpondents to inform me of the beſt method of obtaining copper in its utmoſt ſtate of purity, and fit for alloying gold without occaſioning that brittleneſs which always ariſes from uſing copper, even of the fineſt kind uſually obtained in commerce. The ductility of the gold may be reſtored by ſubſequent fuſions, but this occaſions conſiderable loſs and diſappointment to the workman, which do not happen when the copper is perfectly refined. I have looked into ſeveral chemical works, but without obtaining the ſatiſfaction I want; nor would I be ſo ſolicitous about this information, could I, at any time, procure copper fit for this purpoſe.

I am, SIR,

Your's, &c.

X. Y.

On

XI.

On the Art of Hat making. Supplementary Letter. By N. L.

To Mr. NICHOLSON.

SIR,

Newcaſtle, Feb. 14, 1800.

I SHALL think myſelf particularly obliged to you if you will allow me, through the medium of your Journal, to correct *a very great error in the arrangement* of the laſt paper of mine you printed, in your Number for May laſt: as it is *placed* at preſent, it may be productive of great miſtakes, and as it will take up little room in your Journal, the inconvenience will not be great. The following is the proper arrangements, ſee parts of pages 74, 75, 76, vol. iii. The journeymen tell me, that the dregs are to hold or fill the body, whilſt a little vitriol cleanſes it of the dirt, &c. that may be on the rabbit or other wool; too much vitriol would make the whole that was weighed out to the journeymen work into the hats, but by the mutual action of the vitriol and the dregs, the quantity of the firſt being ſmall, about a wine glaſsfull, the dirt and the ſtrong hairs get purged out (the laſt from the ſhrinking in being ſlow, as well as their being ſtraight; for was the leſſening of the ſize at plank *rapid,* they would, in defiance of their ſtraightneſs, get entangled, and even as it is, they are ſlightly ſo; but care is taken to get them out by rubbing the body of the hat well with the hand in a circular manner) whilſt, at the ſame time, the dregs keep the hats plump. Another advantage attending the uſe of dregs, whether of beer, porter, or wine, is that as the boiling in the dregs does not draw out much of the mucilage from each hat, when they come to be ſtiffened the dregs form a body within the hat ſufficiently ſtrong or retentive to keep the glew from coming through amongſt the nap: vitriol *alone* would purge or weaken the hats too much, conſequently, half the quantity does better with the addition of dregs, and they diſallow the body to be cloſer from its getting more work: many journeymen, however, to hurry this part of the proceſs, uſe a quantity of vitriol, and open the body again by throwing in a handfull or two of oatmeal; by this means they get a great many made, though at the ſame time they are left quite grainy *from the want of labour.* This, in handling the dry grey hat, when made, may be in part diſcovered, but in part only; in wearing the effect is ſhining ſpots, as if of greaſe, but is, in reality, the glew lodging upon the grainy parts. This error in arrangement I had not diſcovered until I read the account of hat making, extracted from your Journal, in the ſupplement to the Encyclopedia Britannica publiſhed lately: I would firſt beg leave to add, that it was not *that* Encyclopedia alone that I meant to charge with *giving a wrong account of the buſineſs in queſtion,* but all the others, as *Perthenſis, Londinenſis.* It becomes neceſſary to mention this, from the manner in which the conductor of the ſupplement to the Encyclopedia Britannica has introduced the ſubject in queſtion.

Believe me,
With great reſpect,
Your's, &c.

N. L.*

XII.

On the Inventions of ROBERT HOOKE *for regulating Time Pieces. By a Correſpondent.*

To Mr. NICHOLSON.

SIR,

London, July 15, 1800.

IN that ſcarce publication of the famous Robert Hooke, entitled "A Deſcription of Helioſcopes and ſome other Inſtruments," printed in quarto, for Robert Martyn, printer to the Royal Society, 1676, I obſerve (beſides the helioſcopes by repeated reflexions of the ſolar light, and the univerſal joint at preſent ſo well known and eſteemed) a poſtſcript reſpecting the regulation of time pieces by a ſpring, of which I ſend you an abſtract, in hopes that ſome of your ingenious readers will inform the world, whether any farther diſcloſure was ever made of the ſubject there hinted at; and alſo that ſome of your correſpondents, who underſtand the univerſal and real character of Biſhop Wilkins, will tranſlate the communication of which I ſend you a faithful copy. It ſeems to me as if this wonderful man had been well acquainted with all that has ſince appeared as the particular diſcoveries of individuals during the laſt century.

The author firſt vindicates his title to the invention of what is now called the pendulum ſpring, contrived by him in 1658, and made the ſubject of a treaty between himſelf and ſome men of fortune, in 1660, which broke off upon their inſiſting that he ſhould have no ſhare in ſuch improvements as might be afterwards made on his principles. They aſſured him that his invention would be diſcovered by others in ſix months, to which he anſwered, he would give the world ſeven years; and he remarks, that more than fourteen had elapſed without that effect taking place.

In 1664, at the earneſt entreaty of a friend, the doctor read ſeveral of his firſt Cutlerean lectures on the ſubject of time pieces, and ſhewed the ground and reaſon of the applications of ſprings to the balance of a watch for regulating its motion, as the ſame time that he explained above twenty ſeveral methods of application, and how the vibrations might be ſo regulated as to make their durations either all equal, or the greater ſlower or quicker than the leſs, and that in any proportion aſſigned. Some of theſe ways were applicable to leſſer vibrations; others to greater, as of 2, 3, 4, 6, or what number of revolutions were deſired; the models of which he produced in the lecture.

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* I am ſorry that this letter having been miſlaid, renders it neceſſary to apologize to ſo valuable a correſpondent.

As his poſtſcript is directed in controverſy againſt Mr. Oldenburg, the writer of the Philoſophical Tranſactions, and in part againſt M. Huyghens, who adopted the pendulum ſpring, and has deſcribed Hooke's circular pendulum to clocks in his book de Horologio Oſcillatorio, without mentioning the inventor: it contains ſome matters of proof and animadverſion, into which I have no preſent intereſt to enter; but conclude with quoting his own words reſpecting the copy I ſend you, begging leave to enter my humble proteſt againſt the injunction of ſecrecy, which a ſenſe of undeſerved injury may, perhaps, have extorted from this great man (page 30.)

"I ſhall conclude this tract with a ſhort communication of the general ground of my "invention for *pocket watches*, the number of particular ways being very great, which (that "the true lovers of art, and they only may have the benefit of) I have ſet down in the "univerſal and real character of the late reverend prelate my honoured friend Dr. John "Wilkins, lord biſhop of Cheſter, deceaſed. In which I could wiſh all things of this "nature were communicated, it being a character and language ſo truly philoſophical, "and ſo perfectly and thoroughly methodical, that there ſeemeth to be nothing wanting to "make it have the utmoſt perfection, and higheſt idea of any character imaginable, as "well for philoſophical as for common and conſtant uſe. And I have this farther to "deſire of my reader, who will be at the pains to decipher and underſtand this deſcrip-"tion, that he would only make uſe of it for his own information, and not communicate "the explication thereof to any, that hath not had the ſame curioſity with himſelf." See Plate X.

"This I do not ſo much to hinder the ſpreading of this deſcription here delivered, as "to revive, and, if poſſible, bring into uſe and practiſe that excellent deſign: it being a "character and language perfectly free from all manner of ambiguity, and yet the moſt "copious, expreſſive, and ſignificative, of any thing or notion imaginable, and which "recommends it moſt to common uſe, the moſt eaſy to be underſtood and learnt in the "world*."

I am,

SIR,

Your obliged reader,

M. M.

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* For ſome account of Biſhop Wilkins's book ſee our Journal, II. 346.---N.

SCIENTIFIC PUBLICATIONS.

Philoſophical Tranſactions of the Royal Society of London for the Year 1800. *Part I. Quarto.* 238 *pages, with a Meterological Journal, occupying* 26 *pages more, and* 8 *Plates.* Sold by Elmſly, London, price 15 Shillings.

THIS part contains the following papers: 1. The Croonian Lecture. On the Structure and Uſes of the Menbrana Tympani of the Ear. By Everard Home, Eſq. F.R.S. 2. On the Methods of determining from the real Probabilities of Life, the Nature of contingent Reverſions, in which three Lines are involved in the Survivorſhip. By Wm. Morgan, Eſq. F.R.S. 3. Abſtract of a Regiſter of the Barometer Thermometer and Rain at Lyndon, in Rutland, for the Year 1798. By Thomas Barker, Eſq. 4. On the Power of penetrating into Space by Teleſcopes, with a comparative Determination of the Extent of that Power in natural Viſion and in Teleſcopes of various Sizes and Conſtructions: illuſtrated by ſelect Obſervations. By Wm. Herſchell, LL.D. F.R.S. 5. A ſecond Appendix to the improved Solution of a Problem in Phyſical Aſtronomy, inſerted in the Philoſophical Tranſactions for the Year 1798, containing ſome further Remarks, and improved Formulæ, for computing the Coefficients A and B, by which the Arithmetical work is conſiderably ſhortened and facilitated. By the Rev. John Hellins, B.D. F.R.S. and Vicar of Potters Perry, in Northamptonſhire. 6. Account of a Peculiarity in the Diſtribution of the Arteries ſent to the Limbs of ſlowly moving Animals: together with ſome other ſimilar facts. By Anthony Carliſle, Eſq. 7. Outlines of Experiments and Inquiries reſpecting Sound. By Thomas Young, M.D. F.R.S. 8. Obſervations on the Effects which take place from the Deſtruction of the Membrana Tympani of the Ear, by Mr. Aſtley Cooper. 9. Experiments and Obſervations on the Light which is ſpontaneouſly emitted, with ſome degree of permanency, from various Bodies. By Nathaniel Hume, M.D. F.R.S. and A.S. 10. Account of a Series of Experiments undertaken with a View of decompoſing the Muriatic Acid. By Mr. William Henry (ſee page 209 of the preſent Number.) 11. On a new Fulminating Mercury. By Edward Howard, Eſq. F.R S. (ſee Philoſ. Journal, page 173, of the preſent volume.) Appendix. Meteorological Journal kept at the Apartments of the Royal Society, by order of the Preſident and Council.

Reſearches, Chemical and Philoſophical, chiefly concerning Nitrous Oxide, or Dephlogiſticated Nitrous Air, and its Reſpiration. By Humphry Davy, Superintendant of the Medical Pneumatic Inſtitution. 580 *Pages Octavo.* London, printed for J. Johnſon, and Biggs and Cottle, Briſtol.

The Doctrine of Phlogiſton eſtabliſhed, and that of the Compoſition of Water refuted. By Joſeph Prieſtley, LL.D. F.R.S. &c. &c. 90 *Pages Octavo.* Northumberland, in America, printed for the Author, by A. Kennedy, 1800.

Tranſactions of the Royal Society of Edinburgh. Vol. V. Part I. for 1799. 116 *Pages, Quarto.*

This work contains, 1. Inveſtigation of certain Theorems relating to the Figure of the Earth. By Mr. Playfair. 2. Account of certain Phenomena obſerved in the Air Vault of the Furnaces of the Devon Iron Works: together with ſome practical Remarks on the Management of Blaſt Furnaces. By Mr. Roebuck (Philoſ. Jour. IV. 110.) 3. Experiments on Whinſtone and Lava. By Sir James Hall, Bart. (Ph. Jour. IV. 56.) 4. A Chemical Analyſis of three Species of Whinſtone, and two of Lava. By Dr. Robert Kennedy. 5. A new Method of reſolving Cubic Equations. By James Ivory, Eſq.

Fig. 1.

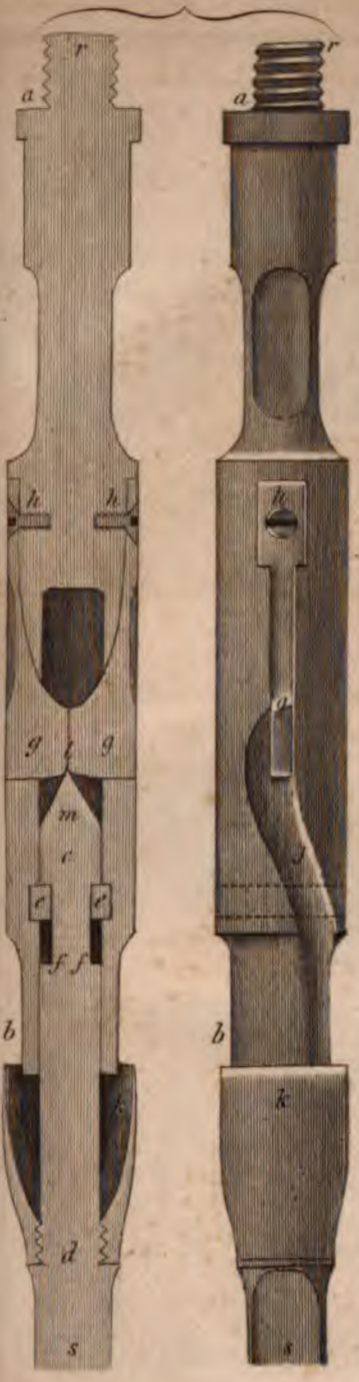


Fig. 2.

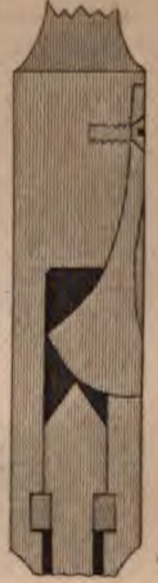


Fig. 4.

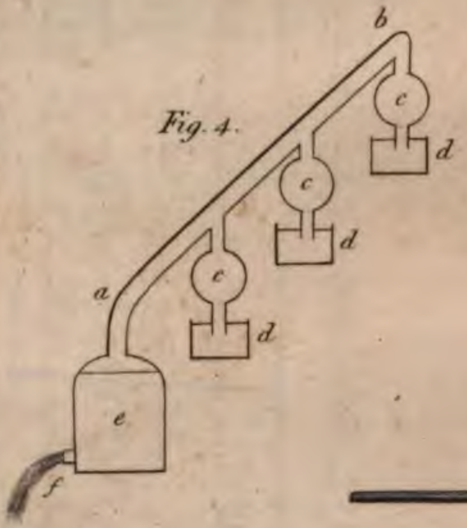
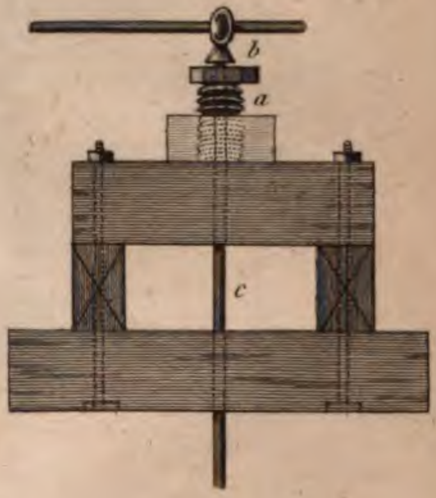


Fig. 3.



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The general Principles of Dr. Hookes Invention of portable Time pieces.

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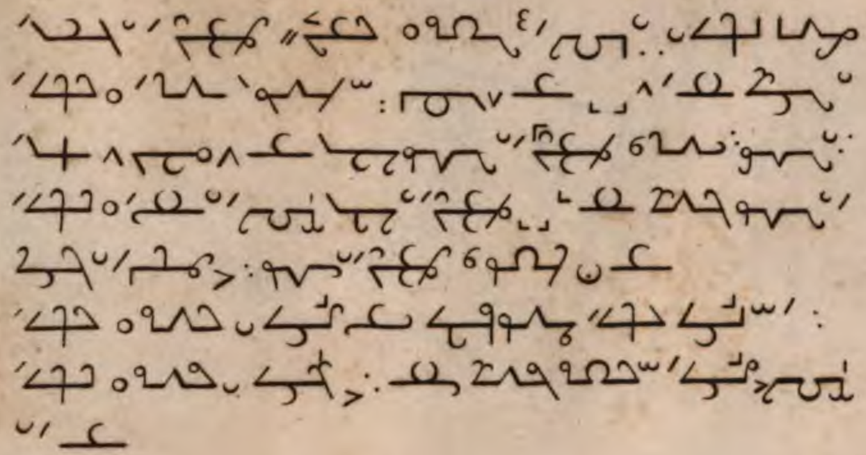

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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SEPTEMBER 1800.

ARTICLE I.

Experiments and Obſervations made with the newly diſcovered Metallic Pile of SIG. VOLTA.

By Lieut. Col. HENRY HALDANE. *With Remarks by* W. N.

IMMEDIATELY after the publication of the proceſs of Signor Volta in a morning paper[*], Col. Haldane, whoſe zeal in the proſecution of philoſophical operation is well known, conſtructed an apparatus, and made a ſeries of experiments for the purpoſe of analyſing the pile itſelf, as well as the nature of its effects, of which he had the goodneſs to favor me with an account. But as theſe inveſtigations, though undoubtedly poſſeſſing the merit of originality, as far as relates to the ſcientific acuteneſs of their author, were in ſome reſpects the ſame as others then unknown to him, but poſſeſſing priority in point of actual date, I poſtponed the public notice of his communication, till it ſhould have the advantage of his reviſal, after peruſing the contents of my laſt paper. The ſame motives at preſent lead me to give an account of the new facts, with a few remarks relative to the theory, of which we are much in want to direct our future operations.

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[*] Paragraph in the Morning Chronicle of May 30, 1800, giving an inaccurate account of the hiſtory of the invention, and deſcribing the general effects as exhibited by Dr. Garnet in the lecture at the Royal Inſtitution.

Col. Haldane's apparatus conſiſted of forty half crowns, with an equal number of pieces of zinc, and 39 cards, which were wetted with pure water. They were placed horizontally on a table, and the experiment of decompoſing water by two copper wires, was repeated and deſcribed with minute accuracy.

He did not receive a ſhock with wetted hands, nor ſee any light when the tongue formed one of the extremes of the circuit; but his ſon was ſtruck with this laſt appearance. Neither did his apparatus affect the electrometer of Bennet, nor my ſpining inſtrument. When a ſewing needle was paſſed beneath the ſkin of a finger of one hand, and another needle inſerted in like manner in the other hand, and theſe were made the extremities of the circuit, a ſharp irritation was felt at the wounded parts, with a convulſive ſenſation, extending to the ſhoulders, and even the neck. But this did not reſemble the electric ſhock, being more unpleaſant, and of longer duration.

The decompoſition of water by ſilver, and by gold wires, was attended with phenomena ſimilar to thoſe deſcribed in our former numbers. The gold wire ſuffered decompoſition.

In the examination of the pile, he found its power diminiſhed by diminiſhing the number of pieces; that it would not act if either of the three plates were omitted; that tinfoil inſtead of ſilver acted tolerably well; and that leather was preferable to card.

When the apparatus was immerſed in water, its action was intirely ſuſpended; but upon taking it out and wiping the external ſurface, without ſeparating the parts, it acted as well as before. Hence I am diſpoſed to conclude, that the power of the Colonel's apparatus was conſiderably impaired by his placing them horizontally, which muſt have favored the efflux of water from the cards, and thence between the faces of the zinc and ſilver: whence his failure in the ſhock and electric ſigns. For the upright pile is certainly much ſtronger than a pile which has been immerſed in water and then wiped.

The apparatus was ſuſpended in the receiver of an air pump, which was then exhauſted of air, till the mercury in the gage ſtood at half an inch. The decompoſition of water in a tube with copper wires, did not proceed during this ſtate, but was renewed when the air was let in.

This author has tried combinations of the various metals, and finds that zinc will act with gold, tin, lead, iron, and copper; that iron will act with the ſame metals, as will alſo lead, though feebly. But no other combinations of theſe metals would anſwer, except that tin and gold afforded a very faint cloud in the water. When iron and ſilver were uſed, the oxidation took place at the wire connected with the iron, and the gas flowed from that connected with the ſilver; but the contrary happened when zinc and iron were uſed, which acted very powerfully, as in this caſe the iron connection afforded the gas. And he conſiders it as worthy of remark, that the oxide of copper wire depoſited in the tube was of a duſky coloured green, different from that produced by the apparatus of zinc and ſilver, as if ſome parts of the metals compoſing the apparatus had entered into the circulation, and affected the colour of the oxide. This obſervation is undoubtedly very curious, and deſerves to be purſued; but I ſhould be inclined to ſuppoſe, that the degree of oxidation in this proceſs would vary with the intenſity of the leading agent, as well as thoſe in which the ſpeed of the effect is governed by heat, dilution, &c.

With an increaſed pile placed in a vertical poſition, Col. H. obtained very weak ſigns of electricity. He connected the apparatus with the conductor of an electrical machine, and found the effect rather impeded than aſſiſted by the common electric ſtream. He placed the plate of Bennet's electrometer in the circuit without producing electric ſigns. He found that the galvanic apparatus placed between the outſide and inſide of a jar prevented its charging, and that it is alſo capable of conducting the charge, though not rapidly: and on the whole, from the very minute exhibition of the attractive and repellent powers, while the cauſticity, the ſhock, and the oxidation, are ſo very powerful, he cannot be perſuaded that electricity is the principal agent, though ſome might be generated, or diſengaged during the operation of the apparatus.

Before I attempt to give a numerical elucidation of the phenomena, I ſhall take the liberty to remark, 1. that Col. Haldane's electric ſtream through the apparatus might have been in the contrary direction to its own ſtream; 2. that the current from an electrical machine may be incomparably leſs in quantity than that produced by the metals; 3. that the experiment with the cap of Bennet's electrometer was not arranged ſo as to direct the ſuppoſed current through the leaves; 4. that the pile muſt in any hypotheſis prevent a jar from being charged higher than itſelf; 5. that Mr. Cruickſhank, of Woolwich, has charged a large jar, ſo as to give a ſhock, merely by placing the pile between its coatings; and, 6. laſtly, whatever may be wanting in *intenſity* of electric force (upon which the ſigns depend) may be made up by *quantity*. Whether the quantity be, indeed, ſufficient to account for what happens, muſt be deduced from the facts. I am aware of the great difficulties which oppoſe themſelves to our reſearches into the laws by which the electric fluid is governed. All the experiments hitherto made, including a conſiderable number which have employed much of my time, but are ſtill too incomplete for publication, are too few and too limited to ſerve as the grounds for computation, or to be extended from that intenſity which affords a ſpark of more than a foot in length, to thoſe minute variations of electrical power which are menſurable by ſparks too ſhort to become the object of our ſenſes. In the following lines, therefore, when I conſider the galvanic phenomena, it is ſimply my wiſh to ſhew that they are not inconſiſtent with deductions made from the preſent ſtate of our knowledge, though the quantities, upon future and more ſtrict examination, may turn out to be very different from what are here exhibited.

The apparatus of Volta may be compared with the common Leyden jar, if we extend by analogy the experiments of Cavendiſh to all ſurfaces and intenſities. For ſhocks nearly equal will be given when the quantities of coated ſurface (or electrical capacity) are directly as the ſquares of the quantities of electricity, or inverſely as the ſquares of the lengths of the ſpark[*]. But it is the character of electric ſhocks which are felt to equal diſtances from the extremities of the animal, that thoſe which are produced by a ſmall quantity of electricity at a high intenſity, produce a ſudden and more tranſient ſenſation than thoſe which are produced by a large quantity of electricity at a low intenſity, and, therefore, probably moving more ſlowly.

From this laſt circumſtance I found it very difficult to take a ſhock from one ſquare foot of coated glaſs which ſhould much reſemble the galvanic ſhock. When the jar was charged to give an exploſive ſpark of one-twentieth of an inch, the ſhock extended above the elbows not quite ſo far as that from a pile of 100 half crowns; but the pain was much more ſudden, ſharp and tranſient. The denſe galvanic ſhock ſeemed to fill the limbs, producing univerſal perſpiration, and leaving a diſpoſition to tremor and unſteadineſs in the parts through which it had paſſed. When the exploſion of the jar meaſured only one-fortieth of an inch, the ſhock ſtill proved to be more unpleaſant at the inſtant than that of any of the piles I have tried, though it did not extend quite to the elbows, and may be conſidered as equal in this reſpect to the ſet of which the electricity was tried by the condenſer, as deſcribed at page 184 of our preſent volume. I ſhall, therefore, take this as one term of compariſon. Another experiment was wanting to determine the length of the ſpark anſwering to the intenſity at which the gold leaf of Bennet's electrometer ſtrikes its coatings. For this purpoſe I took two equal electrical jars, and charged one of them, ſo as to give an exploſive ſpark one-thirtieth of an inch in length. In this ſituation the charged jar was made to communicate with the other in the ſame manner as jars in a battery at top and bottom. It was then removed; having, in fact, loſt half its charge. The receiving jar was in the next place diſcharged, and a connexion again made as before. This ſecond proceſs reduced the original charge to one-fourth. A third reiteration reduced it to one-eighth; and it may eaſily be conceived that by continuing the geometrical ſeries, it was in my power to reduce my charge very much indeed. After ſeven touches the jar ſtill cauſed the leaves to diverge nearly to their greateſt extent, and in ſome repetitions of the experiment the leaves ſtruck. The electricity in this caſe was reduced to $\frac{1}{128}$ part, and, conſequently, would have afforded a ſpark of $\frac{1}{128}$ of $\frac{1}{30}$, or about $\frac{1}{3000}$ of an inch[†]. We may, therefore, take it as a rule, that Bennet's electrometer, at the higheſt ſtate of electricity, had this intenſity.

But we are to compare this intenſity with another ſtill lower. The ſilk face of the condenſer, uſed with the galvanic pile in the experiments referred to, is about one-fiftieth of an inch thick, and the leaves ſtruck when the plate was raiſed about half an inch; the diſtance being thus increaſed twenty-five times. The electric intenſity, or exploſive ſpark of the condenſer, which was the ſame as that of the pile itſelf, was, therefore, ſo many times* leſs than that of the electrometer; that is to ſay, its ſpark was $\frac{1}{25} \times \frac{1}{3000} = \frac{1}{75000}$ of an inch.

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[*] The Hon. Hen. Cavendiſh, Eſq. found that four jars of the ſame ſize and thickneſs gave nearly the ſame, or rather a greater, ſhock than one of them charged with half the quantity of electricity. Philoſ. Tranſ. LXVI. 196.

[†] The length of the ſpark being as the quantity of electricity *ceteris paribus* in moderate intenſities.

Our jar of one foot ſurface, with its ſpark of $\frac{1}{40}$ of an inch, having produced the ſame ſhock as the pile with this ſpark of $\frac{1}{75000}$ of an inch, the ſurface of coated glaſs of the ſame thickneſs as the jar, and equivalent in capacity to the galvanic pile, muſt be as the ſquare of $\frac{1}{40}$ to the ſquare of $\frac{1}{75000}$; that is as 1 to 3½ millions nearly. Such, therefore, according to this method of computing, appears to be the capacity of the galvanic pile, ſo prodigiouſly exceeding the largeſt batteries ever yet conſtructed. But on the other hand, its intenſity being ſo very low, the ſhock may be conſidered as if produced much more by the maſs or quantity of electricity, than by its velocity of motion. Now the quantities of electricity affording equal ſhocks, according to the preceding conditions and doctrine, are inverſely as the intenſities, or in our caſe as 40 to 75000, or 1 to 1875, and our jar might have exploded about 10 times by each turn of a good electrical machine. The large battery of 3½ millions of feet would, conſequently, have required 187 turns to charge it to the low galvanic intenſity; and as the pile was found to charge itſelf in two ſeconds, which is very nearly the time required for each turn of a 24 inch (ſingle) plate machine, we may aſſert that the production, or extrication, of electricity in a ſmall apparatus of this kind is almoſt two hundred times as rapid as that which can be obtained by friction by the labour of one man.

I cannot forbear adverting again to the novelty of the field of reſearch in which I have thus ventured to ſpeculate. We may reaſonably hope that the diſcoveries to which this new exhibition of the joint actions of chemiſtry and electricity may lead us, will ſhew other powers and energies of what is called the electrical fluid, and induce us to reject with gladneſs the imperfect theories afforded by our preſent knowledge of the ſubject †.

II.

Account of a Series of Experiments, undertaken with the View of decompoſing the Muriatic Acid. By Mr. William Henry.

(Concluded from page 214.)

A GREAT variety of ſimilar experiments convinced me, that by electrifying together the carbonated hydrogenous and muriatic gaſes, not the ſmalleſt progreſs was made towards the decompoſition of the latter. All that was thus effected, conſiſted in the decompoſition of the water of the two gaſes, by the carbon of the combuſtible gas; and, when this was completely accompliſhed, no further effect enſued from continuing the electrization. The generation of carbonic acid was proved, by the following experiment.

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* The charge at equal intenſities is inverſely as the thickneſs, as determined by Mr. Cavendiſh in *loco citato*. In extreme caſes, however, I find that the charge follows a higher ratio.

† I have ſince been favoured with another communication from Col. Haldane, which I am obliged to defer to our next.

Exper. 14. To a mixture of carbonated hydrogen and muriatic gaſes, after having received above 100 ſhocks, a drop of water was admitted, which abſorbed the muriatic acid. The liquid was then taken up by blotting-paper; and the reſiduary gas, being transferred into another tube, was brought into contact with a ſolution of pure barytic earth. The precipitation of this ſolution, evinced the preſence of carbonic acid.

It was deſirable, however, that the effects ſhould be aſcertained, of electrifying together pure muriatic acid and pure carbonated hydrogenous gas, both perfectly free from water. Now, from the experiments related in the firſt ſection, it appears highly probable, that a complete purification from moiſture is produced, in both gaſes, by the action of the electric fluid; all the water they before contained being thus decompoſed. In the following experiments, therefore, the two gaſes were ſeparately electrified, before they were ſubmitted to this proceſs conjointly.

Exper. 15. To a portion of muriatic acid, diminiſhed by the action of electricity from 144 to 121 meaſures, 27 meaſures of carbonated hydrogenous gas, expanded as far as poſſible, were added, and 200 ſhocks paſſed through the mixture. The addition of permanent gas amounted to 14 meaſures; 10 of which may be traced to the muriatic acid, and were evolved by its ſeparate electrization. The remaining 4 meaſures, which remain to be accounted for, are too ſmall a quantity to be aſcribed to the decompoſition of the acid.

Exper. 16. To a quantity of carbonated hydrogenous gas, which had received 400 ſhocks, and occupied the ſpace of 212 meaſures, I added 232 of muriatic acid, through which 200 ſhocks had previouſly been paſſed. The electrization of the mixture was next continued, till 800 diſcharges had taken place. On examining the mixture of gaſes, during this operation, no change whatever took place; and, after its cloſe, no more muriatic acid had diſappeared, than would have been deficient after the firſt electrization; nor was there any further production of permanent gas.

Exper. 17. The ſame reſult was obtained, by electrifying together 280 meaſures of carbonated hydrogenous gas, previouſly expanded by 600 ſhocks, and 114 of muriatic acid, after 400 ſhocks. The additional diſcharge, through this mixture, of 1000 ſhocks, did not evince the ſmalleſt progreſs towards the decompoſition of the muriatic acid.

Exper. 18. In the naturally moiſt ſtate of theſe gaſes, it follows, from the 14th experiment, that carbonic acid is produced by electrifying them in conjunction. It appeared to me of ſome importance to aſcertain whether, after a previous decompoſition of their moiſture, carbonic acid would continue to be generated. But the electrified carbonated hydrogenous gas itſelf contains carbonic acid, which, unleſs removed, would render the reſult of the experiment undeciſive. This was accompliſhed by paſſing up, to a portion of electrified gas, a bubble or two of dry ammoniacal gas, which, uniting with the carbonic acid, would condenſe any portion of it that might be preſent. The remainder was transferred into another tube; and, to this carbonated hydrogenous gas, perfectly deprived both of moiſture and carbonic acid, muriatic acid gas, previouſly electrified, was added, and electrical ſhocks were paſſed through the mixture. A drop of water was then admitted; and the reſiduary gas, after having been dried, was transferred into another tube. On paſſing up barytic water, not the ſmalleſt trace of carbonic acid could be diſcovered.

From the preceding experiments, the following concluſions may be deduced.

1. The muriatic acid gas, in the drieſt ſtate in which it can be procured, ſtill contains a portion of water. From a calculation founded on the experiments deſcribed in the firſt ſection, the grounds of which are too obvious to require being ſtated, it follows, that 100 cubical inches of muriatic gas, after expoſure to muriate of lime, ſtill hold in combination 1.4 grain of water.

2. When electrical ſhocks are paſſed through this gas, the watery portion is decompoſed. The hydrogen of the water, uniting with the electric matter, conſtitutes hydrogenous gas, and the oxygen unites with the muriatic acid; which laſt, acting on the mercury, compoſes muriate of mercury.

3. The electric fluid ſerves as an intermedium, in combining oxygen with muriatic acid.

4. The really acid portion of muriatic gas does not ſuſtain any decompoſition by the action of electricity.

5. When electric ſhocks are paſſed through a mixture of carbonated hydrogen and muriatic acid gaſes, the water held in ſolution by theſe gaſes, is decompoſed by the carbon of the compound inflammable gas; and carbonic acid and hydrogenons gaſes are the reſult.

6. When all the water of the two gaſes has been decompoſed, no effect enſues from continuing the electrization; or, if the water of each gas has been previouſly deſtroyed, by electrifying them ſeparately, no further effect enſues from electrifying them conjointly,

7. Since therefore carbon, though placed under the moſt favourable circumſtances for abſtracting from the muriatic acid, and combining with, its oxygen, evinces no ſuch tendency, it may be inferred, that if the muriatic acid be an oxygenated ſubſtance, its radical has a ſtronger affinity for oxygen than charcoal poſſeſſes.

Though the firſt impreſſions excited in my mind by the total failure of the above experiments, in accompliſhing one of the greateſt objects of modern chemiſtry, have induced me for ſome time to withhold them from the ſociety, I am ſatisfied by reflection, that this communication is not without expediency. The means employed in attempting the analyſis of the muriatic acid, were ſuch as, after mature deliberation, appeared to me moſt to promiſe ſucceſs; and the experiments were attended with a degree of labour, which can only be eſtimated by thoſe who have been engaged in ſimilar purſuits; not one third of thoſe which were really made having been deſcribed, in the foregoing account of them. It may ſpare therefore to others, a fruitleſs application of time and trouble, to be made acquainted with what I have done; and the collateral facts, which have preſented themſelves in the inquiry, are perhaps not without curioſity or value.

From the reſult of theſe experiments, I apprehend, all hope muſt be relinquiſhed, of effecting the decompoſition of the muriatic acid, in the way of ſingle elective affinity. They furniſh alſo a ſtrong probability, that the baſis of the muriatic acid is ſome unknown body; for, no combuſtible ſubſtance with which we are acquainted, can retain oxygen, when ſubmitted, in contact with charcoal, to the action of electricity, or of a high temperature. The analyſis of this acid muſt, in future, be attempted with the aid of complicated affinities. Thus, in the maſterly experiment of Mr. Tennant, phoſphorus, which attracts oxygen leſs ſtrongly than charcoal, by the intermediation of lime decompoſes the carbonic acid. Yet, led by the analogy of this fact, its diſcoverer found that a ſimilar artifice did not ſucceed in decompoſing the muriatic acid. "As vital air," he obſerves, "is attracted by a compound of phoſphorus and calcareous earth, more powerfully than by charcoal, I was deſirous of trying their efficacy upon thoſe acids which may from analogy be ſuppoſed to contain vital air, but which are not affected by the application of charcoal. With this intention, I made phoſphorus paſs through a compound of marine acid and calcareous earth, and alſo of fluor acid and calcareous earth, but without producing in either of them any alteration. Since the ſtrong attraction which theſe acids have for calcareous earth tends to prevent their decompoſition, it might be thought, that in this manner they were not more diſpoſed to part with vital air than by the attraction of charcoal: but this, however, does not appear to be the fact. I have found, that phoſphorus cannot be obtained by paſſing marine acid through a compound of bones and charcoal when red-hot. The attraction, therefore, of phoſphorus and lime for vital air, exceeds the attraction of charcoal, by a greater force than that ariſing from the attraction of marine acid for lime*."

By means ſimilar to thoſe employed in attempting the analyſis of the muriatic acid, I tried to effect that of the fluoric acid. When electrified alone, in a glaſs tube coated internally with wax, it ſuſtained a diminution of bulk, and there remained a portion of hydrogenous gas. But, neither in this mode, nor by ſubmitting it, mixed with carbonated hydrogenous gas, to the action of electricity, was any progreſs made towards its analyſis. Theſe experiments, however, render it probable, that the fluoric acid, like the muriatic, is ſuſceptible of ſtill farther oxygenation, in which ſtate it becomes capable of acting on mercury. The carbonic acid, on the contrary, appears not to admit of two different degrees of oxygenation. When the electric ſhock has been repeatedly paſſed through a portion of this acid gas, its bulk is enlarged, and a permanent gas is produced, which is evidently a mixture of oxygenous and hydrogenous gaſes; for, when an electrical ſpark is paſſed through the gas that remains after the abſorption of the carbonic acid by cauſtic alkali, it immediately explodes. Theſe reſults even take place on electrifying carbonic acid from marble, previouſly calcined in a low red heat, to expel its water, and then diſtilled in an earthen retort *.

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* Phil. Tranſ. vol. LXXI, p. 184.

III.

On a New Fulminating Mercury. By Edward Howard, *Eſq. F. R. S.*

(Concluded from page 209.)

SECTION XVII.

I WILL now conclude, by obſerving, that the fulminating mercury ſeems to be characteriſed by the following properties:

It takes fire at the temperature of 368 Fahrenheit; explodes by friction †, by flint and ſteel, and by being thrown into concentrate ſulphuric acid. It is equally inflammable under the exhauſted receiver of an air-pump, as ſurrounded by atmoſpheric air; and it detonates loudly, both by the blow of a hammer, and by a ſtrong electrical ſhock.

Notwithſtanding the compoſition of fulminating ſilver, and of fulminating gold, differ eſſentially from that of fulminating mercury, all three have ſome ſimilar qualities. In tremendous effects, ſilver undoubtedly ſtands firſt, and gold perhaps the laſt. The effects of the mercurial powder and of gunpowder, admit of little compariſon. The one exerts, within certain limits, an almoſt inconceivable force: its agents ſeem to be gas and caloric, very ſuddenly ſet at liberty, and both mercury and water thrown into vapour. The other diſplays a more extended but inferior power: gas and caloric are, comparatively ſpeaking, liberated by degrees: and water, according to Count Rumford, is thrown into vapour ‡.

Hence it ſeems, that the fulminating mercury, from the limitation of its ſphere of action, can ſeldom if ever be applied to mining; and, from the immenſity of its initial force, cannot be uſed in fire-arms, unleſs in caſes where it becomes an object to deſtroy them; perhaps, where it is the practice to ſpike cannon, it may be of ſervice, becauſe, I apprehend, it may be uſed in ſuch a manner as to burſt cannon, without diſperſing any ſplinters.

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* Meſſieurs Landriani and Van Marum (*Annales de Chimie*, tom. ii. p. 270.) obtained only hydrogenous gas, by electrifying the carbonic acid gas. But the conductor of their apparatus was an iron one; which metal would combine with the oxygen of the water, and prevent it from appearing in a gaſeous ſtate. In my experiments, the conductors were of platina.

† Conſequently it ſhould not be incloſed in a bottle with a glaſs ſtopper.

‡ See Philoſophical Tranſactions, for the year 1797, p. 222.

The hard black ſubſtance mentioned by the Count, as remaining after the combuſtion of gunpowder, muſt, I believe, have been an alkaline ſulphuret, mixed chiefly with ſulphite and carbonate of pot-aſh. The conjecture that it is white when firſt formed, is certainly juſt, as my experiment with the glaſs globe evinced.

The inflammation of fulminating mercury by concuſſion, offers nothing more novel or remarkable, than the inflammation, by concuſſion, of many other ſubſtances. The theory of ſuch inflammations has been long ſince expoſed by the celebrated Mr. Berthollet, and confirmed by Meſſieurs Fourcroy and Vauquelin: yet, I muſt confeſs, I am at a loſs to underſtand, why a ſmall quantity of mercurial powder made to detonate by the hammer, or the electric ſhock, ſhould produce a report ſo much louder than when it is inflamed by a match, or by flint and ſteel. It might at firſt be imagined, that the loudneſs of the report could be accounted for, by ſuppoſing the inſtant of the inflammation, and that of the powder's confinement between the hammer and anvil, to be preciſely the ſame; but, when the electrical ſhock is ſent through or over a few grains of the powder, merely laid on ivory, and a loud report is the conſequence, I can form no idea of what cauſes ſuch a report.

The operation by which the powder is prepared, is perhaps one of the moſt beautiful and ſurpriſing in chemiſtry; and it is not a little intereſting to conſider the affinities which are brought into play. The ſuper-abundant nitrous acid of the mercurial ſolution, muſt firſt act on the alcohol, and generate ether, nitrous etherized gas, and oxalic acid. The mercury unites to the two laſt in their naſcent ſtate, and relinquiſhes freſh nitrous acid, to act upon any unaltered alcohol. The oxalic acid, although a prediſpoſing affinity ſeems exerted in favour of its quantity, is evidently not formed faſt enough to retain all the mercury: otherwiſe, no white fumes, during a conſiderable period of the operation, but fulminating mercury alone, would be produced.

Should any doubt ſtill be entertained of the exiſtence of the affinities which have been called prediſpoſing or conſpiring, a proof that ſuch affinities really exiſt, will I think be afforded, by comparing the quantity of oxalic acid which can be generated from given meaſures of nitrous acid and alcohol, with the intervention of mercury, and the intervention of other metals. For inſtance, when two meaſured ounces of alcohol are treated with a ſolution of 100 grains of nickel in a meaſured ounce and a half of nitrous acid, little or no precipitate is produced; yet, by the addition of oxalic acid to the reſiduary liquor, a quantity of oxalate of nickel, after ſome repoſe, is depoſited. Copper affords another illuſtration: 100 grains of copper, diſſolved in a meaſured ounce and an half of nitrous acid, and treated with alcohol, yielded me about 18 grains only of oxalate; although cupreous oxalate was plentifully generated, by dropping oxalic acid into the reſiduary liquor. About 21 grains of pure oxalic acid ſeem to be produced, from the ſame materials, when 100 grains of mercury are interpoſed. (See Section 14.) Beſides, according to the Dutch paper, more than once referred to, acetous acid is the principal reſidue after the preparation of nitrous ether. How can we explain the formation of a greater quantity of oxalic acid, from the ſame materials, with the intervention of 100 grains of mercury, than with the intervention of 100 grains of copper, otherwiſe than by the notion of conſpiring affinities, ſo analogous to what we ſee in other phænomena of nature?

I have attempted, without ſucceſs, to communicate fulminating properties, by means of alcohol, to gold, platina, antimony, tin, copper, iron, lead, zinc, nickel, biſmuth, cobalt, arſenic, and manganeſe; but I have not yet ſufficiently varied my experiments, to enable me to ſpeak with abſolute certainty. Silver, when 20 grains of it were treated with nearly the ſame proportions of nitrous acid and alcohol as 100 grains of mercury, yielded, at the end of the operation, about 3 grains of a grey precipitate, which fulminated with extreme violence. Mr. Cruickſhank had the goodneſs to repeat the experiment; he diſſolved 40 grains of ſilver in 2 ounces of the ſtrongeſt nitrous acid diluted with an equal quantity of water, and obtained (by means of 2 ounces of alcohol) 60 grains of a very white powder, which fulminated like the grey precipitate above deſcribed. It probably combines with the ſame principles as the mercury, and of courſe differs from Mr. Berthollet's fulminating ſilver, alluded to in page 250. I obſerve, that a white precipitate is always produced in the firſt inſtance, and that it may be preſerved, by adding water, as ſoon as it is formed; otherwiſe, when the mother liquor is abundant, it often becomes grey, and is re-diſſolved.

P.S. Since the preceding pages were written, I have been permitted, by the Right Honourable Lord Howe, Lieutenant General of the Ordnance, to make the following trials of the mercurial powder, at Woolwich, in conjunction with Colonel Blomefield, and Mr. Cruickſhank *.

Experiment 1. From the manner in which the ſcrew of the gun-breech, mentioned in Section v. had acted on the barrel, it was imagined, that by burſting an iron caſe, exactly fitted to the bore of a cannon, its ſudden enlargement might make many flaws, and ſplit the piece, without diſperſing any ſplinters. In conformity to this opinion, a caſt iron caſe was conſtructed, with a cylindrical chamber, of equal length and diameter, calculated to hold $3\frac{1}{4}$ ounces troy of the mercurial powder. The caſe, being firmly ſcrewed together, was charged through its vent-hole, and introduced into a twelve-pounder carronade, the bore of which it exactly fitted. The powder was then enflamed, with proper precautions. The gun remained entire, but the caſe divided: the portion forming the upper ſurface of the chamber, was expelled in one maſs; that adjoining the breech, which conſtituted the reſt of the chamber, was cracked in every direction, and in part crumbled; yet it was ſo wedged into ſome indentations which the exploſion had made in the ſides of the piece, that the fragments were not removed without great labour.

Experiment 2. Another caſt iron caſe was prepared, of the ſame ſize as the former, with a chamber alſo cylindrical, but wrought in a tranſverſe direction, and of a greater length than diameter; the thickneſs of metal at each extremity not being more than a quarter of an inch. This caſe was filled with nearly 5 ounces troy of the mercurial powder, and placed in the ſame carronade. Three twelve-pound ſhot were next introduced, and brought into cloſe contact with the upper ſurface of the caſe, as well as with each other. The gun a ſecond time withſtood the exploſion: the caſe was divided acroſs the middle of the chamber, into two equal parts; that adjoining the breech was, as in the former experiment, much flawed, and left immoveable; that neareſt to the muzzle was alſo much flawed, but driven out with the ſhot. All the three ſhot were broken; the two lower being divided into ſeveral pieces, and the upper one cracked through the centre.

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* It is with pleaſure I take this opportunity of acknowledging the civil attention I received from the different officers.

The report was ſo feeble, in both experiments, that an inattentive perſon, I am confident, would not have heard it at the diſtance of two hundred yards.

Experiment 3. It was found ſo difficult to extract the fragments of the caſe remaining in the carronade, after the laſt experiment, that a channel was drilled through them, to the vent-hole of the piece. It was then charged with 6 ounces troy of the mercurial powder, made up as a cartridge, which did not occupy above one half of the diameter of the bore. A wad was placed over the powder, dry ſand ſuperadded, to fill all vacuities, and the gun filled to the muzzle with two twelve-pound ſhot. A block of wood was ſet at a ſmall diſtance, to receive the impreſſion of the ſhot, and the powder was inflamed as uſual. The carronade ſtill reſiſted. One of the ſhot was ſplit into two pieces; and the block of wood was driven to a conſiderable diſtance, but not penetrated by the ſhot above the depth of one inch. The report was ſomewhat louder than the former ones. In all three inſtances, a conſiderable recoil evidently took place. I preſume, therefore, that in the firſt experiment related in the fifth Section, there muſt have been a recoil, though too trifling to be obſerved; and, in the inſtances where the gun and the proof were burſt, it was not ſo much to be expected.

Experiment 4. Finding that the carronade, from the great comparative ſize of its bore to that of its length, required a larger quantity of mercurial powder to burſt it than we were provided with, we charged a half-pounder ſwivel with an ounce and a half avoirdupois of the mercurial powder, (the ſervice charge of gunpowder being 3 ounces) and a half-pound ſhot between two wads. The piece was deſtroyed from the trunnions to the breech, and its fragments thrown thirty or forty yards. The ball penetrated five inches into a block of wood, ſtanding at about a yard from the muzzle of the gun; the part of the ſwivel not broken, was ſcarce, if at all, moved from its original poſition.

Experiment 5. One ounce avoirdupois of the mercurial powder, encloſed in paper, was placed in the centre of a ſhell 4,4 inches in diameter, and the vacant ſpace filled with dry ſand.

The ſhell burſt by the exploſion of the powder, and the fragments were thrown to a conſiderable diſtance. The charge of gunpowder employed to burſt ſhells of this diameter, is 5 ounces avoirdupois.

Experiment 6. A ſea grenade, 3,5 inches diameter, charged like the ſhell in the laſt experiment, was burſt into numerous fragments, by ¼ of an ounce avoirdupois of the mercurial powder. The fragments were projected with but little force, and only to the diſtance of eight or ten yards. The charge of gunpowder required for grenades of this ſize, is 3 ounces.

Experiment 7. A ſea grenade, of the ſame diameter as the laſt mentioned, and charged in the like manner, with ⅛ of an ounce avoirdupois, or 57½ grains, of the mercurial powder, was ſplit into two equal pieces, which were not thrown ten inches aſunder.

The report in the four laſt experiments was very ſharp, but not loud in proportion.

It ſeems, from the manner in which the ſwivel was burſt, in the fourth experiment, that a ſmaller charge would have been ſufficient for the purpoſe. We may therefore infer, both from this inſtance and from the ſecond experiment made with the gun, in Section v, that any piece of ordnance might be deſtroyed, by employing a quantity of the mercurial powder equal in weight to one half of the ſervice charge of gunpowder; and, from the ſeventh and laſt experiment, we may alſo conclude, that it would be poſſible ſo to proportion the charge of mercurial powder to the ſize of different cannons, as to burſt them without diſperſing any ſplinters. But the great danger attending the uſe of fulminating mercury, on account of the facility with which it explodes, will probably prevent its being employed for that purpoſe.

In addition to the other ſingular properties of the fulminating mercury, it may be obſerved, that two ounces inflamed in the open air, ſeem to produce a report much louder than when the ſame quantity is exploded in a gun capable of reſiſting its action. Mr. Cruickſhank, who made ſome of the powder, by my proceſs, remarked that it would not inflame gunpowder. In conſequence of which, we ſpread a mixture of coarſe and fine grained gunpowder upon a parcel of the mercurial powder; and, after the inflammation of the latter, we collected moſt, if not all, of the grains of gunpowder. Can this extraordinary fact be explained by the rapidity of the combuſtion of fulminating mercury? or is it to be ſuppoſed, (as gunpowder will not explode at the temperature at which mercury is thrown into vapour,) that ſufficient caloric is not extricated during this combuſtion.

From the late opportunity I have had of converſing with Mr. Cruickſhank, I find that he has made many accurate experiments on gunpowder; and he has permitted me to ſtate, “that the matter which remains after the exploſion of gunpowder, conſiſts of pot-aſh united with a ſmall proportion of carbonic acid, ſulphate of pot-aſh, a very ſmall quantity of ſulphuret of pot-aſh, and unconſumed charcoal. That 100 grains of good gunpowder yield about 53 grains of this reſiduum, of which three are charcoal. That it is extremely deliqueſcent, and, when expoſed to the air, ſoon abſorbs moiſture ſufficient to diſſolve a part of the alkali; in conſequence of which, the charcoal becomes expoſed, and the whole aſſumes a black or very dark colour.” Mr. Cruickſhank likewiſe informs me, that after the combuſtion of good gunpowder under mercury, no water is ever perceptible.

* *References to the Figures of the glaſs Globe, &c. mentioned in Section VII.*

A, (Pl. VII. of the preſent volume) a ball or globe of glaſs, nearly half an inch thick, and ſeven inches in diameter. It has two necks, on which are cemented the braſs caps, B, C, each being perforated with a female ſcrew, to receive the male ones D, E: through the former a ſmall hole is drilled; the latter is furniſhed with a perforated ſtud or ſhank G. By means of a leather collar H, the neck C can be air-tightly cloſed. When a portion of the powder is to be exploded, it muſt be placed on a piece of paper, and a ſmall wire laid acroſs the paper, through the midſt of the powder: the paper being then cloſed, is to be tied at each end to the wire, with a ſilken thread, as ſhewn at I. One end of this wire is to be faſtened to the end of the ſhank G, and the ſcrew D inſerted to half its length into the braſs cap B; the other end of the wire, *a*, by means of the needle K, is to be drawn through the hole F. The ſcrew E being now fixed in its place, and the wire drawn tight, it is to be ſecured, by puſhing the irregular wooden plug L into the aperture of the ſcrew D, taking care to leave a paſſage for air. The ſtop-cock M, the ſection of which is ſhewn at N, is now to be ſcrewed on to the part D, which is made air-tight by the leather collar *b*. The glaſs tube O is bent, that it may more conveniently be introduced under the receiver of a pneumatic apparatus. P, ſhews the manner of connecting the glaſs tube with the ſtop-cock.

IV.

Additional Remarks on Galvanic Electricity. By Mr. W. Cruickshank, Woolwich.

Communicated by the Author.

BEING deſirous to aſcertain with ſome degree of preciſion, the nature and relative proportions of the gaſes obtained from water and other fluids by this influence, I procured ſome wires of gold, and alſo ſome of ſilver gilt. On trial, I found that theſe wires were not by any means ſo much acted upon as ſilver, at leaſt where water alone was decompoſed; I likewiſe obſerved, that the quantity of oxigen in the mixed gas was much greater, and amounted to nearly one third of the whole; the gas from the zinc wire was alſo more copious, than when ſilver or copper wires were employed.

I took a wide mouthed phial capable of containing 3 ounces, and filled it with very pure lime water, a cork was then looſely introduced into its mouth, and two gold wires paſſed through it; the phial being inverted in a ſmall baſon containing pure water, the other extremities of the wires were connected with thoſe of the pile in the uſual way; a very copious production of gas immediately took place, more eſpecially from the ſilver wire, and in about four hours the phial was completely filled. This gas was ſubmitted to the following experiments:

One meaſure of it being mixed with two of nitrous gas, a diminution of one meaſure took place, the reſiduum contained nitrous gas, mixed with hydrogen.

Four meaſures of this gas were next exploded by the electric ſpark in a ſtrong glaſs jar over mercury, and the whole diſappeared, except about $\frac{1}{15}$ of a meaſure; this was not inflammable, and appeared to be azote.

From theſe experiments it would ſeem, that this compound gas obtained from water by means of gold wires, conſiſts of nearly two parts hydrogen and one oxygen, mixed with a little azote, being nearly the proportions eſtimating by bulk, which are ſaid to enter into the compoſition of water.

It has been ſuppoſed, although not proved by Mr. Nicholſon, that the gas which eſcapes from the wire connected with the ſilver extremity of the pile is hydrogen, whilſt that diſengaged by the one connected with the zinc is oxygen gas.

In order to ſatisfy myſelf about the nature and proportions of theſe gaſes, I took a glaſs tube 10 inches in length, and by means of the blow pipe bent it in the middle, until the legs formed an acute angle reſembling the letter V; while the glaſs was red hot, I contrived to blow an opening at the angle about $\frac{1}{10}$ of an inch, or a little better in diameter. Two gold wires paſſed through corks ſecured by cement, were introduced into the legs, and brought within an inch of each other at the bend. The tube was then filled with diſtilled water, and a finger being placed on the opening at the angle, to prevent the fluid from eſcaping, it was placed in a tea-cup containing water, with the angle downwards, the legs having an inclination of about 45 degrees. The extremities of the wires being then brought into contact with thoſe of the pile, a quantity of gas was diſengaged from both, but by far the greateſt from that connected with the ſilver; by this contrivance the gaſes from the two wires were obtained perfectly diſtinct, each gas aſcending in the leg of the tube which contained its generating wire. When a ſufficient quantity of the two aeriform fluids had been thus obtained, they were examined as follows:

One meaſure of the gas from the ſilver ſide, was mixed with one of nitrous gas, ſome red fumes were obſerved, and a diminution which amounted to $\frac{1}{3}$ of a meaſure took place; the reſiduum conſiſted of a mixture of nitrous and hydrogen gas.

Two meaſures of this gas mixed with one of pure oxygen, being next introduced into a ſtrong glaſs jar over mercury, and fired by the electric ſpark, the whole very nearly diſappeared, not more than $\frac{1}{3}$ of a meaſure remaining; this reſiduary gas appeared from the nitrous teſt to be chiefly oxygen. A denſe white vapour was perceived over the mercury for ſome time after the exploſion.

One meaſure of the gas from the zinc ſide being mixed with two of very pure nitrous gas, the whole very nearly diſappeared, and another meaſure of nitrous gas being added, the total diminution amounted to nearly three meaſures. From theſe experiments it would appear, that the gas obtained from the ſilver wire was chiefly hydrogen gas, and that from the zinc wire, nearly pure oxygen.

Having been favoured by Mr. Nicholſon with ſmall pieces of platina wires, I contrived to faſten them to ſilver ones, and theſe laſt being covered with a compoſition of reſin and bees wax, the platina alone was expoſed to the action of the water, &c. By means of theſe platina points, I obtained gaſes very nearly ſimilar to thoſe juſt deſcribed: but the oxygen was not quite ſo pure, as it often contains $\frac{1}{5}$ or $\frac{1}{4}$ of azote; and when the mixed gaſes were exploded, as in the firſt experiment with the gold wires, there was a reſiduum amounting to $\frac{1}{7}$ of the whole, which was found to be a mixture of hydrogenous and azotic gaſes. It may be proper to obſerve, that in all thoſe caſes where the gaſes were obtained ſeparately, the volume of gas from the ſilver ſide was nearly three times that from the zinc ſide; and that the former always contained a little oxygen, amounting to about $\frac{1}{8}$ or $\frac{1}{10}$ of the whole *.

The platina point, connected with the zinc as well as the gold wire on the ſame ſide, were after ſome time evidently tarniſhed; and this effect was ſoon obſerved when the machine was in full action.

In the following experiment I obtained a perfect ſolution of gold:

After having precipitated magneſia and argill from their ſolutions in acids, by the influence of the wire from the ſilver ſide, I was deſirous to ſee what effect would be produced on ſolutions of lime. For this purpoſe the glaſs tube was filled with a ſolution of the muriate of lime which had been cryſtalliſed, and the gold wires applied in the uſual manner. When the tube was placed in the circle of communication, little or no gas eſcaped from the ſilver wire, but a conſiderable quantity began to aſcend immediately from the zinc one, and the fluid ſurrounding it aſſumed a fine yellow colour, which was found to proceed from a ſolution of the gold, the wire at the end of the proceſs being much corroded. After ſome time gas was likewiſe diſengaged from the ſilver wire, but there was not the leaſt precipitation of lime; when the wires were removed the fluid ſmelled of aqua regia, or the ox. muriatic acid.

When platina points were employed inſtead of the gold wires, the ſmell of the nitromuriatic acid was alſo ſoon perceived, but no ſenſible ſolution of the platina was obſervable.

This formation of the nitro-muriatic, or oxygenated muriatic acids, was not obſerved but where the perfect metals gold and platina were employed; the reaſon of which muſt be obvious. When the tube was filled with a ſolution of common ſalt inſtead of the muriate of lime, a nitro-muriatic acid was likewiſe produced. The effects produced by the gold or platina wires on the tinctures of Litmus and Brazil wood, were ſtill more remarkable than thoſe deſcribed in the former paper, more particularly on the litmus, which was very quickly reddened. When the tube was filled with diſtilled water only, and gold wires employed; after the influence had paſſed through for ſome time (without confining the gas) that portion of the fluid in contact with the ſilver wire being decanted, ſtrongly reddened the tincture of Brazil wood. This mode of making the experiment was preferred, as it might be ſuppoſed that the hydrogen in its naſcent ſtate would unite with the colouring matter of the Brazil wood, and produce the effect of an alkali.

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* Was this oxygen originally held in ſolution by the water? C.

From the experiments related in this and the former paper, we may draw the following concluſions:

1. That hydrogen gas mixed with a very ſmall proportion of oxygen and ammonia, is ſomehow diſengaged at the wire connected with the ſilver extremity of the machine; and that this effect is equally produced, whatever the nature of the metallic wire may be, provided the fluid operated upon be pure water.

2. That where metallic ſolutions are employed inſtead of water, the ſame wire which ſeparates the hydrogen revives the metallic calx, and depoſits it at the extremity of the wire in its pure metallic ſtate; in this caſe no hydrogen gas is diſengaged. The wire employed for this purpoſe may be of any metal.

3. That of the earthy ſolutions, thoſe of magneſia and argill only are decompoſed by the ſilver wire, a circumſtance which ſtrongly favours the production of ammonia.

4. That when the wire connected with the zinc extremity of the pile conſiſts either of gold or platina, a quantity of oxygen gas, mixed with a little azote and nitrous acid is diſengaged, and the quantity of gas thus obtained is a little better than $\frac{1}{3}$ of the hydrogen gas ſeparated by the ſilver wire at the ſame time.

5. That when the wire connected with the zinc is ſilver, or any of the imperfect metals, a ſmall portion of oxygenous gas is likewiſe given out, but the wire itſelf is either oxydated or diſſolved, or partly oxydated and partly diſſolved; indeed, the effect in this caſe produced upon the metal, is very ſimilar to that of the concentrated nitrous acid, where a great deal of the metal is oxydated, and but a ſmall quantity held in ſolution *.

6. That when the gaſes obtained by gold or platina wires, are collected together and exploded over mercury, the whole nearly diſappears and forms water, with probably a little nitrous acid, for there was always a thick white vapour perceived for ſome time after the exploſion. The reſiduary gas in this caſe appeared to be azote.

In reflecting on theſe experiments it would appear, that in ſome of them, the water muſt be decompoſed; but how this can be effected, is by no means ſo eaſily explained. For example, it ſeems extremely myſterious how the oxygen ſhould paſs ſilently from the extremity of the ſilver wire to that of the zinc wire, and there make its appearance in the form of gas. It is to be obſerved likewiſe, that this effect takes place which ever way the wires are placed, and whatever bends may be interpoſed between their extremities, provided the diſtance be not too great. On conſidering theſe facts more minutely, it appeared to me, that the eaſieſt and ſimpleſt mode of explanation, would be, to ſuppoſe that the galvanic influence (whatever it may be) is capable of exiſting in two ſtates, that is, in an oxygenated and deoxygenated ſtate. That when it paſſes from metals to fluids containing

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* The great difference in the effect produced by this influence on gold and ſilver, which have always been conſidered as equally difficult to oxidate, can only be explained on the ſuppoſition, that nitrous acid is generated; for this acid, it is well known, acts powerfully on ſilver, but has no action whatever on gold. The ſame obſervation applies to platina.

oxygen, it ſeizes their oxygen, and becomes oxygenated; but when it paſſes from the fluid to the metal again, it aſſumes its former ſtate, and becomes deoxygenated. Now when water is the fluid interpoſed, and the influence enters it from the ſilver ſide deoxygenated, (and we ſuppoſe that it always paſſes from the deoxygenated to the oxygenated ſide) it ſeizes the oxygen of the water, and diſengages the hydrogen, which accordingly appears in the form of gas; but when the influence enters the zinc wire, it parts with the oxygen, with which it had formerly united, and this either eſcapes in the form of gas, unites with the metal to form an oxyde, or, combined with a certain portion of water, &c. may, according to the German chemiſts, form nitrous acid. When a metallic ſolution is the interpoſed fluid, the effect produced may be explained in two ways[*], but the ſimpleſt is to ſuppoſe, that the influence in paſſing from the ſilver wire, ſeizes the oxygen of the metallic calx, and afterwards depoſits it on entering the zinc one; in this caſe no gas ſhould appear at the ſilver wire, but when a perfect metal is employed, oxygen ſhould be diſengaged from the zinc wire; and this, as has been already mentioned, is exactly what takes place. What I conſider, however, as the ſtrongeſt argument in favour of this hypotheſis, is, that all fluids which do not contain oxygen, are incapable of tranſmitting the galvanic fluid, ſuch as alcohol, æther, the fat, and eſſential oils, as I have proved by direct experiment; but on the contrary, that all thoſe which do contain oxygen conduct it more or leſs readily, as all aqueous fluids, metallic ſolutions, and acids, more eſpecially the concentrated ſulphuric acid; which it decompoſes. In this laſt inſtance, the oxygen produced can hardly be aſcribed to the decompoſition of water; for this acid, when properly concentrated, does not contain any ſenſible quantity. By this theory alſo, we can readily explain the oxydation of the zinc plates in the machine; where the fluid in paſſing from the different pairs of plates, appears to be alternately oxygenated and deoxygenated. Although I am not by any means intirely ſatisfied with this hypotheſis, yet, as it is the only one by which I can explain the different phenomena, it was thought adviſeable to throw it out, merely with a view to induce others to reaſon upon the ſubject, and to incite them to make experiments, by which alone truth can be aſcertained.

A convenient and powerful machine is at preſent a great deſideratum in galvaniſm: the common pile, although at firſt ſufficiently ſtrong, very ſoon loſes its power; it is beſides very troubleſome to be conſtantly repiling it, and clearing the different pieces from oxidation, &c. which muſt neceſſarily be done, if it is intended that the apparatus ſhould produce the full effect. The contrivance which has ſucceeded beſt with me is as follows: I conſtructed a kind of trough of baked wood, 26 inches in length, 1.7 inches deep, and 1,5 inches wide; in the ſides of this trough grooves were made oppoſite to each other, about the tenth of an inch in depth, and ſufficiently wide to admit one of the plates of zinc and ſilver when ſoldered together; three of theſe grooves were made in the ſpace of one inch and three tenths, ſo that the whole machine contained 60 pairs of plates. A plate of zinc and ſilver, each 1,6 inches ſquare, well cemented together, were introduced into each of theſe grooves or notches, and afterwards cemented into the trough by a compoſition of roſin and wax, ſo perfectly, that no water could paſs from one cell to the other, nor between the plates of zinc and ſilver. This circumſtance muſt be ſtrictly attended to, elſe the machine will be extremely imperfect. When all the plates were thus ſecured in the trough, the interſtices or cells formed by the different pairs of plates were filled with a ſolution of the muriate of ammonia, which here ſupplied the place of the moiſtened papers in the pile, but anſwered the purpoſe much better. It is hardly neceſſary to obſerve, that in fixing the zinc and ſilver plates, they muſt be placed regularly, as in the pile, *viz.* alternately zinc and ſilver, the ſilver plate being always on the ſame ſide. When a communication was made between the firſt and laſt cell, a ſtrong ſhock was felt in the arms, but ſomewhat different from that given by the pile, being quicker, leſs tremulous, and bearing a greater reſemblance to the common electrical ſhock. I conſtructed two of theſe machines, which contained in all 100 pairs of plates; theſe when joined together, gave a very ſtrong ſhock, and the ſpark could be taken in the day time at pleaſure; but what ſurprized me not a little, was the very ſlender power which they poſſeſſed in decompoſing water; in this reſpect they were certainly inferior to a pile of 30 pairs, although ſuch a pile would not give a ſhock of one third the ſtrength.

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[*] See the former paper on this ſubject.

This apparatus retained its power for many days, and would in all probability have retained it much longer, had not the fluid got between the dry ſurfaces of the metals. To remedy this defect, I have now ſoldered the zinc and ſilver plates together, and find that this method anſwers very well.

The zinc plates may be cleaned at any time, by filling the different cells for a few minutes with the dilute muriatic acid. Although this apparatus may not intirely ſuperſede the pile, eſpecially if it ſhould be found to decompoſe water, &c. but ſlowly, yet in other reſpects it will no doubt be found very convenient and portable.

Having been lately favoured by Mr. Howard with narrow ſlips of platina, of a conſiderable length, I repeated the experiments reſpecting the decompoſition of water, and with very nearly the ſame reſults. Seven meaſures of the compound gas obtained in the manner formerly deſcribed, being exploded over mercury by the electric ſpark, there remained no more than $\frac{1}{5}$ of a meaſure, or $\frac{1}{35}$ of the whole; this appeared to be azote, for it ſuffered no diminution with nitrous gas, nor was it inflammable. I likewiſe mixed one meaſure of this compound gas with two of nitrous gas, and the diminution amounted to one meaſure; nearly one third of the whole was therefore oxygenous gas, the remaining two thirds being hydrogen gas, with a little azote.

Having found that the ſolutions of metals in acids were decompoſed by this influence, I next wiſhed to aſcertain if their ſolutions in alkalies, more particularly ammonia, could be decompoſed in the ſame way. For this purpoſe, I added to a dilute ſolution of the nitrate of ſilver ſome pure ammonia, until the mixture ſmelled ſtrongly of the latter ſubſtance. This mixture was introduced into a glaſs tube ſecured by corks in the uſual way, and ſilver wires applied. When the tube was placed in the circle of communication, a very rapid production of gas took place from the wire connected with the ſilver extremity of the machine, although but little or none eſcaped from the zinc wire during the whole proceſs.

After ſome time a quantity of greyiſh flacks, evidently metallic ſilver, were ſeparated by the wire which gave out the gas, and a dark grey powder was depoſited on the zinc wire; at this period of the proceſs much leſs gas was diſengaged, although a conſiderable quantity ſtill appeared at the ſilver wire. After ſome hours the apparatus was removed; at this time a very conſiderable quantity of metallic ſilver was depoſited, and the zinc wire was encruſted with a blueiſh black ſubſtance. On endeavouring to remove this cruſt with the finger part of it exploded, although ſtill moiſt: the wire was found to be very much corroded, and full of holes. Next morning the powder which adhered to the wire being dry, was touched with a knife, when it exploded again, with a very conſiderable noiſe. There can be little doubt, that theſe exploſions proceeded from the fulminating ſilver of Bertholet, which muſt have been formed at the zinc wire; indeed ſome effect of this kind was expected. This experiment was repeated, and inſtead of a ſolution of ſilver nothing but pure ammonia was introduced into the tube, which anſwered equally well; for the ſilver of the zinc wire after being corroded, &c. was immediately taken up by the Vol. Alkali, and afterwards depoſited in its metallic form by the ſilver wire. A ſmall quantity of a fulminating ſubſtance likewiſe adhered to the zinc wire; and the fluid ſurrounding it, being poured into a wine glaſs, depoſited after ſtanding for ſome time, a black ſhining film, which when dried, detonated like Bertholet's powder, and reſembled it in all reſpects.

Another tube containing pure ammonia had copper wires introduced into it; after this had been ſubmitted to the action of the machine for ſome time, the fluid ſurrounding the zinc wire aſſumed a fine blue colour from diſſolved copper, and the ſilver wire began to depoſit metallic copper; this proceſs continued for ſeveral hours, when a conſiderable quantity of very pure metal was precipitated; the blue colour of the fluid, however, never extended ſo high as the end of the ſilver wire, for as faſt as it aſcended, the metal was immediately thrown down in its metallic form, by this wire. I next filled the tube with an ammoniacal ſolution of copper, and employed copper wires. In a very ſhort time after this had been expoſed to the action of the machine, the upper part of the fluid in contact with the ſilver wire, became conſiderably paler, and copper was precipitated; in about an hour the whole of the fluid in this part of the tube became as colourleſs as diſtilled water, ſo completely had the metal been precipitated. The precipitated metal was alſo the pureſt I had ever ſeen. From theſe experiments it would appear, that the galvanic influence might be employed with ſucceſs in the analyſis of minerals, more particularly in ſeparating lead, copper, and ſilver, from their different ſolutions; a very ſmall quantity of a metal may likewiſe be detected in this way, as I have found from direct trial. Before I finiſh theſe obſervations on ammonia and ammoniacal ſolutions, I ſhall give the reſult of one experiment, where the vol. alkali itſelf was decompoſed.

A quantity of pure ammonia being introduced into a bottle, and inverted in ſome of the ſame fluid in the manner already explained, the connecting wire on the zinc ſide was changed from ſilver to platina; in this caſe a very rapid production of gas took place from both wires, but the greateſt from the ſilver wire. After two ounce meaſures of this gas had been collected, it was examined, and found to conſiſt of,

Hydrogen gas	15 parts
Azotic gas	13
And oxygen gas	2 nearly.
	30

Indeed the quantity of oxygen was ſo ſmall, that it could only be detected by nitrous gas, and it may have been originally contained in the fluid. In this caſe the ammonia muſt have been decompoſed at the zinc wire, the diſengaged oxygene uniting with the hydrogen, while the azote of the ammonia eſcaped in the form of gas, and mixed with the hydrogen diſengaged at the ſame time from the ſilver wire.

In a former paper I mentioned that the ſulphuric acid was decompoſed by galvaniſm, without deſcribing however the particular appearances. A quantity of the concentrated ſulphuric acid was poured into a tube bent in the form of the letter V, two ſlips of platina were introduced, one into each leg, and brought within an inch of each other at the bend; the tube was then placed in an inclining poſition with the bend downwards, and the platina ſlips were attached to the extremities of the machine.

In a few ſeconds a very conſiderable quantity of gas made its appearance at the zinc wire, and continued to riſe during the whole proceſs; from the ſilver wire a ſmall quantity of gas was likewiſe diſengaged, but in a ſhort time the fluid in this leg of the tube loſt its tranſparency, and became milky and opake. After the proceſs had been continued for ſeveral hours, a quantity of a yellowiſh white powder was depoſited on the ſilver wire, and the fluid ſtill continued muddy; the acid, however, in the other leg, not only preſerved its tranſparency, but became even more ſo; it likewiſe had a peculiar ſmell, ſomewhat like that of the ox. mur. acid when very dilute. The tube being removed, water was added to the muddy acid, in conſequence of which a whitiſh powder was depoſited, which was found to be ſulphur. The yellowiſh powder depoſited on the platina ſlip proved likewiſe to be ſulphur. The gas obtained during this proceſs was not particularly examined, it was only found not to be inflammable, or if it contained any inflammable gas, it muſt have been in very ſmall quantity.

The effects of this influence upon the nitrous acid were not exactly what might have been expected; ſome of this fluid in its moſt concentrated and fuming ſtate, being introduced into the bent tube juſt deſcribed, and the platina ſlips applied, very little or no gas was diſengaged from the ſlips or wires of either ſide, nor even after ſome time, was the appearance of the acid ſenſibly changed. The influence, however, was perfectly tranſmitted, for the machine, although in full action, did not give the leaſt ſhock whilſt the

tube

was applied; beſides the wire on the zinc ſide being removed from the plate, and plunged in a cup containing water, whilſt a copper wire reaching from the cup to the zinc plate, preſerved the communication, gas iſſued copiouſly from the platina wire, and the copper was corroded as uſual.

The acid was then diluted with an equal bulk of water, or rather more, and being again introduced into the tube, ſtill very little gas was produced on either ſide. From theſe facts it would appear that the nitrous acid is ſo perfect a conductor of this influence as to tranſmit it like metals, without being at all acted upon by it. This may poſſibly be owing to the great proportion of oxygen which enters into its compoſition, for we have already remarked, that all fluids, containing very little or no oxygen, are perfect non-conductors, or nearly ſo; and it is probable that the conducting fluids are more or leſs ſo in proportion to their quantity of oxygen.

It was remarked in a former paper, that it was highly probable, the nitrous, or ſome other acid, was produced at the zinc wire, I ſhall now bring forward ſome additional facts and arguments in ſupport of this hypotheſis.

1ſt. If the tincture of litmus be introduced into the bent tube already mentioned, and platina wires employed; after ſome time the whole of the litmus in the leg of the tube connected with the zinc wire will be rendered perfectly red, and in ſome places the colour even partially deſtroyed, a well known effect of the nitrous acid on blue vegetable infuſions. But if the litmus be mixed with a little of any pure alkali, no ſuch change of colour can be perceived.

2dly. All metals which are diſſolved or acted upon by the nitrous acid, are likewiſe very quickly corroded by the galvanic influence; but ſuch as are not acted upon by this acid are not at all affected by this influence. Of this we have ſtriking examples in ſilver, mercury, gold, and platina; the firſt two are readily acted upon both by the nitrous acid and galvaniſm, but the two laſt by neither in any ſenſible degree, and poſſibly not at all when perfectly pure. Now ſilver, which has always been conſidered as a perfect metal, cannot be oxydated by any of the uſual methods, any more than gold, yet this influence has fully as great an effect upon it, as upon copper; which we conceive may be explained in the following manner:—Let us ſuppoſe that a little nitrous acid is produced on the ſurface of the ſilver, the metal muſt in this caſe be in ſome degree acted upon by it, and this action will neceſſarily diſpoſe it to unite with oxygen, which, being thus in its naſcent ſtate, muſt readily combine with the ſilver, and form inſoluble nitrate with exceſs of oxyde, which will, conſequently, be but little ſoluble in water. Now that this is really the caſe, we think is rendered probable by the following experiments:

A ſmall wide mouthed bottle was filled with a dilute ſolution of pure pot-aſh, and inverted in a cup, containing ſome of the ſame ſolution. Two ſilver wires, previouſly bent, were introduced a little way beyond its mouth, and then connected with the machine in the way formerly deſcribed. A very rapid production of gas took place from both wires, and a few black flocks were perceived floating in the fluid: when a ſufficient quantity of gas was thus obtained, it was examined, and found to conſiſt of oxygen eleven parts, and hydrogen twenty-five parts; and four meaſures of it being exploded by the electric ſpark, leſs than half a meaſure remained, or about one-ninth of the whole. From hence it is evident that the ſilver was but very little acted upon in this experiment, for we obtained nearly the ſame quantity of oxygen as if gold or platina wires had been employed. The bottle was next filled with diſtilled water, and the ſame proceſs repeated. In this caſe the ſilver was much corroded, and very little gas eſcaped from the zinc ſide; a ſufficient quantity of gas being, however, obtained, it was examined, and found to conſiſt of oxygen one part, and hydrogen ſix parts. This mixture did not explode without the addition of more oxygen. The cauſe of theſe different reſults is readily explained on the ſuppoſition of an acid. For in the firſt experiment the generated acid would immediately unite with the alkali in preference to the ſilver, and by this means the principal agent in the oxydation would be removed. Similar experiments were made with copper wires, and with exactly the ſame reſults: in this caſe, when the bottle was filled with the alkali, no green ſubſtance was produced at the zinc wire; indeed, the copper was ſcarcely, if at all, acted upon.

3dly, The corroded ſubſtances generated by this influence at the extremities of the zinc wires are not pure oxydes, which, upon the ſuppoſition of ſimple oxydation they ought to be, when nothing but diſtilled water is employed. We have remarkable examples of this in the blueiſh green ſubſtance obtained from copper, and the dirty greeniſh yellow one from mercury, the laſt of which exactly reſembles what has been called nitrous turbeth when too much waſhed with water, and if pure ammonia be added to it, it inſtantly becomes black, a proof that it contains ſome acid, for ammonia has no effect upon the pure oxyde of mercury, but a conſiderable one upon all the impure, rendering them black, as turpeth mineral, &c. With regard to the green ſubſtance from copper, it is very certain that we have no pure oxyde of this colour, for all the green oxydes, as they have been improperly called, contain acids of ſome kind, as the carbonic, arſenical, acetous, &c. and it is well known that the precipitates by the carbonated alkalis, &c. are all of this kind. Indeed, there is but one pure oxyde of copper, viz. the dark red, or deep brown. Now, as theſe impure oxydes cannot be generated without an acid, and as there can be none in diſtilled or lime water, it muſt follow, that an acid is ſomehow generated in the proceſs; and as this acid acts both upon mercury and ſilver, it is moſt probably the nitrous. The quantity, however, is certainly ſmall, owing, in a great meaſure, to the weakneſs and imperfection of our machines; and it is this circumſtance which renders it ſo difficult to detect it with certainty.

It may be aſked, how can this acid be formed? If azote were a compound ſubſtance, as has been lately affirmed, this queſtion could be very readily anſwered; but if not, we muſt then ſuppoſe that a ſmall quantity of azote is always preſent in water, however long boiled, or repeatedly diſtilled, and that it is with this the oxygen in its naſcent ſtate unites and forms nitrous acid. The production of ammonia at the ſilver wire can be explained on either hypotheſis; but we ſhall take another opportunity of examining this part of the ſubject.

The new apparatus which I deſcribed in a former paper, ſeems to anſwer better than was at firſt expected; it has now been in uſe occaſionally for a month, and whenever a freſh ſolution of ſalt, or muriate of ammonia, is introduced, it appears to be fully as ſtrong as at firſt. The addition of a very ſmall proportion of nitrous or muriatic acid to the ſolution of common ſalt, adds conſiderably to its efficacy.

V.

Deſcription of a Mercurial Air Pump; and of a Double-barreled Air Pump. By RICHARD AUGUSTUS CLARE, *Surgeon, Jamaica. Communicated by the Author.*

FIG. 1, Pl. XI. repreſents a ſection of the different parts of the pump; A, a large iron tube (being the barrel of the pump) B a ſmaller ditto. Theſe tubes are flanched to the iron ſtep C. This ſtep has a perforation in it, making a communication between the bottoms of the tubes. The tube B is ſurmounted by a plate of iron, on which is flanched the iron hemiſphere D. The hemiſphere has a hole at top, one-tenth of an inch in diameter, with a cap valve and baſon at E. A pipe, *b b*, of iron or glaſs, is inſerted through the upper part of the hemiſphere, and reaches within a little of the iron plate on which it is flanched. The pipe *b b* riſes about thirty-five inches above the level of the baſon E; it is reverted at top, and deſcends to be inſerted into the upper part of the piece G, which has a ſtop cock at *c*, leading to the receiver. To the lower part of the piece G, the long barometer gauge *d* is attached, the lower end of which deſcends into the baſon of mercury *e*. F is a cylindrical piece of light wood (which I ſhall call the piſton) made to ſlide eaſy in the hollow cylinder A. There is a ſmall ſcrew at *a*, to draw off the mercury when required. The valve at E is made of a thin iron hoop about three-fourths of an inch long, and half an inch in diameter; the lower end of this hoop is covered with a piece of bladder in the manner of a drum. The valve fits looſely into the cap, which has three grooves on the inſide, made in a perpendicular direction from bottom to top. The uſe of theſe grooves is to allow the mercury to riſe freely by the ſides of the valve hoop. The uſe of the baſon is only to contain the ſuperflous mercury. To try the power of this pump, let the piſton be puſhed down to the bottom of the cylinder, and retained there, as it will have a tendency to riſe when the mercury is poured in. Take the valve out of the cap at E, and pour mercury into the baſon till it riſes as high as the top of the cap, then thruſt down the valve, till the mercury runs over the top of its hoop and fills it. The ſpecific gravity of the hoop ſo filled will be little leſs than that of mercury itſelf. Let the piſton be now drawn up, the mercury will ſubſide from the bottom of the valve, and make a vacuum in the hemiſphere. No air can enter by the valve, as it will be always ſurrounded by and covered with mercury.

When the mercury ſinks below the bottom of the tube *b b*, the air will enter the vacant hemiſphere from the gauge or receiver. When the piſton is depreſſed, the mercury will again riſe in the hemiſphere, and cloſing the orifice of the tube *b b*, will expel the air through the valve at E. The mercury will (as the exhauſtion goes on) riſe in the tubes *b b* and *d*, and thereby indicate the degree of rarefaction, but it can never riſe ſo high as to run over the arch of the tube *b b*, becauſe the column of mercury in that tube will be long enough to balance the weight of the atmoſphere. As the oſcillation of the mercury in the tube *b b* is apt to be very violent if not reſtrained, a ſmall cork ball, with a ſhort piece of iron wire ſtruck into one ſide of it, may be placed under the orifice of that tube, the iron wire riſing within the tube as repreſented at fig. 2. This ball will float on the mercury, and being thereby preſſed tight againſt the orifice of the tubes, will prevent the mercury from riſing too faſt, or producing any ſudden oſcillation therein. The drawings and deſcription of an air pump on this principle, reduced to a very portable ſize, were ſent to Mr. Adams, mathematical inſtrument maker, of Charing-croſs, London, in the beginning of the year 1796, who deemed the execution of it impracticable. Being informed of this, I deſired the drawings, &c. might be laid before Mr. Nairne; who, though he offered to make the inſtrument, gave it as his candid opinion that it would not anſwer my purpoſe to the full extent of my wiſhes. I, therefore, dropt the idea of getting an inſtrument of this kind made in England, on account of the great diſtance and length of time neceſſary to carry on a correſpondence on ſuch a ſubject.

Mr. Nairne having given no reaſon, however, for his opinion, except that it would coſt a great deal of money, I was reſolved to attempt the fabrication of one myſelf. As I had no means of getting iron pipes made to the ſize I wiſhed, I was obliged to make ſhift with what I could get. The tube A A was of caſt iron (and had formerly belonged to a water pump) cut to the proper length. The tube B B was a gun barrel, and the reſt of the inſtrument was made up in the ſame way, of any thing I could pick up. Being but a bungling workman, however, having but few tools, and no advice, or aſſiſtance, I never could make all the joints perfectly air tight, though I laboured hard to effect it.

I am fully convinced, however, from the experience I have gained with this clumſy and imperfect inſtrument, that it would anſwer the purpoſe if it were well conſtructed.

The greateſt inconvenience attending the uſe of it was occaſioned by the oſcillation of the mercury, in conſequence of the great difference in the diameters of the tubes through which it moves.

Deſcription of a Double-barreled Air Pump.

Fig. 2. repreſents a ſection of the pump, &c. where A and B are the two barrels, open at top, and furniſhed with piſtons, which are worked by rack and wheel in the uſual manner. The piſtons are alike in each barrel, and are perforated through the centre, as is the ſolid part of the rack alſo, to admit the hollow wire *a a* to ſlide therein. This wire is made to move air tight, by means of collars of leather in the centre of the piſton. The wire is hollow from end to end, and is fixed to the centre of the bottom of the barrel. Near the upper end of the wire a hole is drilled through it, at right angles with that in its axis; this hole is at ſuch a diſtance from the bottom of the barrel, that, when the piſton is drawn up, the hole opens into the barrel, as is ſeen in the barrel B. Each piſton is furniſhed with a common ſilk valve *g*, opening upwards. The hole in the piſton rack, through which the wire ſlides, is ſecured at top by the ſtop ſcrew *h*. The barrels are flanched on the bottom piece *c c*, into which the ſliding wires are fixed as above deſcribed. The hollow wire in the barrel B communicates at bottom with the duct *d b*, leading through the cock *c*, and that in the barrel A communicates with the duct *e f*; *f* is the hole leading back to the receiver.

The cock *c* is perforated, as repreſented in the figure, and has a motion of one quarter of a revolution to the left, from its preſent poſition. The piſtons ſhould be well leathered, and always have about a quarter of an inch of oil on their upper ſurfaces. The operation of this pump is as follows:—Let the cock ſtand as repreſented in the figure, in which ſituation it opens a communication between the receiver and the barrel B, through the duct *c b* and ſliding wire, when the piſton is drawn up. When the piſton B is depreſſed, the lateral hole in the ſliding wire is ſtopped by paſſing through the collar of leather, and the air below it not being able to return into the receiver, is expelled through the valve *g* in that piſton. By this time the piſton A has arrived at the top of that barrel, and the air will have a free paſſage into it from the receiver, through the hollow wire, &c. On the depreſſion of this piſton the air below it will be expelled through its valve *g*, and thus by the alternate elevations and depreſſions of the piſtons a pretty good exhauſtion will be made: at length, however, it will ceaſe to take more air from the receiver, in conſequence of the ſmall quantity of air in the valve holes and under the piſtons that cannot be expelled. When this is the caſe, let the cock be turned one quarter round to the left, in which ſituation it will make a paſſage through the duct *d b*, at the ſame time that it cuts off the communication between the barrel B and the receiver, and the little air under the valve, &c. in the barrel A will immediately run into the exhauſted barrel B through the duct *d b* and hollow wire *a a*. The pump A will now make a better vacuum, and take air again from the receiver, which will, when the piſton A deſcends, be transferred to the barrel B; and, finally, when the piſton B deſcends, the air will be expelled through the valve in its piſton. Thus, by only turning the cock one way or the other, the cock is made to exhauſt with one barrel or both. When it exhauſts with one only, the other is rendered ſubſidiary to it by clearing it of air after each ſtroke.

To render the pump more eaſy in working, there ſhould be a hole in the bottom of each barrel, furniſhed with a ſilk valve, through which the air would enter during the firſt ſtrokes of the pump. The ſituation of theſe holes are ſhewn by dotted lines in the figure. The air may be reſtored to the receiver by unſcrewing the ſtop ſcrew *h*.

Being foiled in all my attempts to procure a mercurial air pump, I turned my thoughts to the improvement of the common double barreled pump, which, I believe, was invented by Dr. Robert Hooke; and which appeared to me the moſt ſimple and convenient form, provided it could be made more accurate in its operation.

The reſult of ſome thought, and various experiments made to this purpoſe, was, that in theory I brought that pump to the perfection of rarefying permanent dry air 248000000 times, ſuppoſing the air to have a uniform power of expanſion to ſuch a degree, and the barrels of the pump to be twelve inches long by two in diameter. The drawings and deſcription of this pump are now in the hands of Mr. Nairne, who is making one on that conſtruction, particularly adapted to the performance of chemical experiments on air. I have been purpoſely ſhort in my deſcription of the ſcheme herewith ſent (which differs in many reſpects from that making by Mr. Nairne) as I purpoſe giving a full deſcription of the other and its performance (if I find it to equal thoſe hitherto contrived) when the inſtrument comes to my hands.

Query, does the lower valve of Mr. Cuthberton's air pump increaſe the power of that inſtrument, and would it not work equally well without it?

My reaſons for ſuſpecting the utility of this valve are the following:

Firſt, there muſt always be a quantity of air above the piſton, when drawn up, that cannot be expelled.

Secondly, this quantity of air will be uniformly the ſame, whether at the beginning or end of the exhauſtion, as it will take the ſame force to open the expulſion valve at one time as at another, and this air muſt be as denſe as that of the atmoſphere at leaſt.

Thirdly, when the piſton begins to deſcend, the large conical valve muſt thruſt out of its ſhell, before that at the bottom of the barrel can poſſibly ſhut, for the ſhutting of the one depends on the motion which cauſes the opening of the other.

Fourthly, when the piſton begins to aſcend, the valve at the bottom of the barrel muſt open before that in the piſton can ſhut, for the reaſon above ſtated.

Now let us ſuppoſe the piſton at the top of the barrel, when the winch is turned the large conical valve will be opened, before that in the bottom of the barrel can ſhut, at leaſt there will be an inſtant when both valves may be ſaid to be half open, the air above the piſton being much denſer than that below it, will take advantage of this opportunity to diffuſe itſelf equally through the barrel and receiver. If, however, the air does not all eſcape at the commencement of the down ſtroke, it will have another opportunity at the commencement of the up ſtroke, for then the bottom valve opens before that in the piſton can ſhut, and the air in the barrel will again be at liberty to exert its ſuperior ſpring to get back into the receiver.

It is with the greateſt diffidence I put this queſtion, having the higheſt reſpect for Mr. Cuthberton's abilities.

If I have advanced what is erroneous, or if I have miſtaken the principle of the inſtrument, I beg to be corrected and informed.

VI.

A Memoir, in which the Queſtion is examined, whether Azote be a ſimple or compound Body? By CHRISTOPHER GIRTANNER, *Doctor of Phyſic at Gottingen.*

(Concluded from page 171.)

FROM this experiment we ſee that pure hydrogen gas combines with the oxygen gas which had remained in the lungs, and forms azote gas.

16. Another experiment, which appears to me to confirm my opinion, was made by Mr. Henry of Mancheſter. He repeated it ſeveral times, and always with the ſame ſucceſs. His explanation of this phenomenon may be ſeen in Scherer's Journal, which is different from mine, becauſe he conſidered azote as a ſimple body. His experiment is as follows:—In a recurved tube over mercury a mixture was made of 94,5 meaſures of carbonated hydrogen gas from acetite of pot-aſh, and 107,5 meaſures of very pure oxygen gas obtained from the oxygenated muriate of pot-aſh. The mixture amounting to 202 meaſures, was reduced, by an electric exploſion paſſed through it, to 128,5 meaſures, and afterwards by lime water to 54,0 meaſures. So that in this experiment 23 meaſures of azote gas were produced by the electric exploſion of the oxygen united with the hydrogen.

17. Of all known bodies zinc is, if I am not deceived, that which moſt eaſily unites with oxygen; it takes it from almoſt every other body, and this property renders it highly uſeful for the exhibiting the minuteſt quantities of oxygen. It was more particularly by means of zinc that I ſucceeded in ſeparating the oxygen of the muriatic acid from its baſe. I have likewiſe uſed it to make the laſt analyſis of ammoniac. Filings of zinc are to be mixed in a retort, with concentrated fluid ammoniac. A communication being thus made between the retort and the pneumatic apparatus, the whole being kept in digeſtion for ſeveral days, taking care not to raiſe the heat too much, the ammoniac is decompoſed. The oxyde of zinc remains in the retort, and in the pneumatic apparatus a conſiderable quantity of hydrogen gas is obtained, together with a ſmall quantity of ammoniacal gas and of undecompoſed azote. It is eaſy to prove that the hydrogen gas was

not obtained from the water in which the ammoniac was diſſolved; becauſe the quantity of the gas is too large, and that of the azote too ſmall, to admit of any doubt reſpecting the decompoſition of the latter.

Such are the experiments, from which it appears to me, that we are juſtified in concluding, that azote is a compound of oxygen and hydrogen. I muſt refer to a ſecond memoir for ſeveral other experiments no leſs deciſive, which require to be repeated before I ſubmit them to the examination of thoſe accurate and enlightened chemiſts, to whom this memoir is addreſſed.

Azote being, therefore, if I am not deceived, a body compoſed of hydrogen and oxygen, it follows:

That the atmoſphere is not, as has been hitherto ſuppoſed, a mixture of oxygen and azote gaſes, but rather a mixture of oxygen and hydrogen-gaſes; water in form of gas, if I may be permitted to uſe this expreſſion.

When, by thoſe chemical experiments which have been improperly called eudiometrical, the oxygen is ſeparated from the hydrogen, this ſeparation can never be totally or completely made. Part of the oxygen remains united to the hydrogen, and forms that chemical combination which we call azote, and obtain in theſe experiments. The oxygen, ſo indiſpenſible for ſupporting the life of organized beings, is changed by its combination into hydrogen, into azote, which is not only incapable of ſupporting life, but is a true poiſon, in conſequence of the affinity it has with oxygen, and the avidity with which it takes it from organized bodies.

Oxygen has ſo ſtrong an affinity with hydrogen when both are mixed in the atmoſphere, that it is very difficult to ſeparate them entirely, and hence the great difficulty of analyſing azote. When charcoal, ſulphur, a lighted candle, or the metals, have ceaſed to burn in atmoſpheric air, when animals have periſhed in a given quantity of this fluid, the remainder ſtill contains a notable proportion of oxygen. Phoſphorus burns in this reſidue very well, and for a conſiderable time: and even when the phoſphorus has ceaſed to burn, there always remains a ſmall quantity of oxygen united to hydrogen, that is to ſay, there is a remainder of azote. We may, nevertheleſs, as I have ſeveral times obſerved, deprive the atmoſpheric air of almoſt the whole of its oxygen, and render the analyſis nearly complete by heating phoſphorus in it for a certain time. Phoſphorated hydrogen gas is then obtained by the change of part of the azote into hydrogen.

From the experiments here recited, it follows, that the eudiometer, ſuch as it exiſts at preſent, is founded on erroneous principles, the azote obtained in theſe experiments being always a product of the operation, which did not exiſt before in the form of azote previous to the examination. Von Humboldt, who is much diſpoſed to draw general concluſions from inſulated facts, appears to have been deceived when he advances that the earths may be uſed to determine the quantity of azote contained in atmoſpheric air. The earths do not point out the azotic part; but change a portion into azote.

Cultivated earth, brought into contact with the atmoſphere, abſorbs the oxygen, and forms azote. In countries ſituated near the poles, and on the top of the Alps, where the earth is always covered with ſnow, the atmoſpheric air contains a greater quantity of oxygen than in ſouthern or low countries, becauſe the ſnow prevents the air from coming into contact with the earth.

The conſtituent principles of azote being now known, we are enabled to form a new theory of the art of making nitre. In the Annals of Chemiſtry, Vol. XX. I obſerve ſome valuable obſervations on this ſubject. For inſtance, p. 313, " Earth extracted from deep ſubterraneous " places, inacceſſible to the light, ſuch as caverns, requires only to be expoſed to the air " a few days to produce ſalt-petre in abundance. It is proper to obſerve, that theſe earths " do not furniſh a ſingle atom of nitre when they are firſt taken from the moiſt and dark " place where they were formed; and that it is only by combination or combuſtion of the " azote of the earth with the oxygen of the atmoſphere that this ſalt is produced."

The explanation which is given, page 314, of that intereſting phenomenon, is a proof how diſtant we were from the truth before the nature of azote was underſtood.

As lime abſorbes oxygen with avidity, we ſee the cauſe why rooms that have been newly white-waſhed are unwholeſome.

The following bodies are combinations of hydrogen with oxygen in different proportions:

Azote.
Gaſeous oxyde of azote.
Nitrous gas.
Nitrous acid.
Nitric acid.
Muriatic acid.
Oxygenated muriatic acid.
Nitro-muriatic acid.
Water.
Atmoſpheric air.
Ammoniac.

I hope ſoon to prove that pot-aſh, ſoda, and ſulphur, ought to be added to that liſt. Phoſphorus appears to me to be hydrogen, in the pureſt ſtate in which we are acquainted with it; but this I am very far from having it in my power to prove. The analyſis of fixed alkalis is completed. As, however, I have no intention of ſaying any thing upon that ſubject in the preſent article, I ſhall confine myſelf to pointing out one of the deciſive experiments. When an alkali is fuſed with ſiliceous earth to make glaſs, the alkali is analyſed. The hydrogen eſcapes under the form of gas, and the oxygen becomes combined with the ſiliceous earth; glaſs being merely an oxyde of ſilex. Too great a quantity of oxygen ſhould not be admitted, as it renders it leſs tranſparent; for this reaſon it is uſual to add ſubſtances which ſtrongly attract oxygen, ſuch as mangancſe.

Mr. Mayer, to whom I had communicated the reſult of my experiments, by which I ſo evidently confirmed an idea which he had ventured to throw out, and of which he has the merit of being the author, ſent me a paper, from which I ſhall give an extract:

" I am inclined," ſays he, " to think with Mr. De Luc, that the evaporation of water, " as it continually takes place on a large ſcale in nature, is a real converſion of the water

“ into air. It is true, that we have not been able in our laboratories* to accompliſh the changing of water into evaporation. This ariſes from our being ignorant of the ſhare which electricity and light have in this natural proceſs. Nevertheleſs, it appears probable to me, that the ponderable parts of atmoſpheric air, that is to ſay, oxygen and azote has no other ſource than the water with which the ſurface of the globe is covered. The very ſmall quantity of oxygenous gas which is exhaled by vegetables by means of the ſun, is far from being able to repair the enormous conſumption of that principle which daily takes place in our atmoſphere in ſo many different ways. Now, ſuppoſing, according to this theory, that 100 grains of water are changed, by the unknown proceſs of nature, into 100 grains of atmoſpheric air, that is to ſay, into a chemical mixture of oxygen and hydrogen, we can find, by a very ſimple calculation, in what proportion oxygen and hydrogen are combined to form the azote which we obſerve in the atmoſphere. Let us not, however, forget that the nature of what we call azote is not always the ſame, and that it varies conſiderably in the proportions of its conſtituent principles†.

“ I ſhall call water E, oxygen O, hydrogen H, the ponderable part of the atmoſpheric air L, the ponderable part of azote gas S.

“ Then $100\,E = 85\,O + 15\,H$.

“ But a cubic inch of atmoſpherical air is compoſed of $\frac{1}{4}$ of a cubic inch of gas, and $\frac{3}{4}$ of a cubic inch of azote gas; therefore

$$1\,L = \tfrac{1}{4}\,O + \tfrac{3}{4}\,S.$$

“ One inch of atmoſpherical air weighs 0:46 grains.

“ One inch of oxygenous gas weighs 0.51 grains.

“ One cubic inch of azote gas weighs 0.44 grains; conſequently

$$\begin{aligned} 0.46\,L &= \tfrac{0.51}{4}\,O + \tfrac{3}{4}0.44\,S. \\ 1.84\,L &= 0.51\,O + 1.32\,S. \\ 184\,L &= 51\,O + 132\,S. \\ 100\,L &= 27.8\,O + 72.2\,S. \end{aligned}$$

“ Azote gas being a compound of oxygen and hydrogen.

$$S = x\,O + y\,H \text{; ſo that}$$

$$100\,L = (27.8 + 72.2\,x)\,O + 2\,y\,H$$

“ Now nature changes 100 grains of water into an hundred grains of atmoſpherical air; conſequently

$$\begin{aligned} 100\,E &= 100\,L\text{; whence} \\ 27.8 + 72.2\,x &= 85\text{, and} \\ 72.27 &= 15\text{;} \end{aligned}$$

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* See, however, Prieſtley, in Philoſ. Journal, IV. 193, and elſewhere.---N.

† This is a very important point upon which Mr. Mayer fixes our attention. It will be neceſſary to make a great number of very exact experiments to determine the different degrees of the oxydation of hydrogen, and to diſtinguiſh the different gaſes comprehended, till now, under the general name of azote gas.

Then $x = 0.79$, and $y = 0.21$ nearly.

$$S = 0.79\,O + 0.21\,H, \text{ or } 100\,S = 79\,O + 21\,H.$$

" That is to ſay, 100 grains of azote gas are compoſed of 79 grains of oxygen and 21 " grains of hydrogen."

Having applied this ingenious calculation of Mr. Mayer to other oxydes of hydrogen, I found as follows:

Let N denote the nitric acid: we know by experiments that

$$100\,N = 79.5\,O + 20.5\,S.$$

Inſtead of S, let us put its value, as found by the preceding calculation, and we ſhall have

$$100\,N = 79.5\,O + 20.5\,(0.79\,O + 0.21\,H) = 95.7\,O + 4.3\,H \text{ nearly}.$$

Thus 100 grains of nitric acid contain nearly 96 grains of oxygen, and 4 grains of hydrogen.

The following are the reſults of the ſame calculation:

100 parts of ammoniac = 63.72 oxyg. + 36.28 hydrog.
100 parts of azote = 79.00 oxyg. + 21.00 hydrog.
100 parts of atmoſph. air 84.67, oxyg. + 15.33 hydrog.
100 parts of water = 85.66 oxyg. + 14.34 hydrog.
100 parts gaſeous oxyde of azote = 86.77 oxyg. + 13.23 hydrog.
100 parts of nitrous gas = 93.28 oxyg. + 6.72 hydrog.
100 parts of nitrous acid = 94.33 oxyg. + 5.67 hydrog.
100 parts of nitric acid = 95.70 oxyg. + 4.30 hydrog.

Are then oxygen and hydrogen the two elements of which all the bodies in nature are formed. It appears to me very probable. It appears alſo probable that the heavier a body is the more oxygen concentrated and deprived of caloric it contains, and that, on the contrary, the more hydrogen a body contains, the lighter it muſt be. But theſe are only hypotheſes which future experiments will either confirm or replete.

It ſeldoms happens that the two conſtituent principles of azote enter, as ſimple bodies, into other combinations; they much oftener form one ſingle ingredient by their union in different proportions. Coal, or the oxyde of diamond, is found in many bodies, and the diamond itſelf in none with which we are acquainted. We obtain coal in our chemical decompoſitions, and never the diamond. We do not even know ſuch a thing as diamantic or adamantic acid, although carbonic acid is familiar to us. No chemiſt would think of affirming that we exhale diamond in the act of reſpiration, but that we exhale a quantity of coal. The diamond itſelf is not, perhaps, a ſimple body; it probably contains oxygen; ſince all tranſparent bodies, if I miſtake not, contain more or leſs of it. Azote, like coal, is ſuſceptible of various combinations merely as azote; as a compounded principle, which gives to bodies properties very different from what is given to them by the combination of the two ſimple principles of which azote is compoſed. Azote, as a conſtituent principle of bodies, is not hydrogen and oxygen, but azote: exactly as ſoap is not oil and aklali, but ſoap. We muſt never loſe ſight of this.

I have obſerved, in ſeveral experiments, that ſulphur frequently aſſumes a yellow, yellowiſh, or orange colour. We are ſeldom deceived in ſuppoſing that there is ſulphur in bodies which exhibit that colour. Thoſe bodies which contain coal are of a black, brown, or a violet colour. Azote diſcovers itſelf by a green colour. Plants (etiolées) which have vegetated, defended from the air and light, are white, and contain very little azote. Expoſe them to theſe two agents for a few days, they aſſume a green colour, and from that time the proportion of azote in their compoſition becomes much more conſiderable. When nitrate of pot-aſh is fuſed in a glaſs retort, and the oxygen gas is ſeparated, it is obſervable at the end of the operation, whilſt the azote is forming, that is to ſay, when the oxygen has a greater affinity to hydrogen than to caloric, that the green colour appears, and the ſeparation of the oxygen gas entirely ceaſes.

But what demands the attention of every philoſopher is, that when water is expoſed to the ſun, the light decompoſes it, and ſeparates the oxygen in large quantities. The hydrogen retains the laſt particles of the oxygen; azote is formed, and announces itſelf by a green colour, More and more of the water is decompoſed; the oxygen (which I have already ſhewn to be the principle of life and irritability in organized nature) becomes more fixed; and this azote, produced from the effect which the ſun has upon water, becomes an organized body, the conſerva fontinalis, a plant which lives, grows, and perpetuates its ſpecies. Here are the limits at which philoſophy muſt ſtop—to admire and reſpect the ſecrets of nature, without knowing if we ſhall ever be permitted to penetrate them. It is certain, and I have ſatisfied myſelf reſpecting it by a great number of experiments in which I have been engaged every ſummer for the ſpace of more than twelve years, that the influence of the ſolar light is abſolutely neceſſary for changing the water into a plant, or organized azote. No degree of heat can ſupply the place of it, and this experiment alone ought to be ſufficient to convince every unprejudiced reaſoner that heat and light are two ſubſtances totally different. I am ſurprized that this green matter of Prieſtley, this fontinalis, to which I have given the name of organized azote, has not engaged more of the attention of chemiſts. It is the moſt wonderful ſubſtance which exiſts, the moſt ſingular body in nature. Nothing can be more abſurd than what Prieſtley has ſaid concerning it. To reaſon as he has done, is, indeed, not to reaſon at all. This great man, whoſe name will live as long as the ſciences ſhall be cultivated, has made the moſt eminent diſcoveries. I admire his talents, but I am ſorry to obſerve, that in all his productions he appears to be more of an experimentaliſt than a philoſopher. Whilſt with one hand he unfolds to us the aſtoniſhing ſecrets of nature; with the other he is always ready to ſhut our eyes in caſe we have a deſire to penetrate further than he wiſhes us to ſee. In the diſpute which he maintained with Dr. Ingenhouſz, concerning the ſubject of the green matter about which we are ſpeaking, he has given a ſtriking inſtance of his diſinclination to permit the wonders of nature to be beheld, except through his eccleſiaſtical perſpective. Ingenhouſz, an enlightened chemiſt, having deſcribed ſeveral experiments, worthy the attention of every thinking being, relating to this ſingular ſubſtance, adds,

" Water itſelf, or ſome ſubſtance in water, is, in my opinion, changed into this ſpecies of " vegetation. It is a real tranſmutation, which may appear incomprehenſible to a philoſopher, but which, in reality, is not more extraordinary than the transformation of " graſs and other vegetables into fat in thoſe animals which feed upon plants, or than that " of the aqueous juice of the olive into oil."

Water is changed into a plant. This is the fact. Ingenhouſz ſtops there, and ſays, I do not comprehend it; this is the language of a philoſopher. Prieſtley, on the contrary, is ſcandalized at ſuch language. He aſks Ingenhouſz if he is not aſhamed to wiſh to re-eſtabliſh the long refuted doctrine of ſpontaneous generation [*].

He ſpeaks to him nearly in the language of the inquiſitor to Galileo, when he proved to that immortal man that the ſun turns round the earth. Prieſtley afterwards gives us his own theory of the production of the organized azote. He ſays, that the ſeeds of this plant float every where in the air, over the earth, the ſea, the Alps, the low ground, at the poles, and at the equator, in ſummer, in winter, and in every ſeaſon, and that they are received into water where they germinate. But organized azote is produced in bottles well ſtopped. Dr. Ingenhouſz has even proved that, if a bottle be filled with well water, and reverſed over a baſin filled with water, a great quantity of organized azote will be formed. Prieſtley, who could not, without attributing a ſuperior intelligence to the pretended ſeeds of this plant, maintain that they came through the water purpoſely to fix themſelves into this bottle which was prepared for them, extricates himſelf by making them paſs through by imperceptible cracks [*]. Such are the means which are always uſed when we will not ſee that which is too obvious. Thus it was that phlogiſton was made to paſs through crucibles, and azote through tubes and retorts.

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[*] Dr. Prieſtley ſays, that the theory of ſpontaneous generation is a doctrine long ſince refuted. Theſe, are his words:---" Conſidering how long the doctrine of equivocal, or ſpontaneous generation has been " exploded." A philoſopher ſhould never make uſe of ſuch an expreſſion. There is not an exploded opinion which we may not recur to and re-examine. The philoſopher acknowledges no authority which prohibits him from admitting any opinion, or forbids him to examine it. There are many other opinions long ſince exploded to which we muſt now have recourſe. That, for inſtance, of the tranſmutation of metals. What chemiſt, of the preſent day, would preſume to deny the poſſibility of it. The change of one metal into another ſhould appear leſs difficult than the changing of the ſweeteſt body (ſugar) into the soureſt body (the oxalic acid); than the change of the hardeſt body (the diamond) into the ſofteſt body, the carbonic acid gas; or than the changing the moſt tranſparent body, the diamond, into coal, the moſt opaque of all bodies. In the nineteenth century, the tranſmutation of metals will be generally known and practiſed. Every chemiſt, every artiſt, will make gold. The utenſils for our kitchens will be made of ſilver, or even of gold, and this will contribute, more than any thing elſe, to prolong life, which has hitherto been poiſoned by the oxyde of copper, lead, and iron, which we daily ſwallow at our meals. There will then be no other wealth than natural wealth, the production of the ſoil. Artificial wealth, ſuch as gold, ſilver, and paper, will be extinguiſhed in the hands of thoſe who ſhall have accumulated them. What a revolution in ſociety! But every enlightened chemiſt will agree with me, that this revolution is not only probable, but near.---Note of the author.

Though I have no remarks to offer on this laſt aſſertion of our author, I cannot forbear remarking that gold and ſilver, though much uſed as ſigns of wealth, or tickets of commercial transfer, have always, and more eſpecially in modern times, conſtituted a very ſmall portion of national wealth. If gold could be rendered as cheap as iron, the effect upon the wealth of ſociety would be incomparably leſs than has reſulted from either of the inventions of the ſtocking loom, the ſpining gear, or the ſteam engine.---N.

I muſt obſerve, that in order to ſucceed in theſe experiments, it is abſolutely neceſſary that the water which is uſed ſhould contain gas in a ſtate of diſſolution, whether it be oxygenous gas, or carbonic acid gas. The more gas it contains, the greater quantity of organized azote will be formed.

It is to be wiſhed that the change, which earths undergo from the oxygen which they abſorb in decompoſing the vapours of water, were attentively inveſtigated. I do not doubt but that ſuch an inveſtigation would lead to ſome very important diſcoveries.

I ſubmit theſe ideas to the enlightened criticiſms of thoſe illuſtrious French chemiſts, the editors of the Annales de Chimie, the fathers and creators of this ſcience. Before their time, chemiſtry was only a crude maſs of facts, ill arranged, and worſe explained.

VII.

An Account of ſome Experiments made with the Galvanic Apparatus of Signor Volta. By Mr. DAVY, *Superintendant of the Pneumatic Inſtitution. Communicated by the Author.*

IN purſuing a courſe of experiments on the galvanic influence I have made ſome new obſervations. They are connected with the curious facts already detailed in Mr. Nicholſon's Philoſophical Journal, and they may poſſibly lead to elucidations of the phænomena.

The apparatus that I employed was conſtructed for Dr. Beddoes, and never conſiſted of leſs than 110 pairs of metallic plates. I found the ſenſible galvanic ſhock very much increaſed when the parts communicating with the conductors were moiſtened with ſolution of green ſulphate of iron. A pile erected with pieces of cloth, wetted in that ſubſtance, acted rather more intenſely than a ſimilar pile erected in the uſual mode. It, however, loſt its powers in a ſhorter time: perhaps a ſolution of ſulphate of zinc may be employed with advantage.

a. Struck with the curious phænomena noticed by Meſſrs. Nicholſon and Carliſle [†], namely, the apparent ſeparate production of oxygen and hydrogen from different wires, or from different parts of the water compleating the galvanic circle, my firſt reſearches were directed towards aſcertaining if oxygen and hydrogen could be ſeparately produced from quantities of water not immediately in contact with each other:

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[*] "Through ſome unperceived fracture," Vol. I. p. 294. "The ſeeds of this plant inſinuate themſelves into veſſels of water, through the ſmalleſt apertures," p. 308.

[†] Philoſ. Journal, page 183, Vol. IV.

Two ſilver wires, one from the zinc end of the apparatus, and the other from the ſilver end, were made to communicate with two glaſſes, diſtant from each other about five inches, and filled with water that had been long boiled, and was yet warm. Into one of theſe glaſſes I dipped the fingers of my right hand, and into the other the fingers of my left, ſo that the communication between them was made through my body. Immediately after the ſhock, the zinc wire[*] began to calcine very faſt, white clouds diffuſing themſelves from it through the water. At the ſame time gas was formed round, and extricated from the ſilver wire in the other glaſs. The communication was kept up for half an hour; during this time no gas was produced from the zinc wire, which continued to calcine throughout the proceſs. The gas from the ſilver wire was caught in a ſmall inverted cylinder; examined by the teſt of nitrous gas, it appeared to contain no oxygen, and inflamed with twice its bulk of common air, gave ſuch a diminution, as denoted it to be hydrogen nearly pure.

This apparatus being adjuſted as before, the communication between the glaſſes was made through three perſons; the proceſs went on, though leſs rapidly; the oxygen was fixed as before by the ſilver in one veſſel, whilſt the hydrogen was given out in the other. When muſcular fibre, living vegetable fibre, or a moiſtened thread not exceeding three feet in length, was employed as the medium of connection between the glaſſes, ſimilar effects were produced, though the gas was evolved more ſlowly than when the living animal was the connector. Muſcular fibre appeared to be a better conductor than vegetable fibre, and vegetable fibre a better conductor than the moiſtened thread.

b. Several glaſs tubes about $\frac{1}{3}$ of an inch in diameter, and four inches long, having each a piece of gold wire inſerted into one end hermetically ſealed, and the other end open, were provided. Two of theſe tubes were filled with diſtilled water, and inſerted into ſeparate glaſſes filled with that fluid. The glaſſes were made to communicate by means of freſh muſcular fibre; the gold wires were connected with the machine by means of ſilver wires; one with the ſilver end, the other with the zinc end. Gas was immediately given out from both the gold wires; but moſt rapidly from that connected with the ſilver. In four hours and a quarter the proceſs finiſhed; the water in the tube communicating with the ſilver being below the gold wire. The gaſes were examined; the quantity from the water communicating with the zinc was equal to 33 grain meaſures, that from the water connected with the ſilver to nearly 65 grain meaſures. The zinc gas was mingled with 80 meaſures of nitrous gas, containing $\frac{1}{22}$ nitrogen; a rapid diminution took place, and when the reſiduum was expoſed to ſolution of green muriate of iron [†], not quite five meaſures remained. Hence the 33 meaſures of gas evidently contained more than 31 meaſures of oxygen. The 61 meaſures in the other tube, gave with nitrous gas a diminution but barely perceptible, and fired after the abſorption of the nitrous gas by the electric ſpark, with 60 meaſures of oxygen, left a reſiduum nearly equal to 36 meaſures; hence the gas was hydrogen almoſt pure.

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[*] To prevent unneceſſary repetitions, after Mr. Cruickſhank, I have called the wire connected with the zinc end of the apparatus, the zinc wire, and that connected with the ſilver end, the ſilver wire.

[†] Solution of green muriate of iron rapidly abſorbs nitrous gas, without effecting any change in it at common temperatures, and it is poſſeſſed of no action on gaſes not abſorbable by water, and incapable of ſupporting flame. Solution of green ſulphate of iron likewiſe abſorbs nitrous gas without decompoſing it. See *Reſearch. Chem. & Phil. concerning Nitrous Oxide, page* 179. *Johnſon.*

c. There was every reaſon to ſuppoſe, that the ſlight diminution produced by the mixture of the hydrogen with nitrous gas in the laſt experiment, as well as the reſidual gas of the oxygen, were owing to common air held in ſolution by the diſtilled water, and given out from it during the proceſs. To aſcertain if the gaſes could be obtained perfectly pure, when water deprived of its looſely combined air by boiling was employed. The two tubes were filled with water that had been boiled for more than eight hours, and that was yet ſo hot as to be painful to the fingers; the glaſſes were filled with water of the ſame kind, and the proceſs conducted as before; the tubes being ſuffered to cool before the communication was made, gas was given out very rapidly from the water connected with the ſilver; but very ſlowly from that connected with the zinc. During the whole of the proceſs, no globules of air formed on the ſides of the tubes, as in the laſt experiment. In five hours the tube connected with the ſilver contained 56 grain meaſures of gas. That connected with the zinc contained only 14 meaſures. The 56 meaſures gave no diminution with nitrous gas, and appeared by the teſt of detonation, to be pure hydrogen. The 14 meaſures tried by the teſts mentioned in the laſt experiment, appeared to be oxygen, mingled with no perceptible quantity of other gas. In this experiment, as in the laſt, the gold wires were not apparently acted upon, nor was their color in the ſlighteſt degree altered; the deficient proportion of oxygen, there was every reaſon to believe, was owing to the abſorption of that gas in the naſcent ſtate by the boiled water. Boiled water was now expoſed to, and agitated in oxygen over mercury, till it was judged to be ſaturated with that gas. The tube connected with the zinc was filled with this water; the other tube was filled with common boiled water. The galvanic proceſs was continued ſeven hours. In this time the water connected with the zinc had given out 27 grain meaſures of oxygen, apparently pure; from the water connected with the ſilver, 57 meaſures of hydrogen had been extricated.

d. Having thus aſcertained that oxygen and hydrogen, nearly in the proportions required to form water, could be ſeparately produced from quantities of water, having no communication with each other, except by means of the dry metallic conductors and muſcular fibre. I next endeavoured to aſcertain, if the contact of the metallic wires, with the metallic plates of the apparatus, were eſſential to the effect. The conducting ends, *i. e.* the ſilver and zinc, were made to communicate with two glaſſes of water, by means of two unconnected pieces of muſcular fibre. A piece of ſilver wire was made the medium of connection between the glaſſes. Immediately after the connection, I was ſurprized by ſeeing that end of the wire in the water communicating with the ſilver calcining; whilſt gas was given out from that part of it in the veſſel communicating with the zinc; as was the caſe in the broken circuit deſcribed by Mr. Nicholſon. When the tubes with gold wires, connected by ſilver wires, were employed, oxygen was given out in the water connected with the ſilver, and hydrogen in that communicating with the zinc. In none of theſe experiments could any production of gas from the muſcular fibre be perceived; but the parts expoſed to the water became whiter than before.

When (the glaſſes being connected with the apparatus by muſcular fibre) the communication between them was made through my body by means of the gold wires in the tubes; one being in contact externally with my right hand, the other with my left, oxygen was produced as before in the glaſs connected with the ſilver, and hydrogen in that connected with the zinc. When I made the communication, holding a ſilver wire partly plunged into the water connected with the ſilver, in my right hand, the fingers of my left hand being in the other glaſs, the ſilver wire became ſlowly oxydated, and no gas was perceptibly given out in either of the glaſſes. When on the contrary, I introduced my hand into the ſilver glaſs, and the wire into the zinc glaſs, gas was given out round the wire, no oxydation took place, and no gas was extricated in the ſilver glaſs.

When the glaſſes were made to communicate both with the machine, and with each other, by means of muſcular or vegetable fibre; and metallic wires introduced into either or both of the glaſſes, and wholly or partially covered with water, no gas was given out from them, and no apparent chemical change took place.

Reaſoning on this ſeparate production of oxygen and hydrogen, from different quantities of water, and on the experiments of Mr. Henry, junr. on the action of galvanic electricity on different compound bodies *, I was led to ſuppoſe, that the conſtituent parts of ſuch bodies (ſuppoſing them immediately decompoſable by the galvanic influence) might be ſeparately extricated from the wires, and in conſequence obtained diſtinct from each other.

a. I filled two of the ſmall tubes mentioned in *b.* 1. with ſtrong ſolution of cauſtic pot-aſh, and inverted each of them in a glaſs filled with the ſame ſubſtance; the glaſſes were made to communicate with each other by means of muſcular fibre, and the gold wires in the tubes connected with the ends of the pile. Gas was produced much more rapidly in this proceſs from both wires, than in the experiment with ſimple water. In three hours no depoſition had taken place in either of the glaſſes, nor were the gold wires ſenſibly acted upon. The gas given out in the tube connected with the zinc, meaſured exactly 37 grain meaſures, and proved to be oxygen abſolutely pure, for with 80 meaſures of nitrous gas, containing about $\frac{1}{36}$ nitrogen, it diminiſhed to leſs than 3 meaſures. The gas given out in the tube connected with the ſilver, was equal to rather more than 72 grain meaſures. It gave no diminution with nitrous gas, and two 20 grain meaſures of it, fired with rather more than one 20 grain meaſure of oxygen, containing about ,06 nitrogen, left a globule of air hardly perceptible.

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* Phil. Journ. Vol. IV. p. 224.

Surprized at theſe reſults, which proved that no decompoſition of pot-aſh had taken place, and that that ſubſtance in this mode of operating, only enabled the galvanic influence to extricate oxygen and hydrogen more rapidly from water, I was induced to operate upon this ſubſtance in the way of direct communication. Two gold wires were paſſed through holes in the ſide of a ſmall glaſs tube cloſed at one end, and cemented ſo as to be diſtant about the eighth of an inch. This tube was filled with ſolution of pot-aſh, and inverted in glaſs filled with the ſame ſubſtance; the gold wires were made to communicate with the ends of the pile. Gas was produced rapidly from both wires, but moſt from the ſilver wire; the gold was not acted upon, and no depoſition took place. When near a quarter of a cubic inch of gas had been collected, it was transferred to a detonating tube, and fired by the electric ſhock over mercury, it gave a vivid inflammation, and left a globule of air not equal to $\frac{1}{80}$ of the whole quantity.

b. Solutions of cauſtic ammoniac were expoſed to the galvanic influence in the two tubes with gold wire, and connected by muſcular fibre. Gas was given out very ſlowly in the tube connected with the zinc, and the gold wire was evidently acted upon, being in ſome parts corroded, and in other parts covered by a yellow depoſit. In the tube connected with the ſilver, gas was given out more rapidly, and the gold was not altered in appearance. In five hours the gas in the zinc tube was equal to 5 meaſures, and proved to be a mixture of nearly 3 oxygen, and 2 of nitrogen. The gas in the ſilver tube meaſured 31 grains, and appeared to be hydrogen, mingled with a minute quantity of nitrogen. I repeated this experiment ſeveral times, to aſcertain if after the ſolution of ammonia had been long galvanized, the proportion between the gaſes would be different. The gas given out in the zinc tube was always to that in the ſilver tube nearly as 1 to 6, but the quantity of oxygen appeared to increaſe towards the end of the proceſs. A quantity of ſolution of ammoniac, the ſame as that uſed in the experiment expoſed to heat in a mode deſcribed in *Reſearch. Chem. & Phil.* page 241, readily produced $\frac{1}{20}$ of its bulk of unabſorbable gas, which gave no diminution with nitrous gas, and appeared to be nitrogen. In every experiment yellow depoſit was formed upon the gold in the zinc tube; muriatic acid poured upon ſome of this yellow depoſit ſlowly diſſolved it, a little nitrogen being given out during the ſolution.

When a ſolution of cauſtic ammoniac was expoſed in the ſilver tube, and water in the zinc tube, the gold wires were not perceptibly altered, and oxygen was given out in the zinc tube and hydrogen in the ſilver tube, nearly in the proportions required to produce water. When on the contrary water was connected with the ſilver, and ſolution of ammoniac with the zinc, hydrogen was produced from the water; the zinc gold wire was corroded, and the mixture of oxygen and nitrogen to the hydrogen, as ſix to one, (one to ſix?) was produced as before.

c. Concentrated ſulphuric acid was galvaniſed in the double tubes connected by muſcular fibre. The gold wire in the zinc tube was not diſſolved or corroded, and in a great length of time 41 meaſures of gas, which proved to be pure oxygen, were given out from it. But little gas was given out in the ſilver tube, the acid in it was clouded with a white ſubſtance, which was evidently ſulphur; it was perpetually produced round the point of the wire. 15 meaſures of gas were evolved, which gave rather greater diminution, fired with oxygen, than pure hydrogen; hence they might poſſibly have been partially ſulphurated hydrogen.

When ſolution of cauſtic pot-aſh was put into the ſilver tube, and ſulphuric acid into the zinc tube, pure hydrogen was diſengaged in the ſilver tube, and pure oxygen in the zinc tube; the ſame phenomena took place when water was employed inſtead of ſolution of pot-aſh.

When water was connected with the zinc, and ſulphuric acid with the ſilver, the products were the ſame as when pure ſulphuric acid was uſed in both tubes. When very diluted ſulphuric acid was employed in both tubes, oxygen and hydrogen, nearly in the proportions required to form water, were ſeparately evolved.

d. The tubes were filled with pure ſolution of muriatic acid, and the communication made as before. No gas was given out in the zinc tube, and the gold in it was very much corroded; 30 meaſures of pure hydrogen were collected in the ſilver tube, the gold in which was not perceptibly acted upon. When water was made to communicate with the zinc, and muriatic acid with the ſilver, neither of the gold wires were acted upon, and 22 meaſures of oxygen were collected from the water, and 41 of hydrogen from the muriatic acid. When water was connected with the ſilver, and muriatic acid with the zinc, the ſame phænomena took place, as when pure muriatic acid was uſed in both tubes.

e. Concentrated ſolutions of nitric acid were galvaniſed in the tubes, 19 meaſures of pure oxygen were produced in the zinc tube. A globule of gas only, not equal to half a meaſure, was produced in the ſilver tube, and the acid became green on the top. The globule of gas did not diminiſh with oxygen, it was too ſmall to be tried by other teſts. The gold wires were not acted upon.

When nitric acid was connected with the zinc, and water with the ſilver, oxygen and hydrogen were ſeparately produced. When water was connected with the zinc, and nitric acid with the ſilver, oxygen was given out from the water; no gas was given out from the acid, and it became green.

In the experiments on the ſulphuric and nitric acids, it is moſt probable, that the acids were decompoſed by the naſcent hydrogen in the ſilver tube. In the experiments on the muriatic acid and ammoniac, the deficiency of the oxygen in the gold tube moſt probably, partly aroſe from the oxydation of the gold, in conſequence of what may be called prediſpoſing affinity. In theſe proceſſes, none of the compound bodies appear to have been immediately decompoſed by the galvanic influence.

The difference between my reſults and thoſe of Mr. Henry, may be accounted for from the difference between our modes of operating; I ſuſpect, however, that on repeating his experiment on the ſolution of pot-aſh under new circumſtances, that ingenious chemiſt will find reaſons for altering his concluſions with regard to the decompoſition of the alkali.

Judging from the rapidity with which the gaſes were extricated, ſolution of pot-aſh would ſeem to be a better conductor than water, water a better conductor than ſolution of ammoniac, and ſolution of ammoniac, better than either of the three mineral acids. Poſſibly phoſphoric acid, and other acids, may be decompoſed when expoſed to naſcent hydrogen, produced under the galvanic influence. If the ratio between the quantities of the oxygen and hydrogen produced from the different wires, be always the ſame, whatever ſubſtances are held in ſolution by the water connected with them, this naſcent hydrogen will become a powerful and accurate inſtrument of analyſis.

VIII.

Deſcription of the Machine for kneading Dough, which is made Uſe of in the Public Bake-Houſes of Genoa; from a Model preſented to the Patriotic Society of Milan *.

THE drawings in Plate XII. repreſent the machine for kneading; the parts being regularly numbered. 1. A wooden frame upon which the gudgeon of the arbor of the wheel is ſupported. 2. A ſtrong wall of three and a half palms † thick, through which the arbor paſſes. 3. Another wall of the ſame kind at the diſtance of 21 palms. 4. The arbor, 30 palms long, and $1\frac{1}{3}$ palms thick. 5. A large wheel turning on the arbor. Its diameter is 28 palms, and its width for admitting two men is 5 pounds. 6. Steps within the wheel to afford foot hole for the men. They are $\frac{1}{3}$ of a palm high, and 2 palms aſunder. 7. A concrete cog wheel near the end of the arbor, and of the diameter $12\frac{1}{2}$ palms. 8. A bar or beam of wood parallel to the arbor, proceeding from one wall to the other, 21 palms in length, and $1\frac{1}{3}$ in thickneſs. 9. Another beam of the ſame dimenſions not viſible, becauſe on the oppoſite ſide of the arbor; but its relative ſituation may be ſeen in the ſhadow upon the wall. 10. A croſs piece near the wall 3, which connects the two pieces 8 together, and ſupports the extremity of the arbor. Its length is 14 palms, and its thickneſs $1\frac{1}{3}$. 11. Another croſs piece of the ſame deſcription near the firſt wall, but not viſible on account of the interpoſition of that wall. 12. A ſtrong curved piece attached to the pieces 8, 9. It receives the pivot of the upright arbor, which carries the lantern pinion and kneaders. Its clear length is 14 palms, and its thickneſs $1\frac{1}{2}$ palms. 13. The lantern pinion, in diameter $5\frac{1}{2}$ palms, and height $1\frac{1}{4}$. 14. The arbor to which is affixed a ſtrong croſs of wood. 15. The croſs formed of two bars of wood unequally divided, in ſuch a manner as that the four branches are of unequal lengths. They are 1 palm wide, and $\frac{7}{12}$ of a palm thick. 16. Four wooden pieces or kneaders of a triangular form, fixed

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* From the Italian of the *Atti della Societa Patriotica di Milano, Vol. II.*

† The Genoeſe palm, which is ſtated by Dodſon, in his Calculator, at 9,780, Paris ſq. inches of our meaſure.

in each branch of the croſs in its lower face, ſo that they revolve in the trough parallel to the arbor, with one of their edges foremoſt. They are 1¾ palms long, and ½ a palm thick.
17. A circular trough, or barrel, made very ſtrong, and well hooped with iron. Its diameter is 6 palms, its clear height 1½ palms, and the thickneſs of the wood ¼ of a palm.

The leaven or mixture required to be kneaded, is made in a wooden trough 4 palms long, and 3 palms by the ſide in which it is carried to the kneading mill.

The action of this mill is eaſily underſtood. When the men walk in the large wheel, they turn the kneaders which divide and agitate the paſte in the trough, and perform the ſame effect as is commonly produced by human hands. The trough contains 18 rubbi (   ) of flour, which being properly mixed and raiſed, is carried to the mill to be kneaded, where in a quarter of an hour it is made into excellent paſte. But the ſtate of the dough determines the proper time for taking it out, according to the judgment of the attendant baker; ſo that it may take a little more or leſs time, according to circumſtances.

IX.

On the Uſe of Caoutchouc in Manufactures, and on the Amelioration of Spirits by Age. By J. A.

To Mr. NICHOLSON.

SIR,

Clyde, Auguſt 19, 1800.

TO obſerve your attention to the uſeful parts, in preference to the mere curioſities of philoſophy, has given me, in common with many others, great ſatisfaction: and I ſincerely hope your labours are proving uſeful to yourſelf.

In No. 37, of your inſtructive Journal, I obſerve that Mr. Howitſon, an old acquaintance of mine, has made ſeveral promiſing eſſays towards obtaining a valuable manufacture by covering cloth with the Eaſt Indian caoutchouc. In confirmation of the practicability of this, I have to mention, that I have had an opportunity of knowing that hats have been covered with it in South America. Capotas (a kind of cloaks common to the Portugueſe and Spaniards) have alſo been covered with this ſubſtance; and I have known capotas made in Portugal, and ſent to Brazil for this expreſs purpoſe. It is evident that any article of dreſs covered with caoutchouc muſt be perfectly impervious to wet; but the weight of the capotas (which it was ſaid the manufacturers could not remedy) was ſo enormous, that they could only be ſerviceable as boat cloaks.

I have often remarked that ſpirits, particularly Weſt India rum, is univerſally admitted to improve as its age encreaſes, but I do not find that this has been completely and philoſophically accounted for: therefore, through the medium of your monthly publication, I ſhould be glad to know the reſult of any reſearch that may have been made into the nature of this change. Your obſervations on this ſubject, and perhaps thoſe of ſome of your correſpondents who have directed their attention this way, cannot fail to be very intereſting and uſeful to the Weſt India merchants, and will be highly gratifying to

SIR,

Your humble ſervant,

J. A.

P. S. As expoſure to the atmoſphere has been found to produce a premature old age in rum, it has been preſumed that it was cauſed by evaporation; but if this was the caſe, reducing the ſtrength of the ſpirit with water ought to have an effect nearly ſimilar. Whatever may be loſt by expoſure, I have formed the idea, and am clear in it (though I have only the vague evidence of the ſenſes, without any chemical experiment on my ſide) that a great part of the improvement ſo obſervable ariſes from ſome abſorption. At firſt I thought oxygen was abſorbed; but the increaſe of a certain oily appearance does away this idea. It has alſo occured that (agitation promoting the improvement) the particles of liquor may undergo ſome more intimate combination in conſequence of ſomething ſimilar to friction. The particles of old liquor having been kept long in cloſe contact, may have been by that means more aſſimilated. Theſe ideas are, however, quite crude, and deſerve little attention.

The changes which take place in ardent ſpirits and the other products of fermentation by keeping, have not formed the ſubject of direct inveſtigation. How far the ſlow re-action of principles, the extrication of eſſential oil by ſubſidence or floating, the abſorption or combination oxygen from without, or of tannin or gallic acid from the caſk, &c. may be concerned, can only be collected from enquiry among intelligent dealers in theſe articles. I ſhall take the firſt opportunities of doing this, and ſtating the reſults.—W. N.

X.

Abſtract of a Memoir on a Method of uſing the Syphon for raiſing Water in the Machine of C. Trouville. By Citizen PRONY[*].

TO a tube A B (Plate IX. fig. 4.) of any form, are adapted a number of vertical branches; one of them terminates in a reſervoir E, filled with water, and diſtinguiſhed by the name of the great aſpirator, and the others lead to reſervoirs C, full of air, named ſmall aſpirators. Each ſmall aſpirator communicates by its tube, ſuppoſed to be filled with a reſervoir beneath, which is open, and filled with water; and there is a valve opening upwards at the lower end of each of theſe tubes. The air within is of the ſame denſity and temperature as that without; but the external communication of this included air is prevented by the ſides of the engine and the water which occupies part of its interior.

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[*] Bulletin de la Societé Philomatique, II. 92.

The great aſpirator, the ſmall aſpirators, and the reſervoirs, placed beneath theſe laſt, have all the form of vertical priſms; the ſmall aſpirators are equal and ſimilar, and the portions of tube comprized between the aſpirators and their reſervoirs are of equal length.

Things being in this ſituation, we are to ſuppoſe that the water of the great aſpirator flows out by the orifice *f*, and it is required, 1. what will be the depreſſion of the water when the ſpring of the included air, and the charge of the water on the orifice of emiſſion, ſhall be equal to the weight of the atmoſphere; 2. the quantity of water which, at this period, ſhall have riſen in the ſmall aſpirators; 3. the requiſite dimenſions of the apparatus, in order that the water emitted from the great aſpirator, plus that raiſed in one of the other aſpirators may be a determinate part of the water which a ſpring of known product affords in a day; and, 4. the proportions of the ſame apparatus which ſhall give the greateſt ratio between the ſum of the volumes raiſed in all the ſmall aſpirators, multiplied by their elevation, and the volume of water emitted from the great aſpirator, multiplied by its height.

Formulæ for the Solution of the foregoing Problem.

Let $Q =$ the volume of water afforded by the ſpring in one day.

$\gamma =$ the number of times which this volume of water contains that which flows from the great aſpirator from the inſtant of its being filled to the ceſſation of its efflux.

$c =$ the height of the great aſpirator.

$b =$ the common length of the portion of pipe comprized between a ſmall aſpirator and the reſervoir, whence its water is drawn.

$n =$ the number of ſmall aſpirators.

$s =$ the horizontal ſection of each ſmall aſpirator.

$t =$ the horizontal ſection of the lower reſervoir, from which the ſmall aſpirator draws the water.

$S =$ the horizontal ſection of the great reſervoir.

$V =$ the capacity of the interior parts of the machine, which are at no time occupied by water, but conſtantly hold air.

$\lambda =$ the height of the column of water which meaſures the total preſſure of the atmoſphere.

$x =$ height of the water above the orifice of emiſſion of the great aſpirator, at the moment when the ſpring of dilated air within, and the charge of water on the orifice of emiſſion, are equal to the weight of the atmoſphere.

$y =$ the correſpondent depreſſion of the upper ſurface of the water in the great aſpirator.

$z =$

$z =$ the correſpondent height of the priſm of water, which penetrates into any one of the ſmall aſpirators.

Making $\frac{t}{s} = K$; $K + 1 = m$; $Vm + Sm(\lambda + c) + ns\lambda = A$, we have

$$x = \frac{A \pm \sqrt{A^2 - 4Sm\lambda(nbs + Smc)}}{2Sm};$$

and aſſuming the equation $\psi = \frac{\lambda}{\lambda - x}$, we obtain the following:

$$y = c - x = \frac{\lambda - (\lambda - c)\psi}{\psi},$$

$$z = \frac{x - b}{m} = \frac{\lambda(\psi - 1) - b\psi}{m\psi}$$

$$\psi = \frac{\lambda}{\lambda - b - mz} = \frac{\lambda}{\lambda - c + y}$$

$$b = \frac{(\lambda - mz)\psi - \lambda}{\psi}$$

which relates to the vertical dimenſions. With regard to the horizontal, making $\alpha = \frac{V}{nsz}$, we have,

$$s = \frac{Q - \gamma S y}{\gamma z} = \frac{Q}{\gamma z(1 + n[a(\psi - 1) + \psi])}$$

$$S = \frac{Q - \gamma s z}{\gamma y} = \frac{z}{y} \cdot ns(\alpha(\psi - 1) + \psi).$$

Laſtly, if we ſuppoſe the priſms which we have called great aſpirators, to have their height $=$ the value of z here found; the ratio between the product of the height of the great aſpirator by its expenditure, and the product of the maſs of the water raiſed by the height to which it is raiſed, namely, $\frac{c}{b} \cdot \frac{Sy + sz}{nsz}$, being denoted by Φ, we have the formula.

$$\Phi = \frac{c}{b}\left\{a(\psi - 1) + \psi + \frac{1}{n}\right\}.$$

If from Φ the term $\frac{c}{nb}$ be retrenched, the ſurplus will be the ratio between the moving cauſe and the effect produced; and the leſs this ratio differs from unity, the more will the effect approach the maximum of value of which it is capable. It is requiſite, in order that this maximum of value ſhould take place, 1. that the exceſs of m beyond unity, and the height ſhould be diminiſhed, as much as the poſſibility of execution will allow; and, 2. that ψ ſhould be determined by the equation.

$$\psi = \frac{\lambda}{\lambda - mz}\left\{1 \pm \sqrt{\frac{\lambda + amz}{(a + 1)\lambda}}\right\}.$$

The theory of the machine of Cit. Trouville is deduced from the preceding formula, ſuppoſing the ſpring to have a fall equal to the height of the great aſpirator, to which it ſupplies water; and that the firſt ſmall aſpirator draws from the ſpring, and empties itſelf into the reſervoir of the ſecond aſpirator; that this diſgorges into the reſervoir of the third, and ſo forth; all theſe effects producing each other by appropriate machinery.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Klaproth's Analytical Eſſays.

It will undoubtedly afford much ſatisfaction to the cultivators of philoſophical chemiſtry, to be informed, that a tranſlation of the above work, by the learned editor of *Gren's Principles of Modern Chemiſtry*, is in the preſs, and will ſhortly appear.

Multiplication of Engravings in Relief.

Mr. George Gray, of Newcaſtle upon Tyne, has preſented me with ſome ſpecimens of a very uſeful application, of the art of taking impreſſions in foil, or leaf metal. They conſiſt of three blocks; the firſt about three inches ſquare, containing ſixteen lines of pica italic, formed by impreſſing a leaf of tin foil upon the face of the printers types, ſet up in the uſual manner. The tin foil is ſtrengthened by plaiſter of Paris poured into the concavity, to the thickneſs of a little more than half an inch. This block is not as ſharp as the type itſelf; but affords impreſſions on paper perfectly legible, and about as neat as common ballads are uſually printed. The ſecond is an impreſſion taken in braſs foil, from a wood cut of *Bewicke*, 2¼ inches long, and near 1½ inch broad. A ſolution of ſal ammoniac was applied to the concave ſide, and lead then poured in, which conſequently adhered, and gave the requiſite ſtrength. It is very ſharp, but the face has been bended in taking off; ſo that it is not eaſy to print from it. The third is a ſmall figure of a ſhip, within a bounding line of three quarters of an inch ſquare. It is made in the ſame manner with braſs foil, and though the engraving abounds with fine ſtrokes, it affords an impreſſion which could not, I think, be diſtinguiſhed from another taken from the original.

It may perhaps be true, that this art is rather inferior to the method of moulding and caſting, deſcribed at page 63 of the ſecond Volume of this Journal. But it requires leſs of preparation, as well as of ſkill, and with ſome improvements in the foil, the manipulations and the ſtrengthening, may probably exceed the other method in preciſion and effect.

Rational Toys.

I give the following from a paper tranſmitted to me:

It requires but ſlight obſervation to be ſatisfied of the utter inutility of the articles with which the Toy-ſhop is uſually repleniſhed. And on cloſe reflection, it may poſſibly appear neither ſtrained nor ſevere to condemn moſt play-things as worſe than uſeleſs. Their gay appearance, and the movements which they are ſometimes contrived to perform, doubtleſs raiſe ſtrong and ſudden deſires in the mind of children. But ſatiety as quickly follows. The fluſh of delight, ariſing from the firſt impreſſion, cannot but be tranſitory; and no ſooner does the little poſſeſſor examine into the ſtructure of his new acquiſition, than he flings it aſide, or breaks it to pieces and tramples it under foot, as if to revenge himſelf upon it for belying the promiſe of its exterior.

This ſucceſſion of longing and loathing is a more ſerious evil than may at firſt be apprehended. If it be true, that youthful curioſity cannot be frequently baulked with impunity, every ſuch diſappointment may be conſidered as ſome advance towards dullneſs.

We often meet with a ſpecies of toy calculated to excite ſurprize. This, if not liable to the ſame objection as the unmeaning toy, may be ſuſpected of foſtering a diſpoſition for petty ſtratagems, by which a connection between pleaſure in the individual who plays them off, and pain in others is almoſt inevitably eſtabliſhed.

Ten years ago, the idea of ſubſtituting models of machines in the place of ordinary toys, ſuggeſted itſelf to one of the perſons whoſe names are ſubjoined to the preſent paper.

Every quality, he conceived, which diſtinguiſhes models, would ſecure them againſt neglect and deſtruction, the merited fate of toys. The knowledge conveyed by the mutual dependency of the parts, and by the purpoſe of the whole, would be laid up with advantage and might be revived with pleaſure. Whatever improvement the underſtanding derives from mathematics, would more agreeably flow from well-conſtructed models. And mathematics would be ſtudied with more ſucceſs by children accuſtomed to ſuch models. They would rouſe the faculty of invention, and confer the habit of purſuing trains of thought to a great extent. To girls, by conveying information without awakening their ſenſibility, they would be particularly ſerviceable. From this ſtatement, the utility of a ſet of models in ſchools and private families is obvious. It is equally obvious that their utility would not be confined to young people.

This ſcheme has been generally approved by thoſe to whom it has been mentioned. Different perſons, long ſince, offered to advance money towards its execution. Indeed, in 1796, it was partially carried into execution, when the whole deſign was announced under the title of Rational Toys, in a letter prefixed to Mr. Donne's explanation of his elementary mathematical models. Towards its compleat execution, however, there was wanting a perſon well informed concerning machines, and of ready mechanical invention. This difficulty is now removed by the offer of Mr. Robert Weldon, to conduct a manufactory of Rational Toys.

It is however, requiſite that a ſum of money ſufficient to ſet it on foot be advanced; and to ſatisfy ſubſcribers (as far as ſuch a circumſtance may afford ſatisfaction) that they are likely to receive an adequate return for any ſum they may advance, and alſo out of regard to Mr. Weldon, the perſons, named below, have agreed to act as a committee of ſuperintendance.

Soon after a ſufficient ſum ſhall have been ſubſcribed, Mr. Weldon will furniſh the ſubſcribers with a liſt of models and prices, from which each may chooſe to the amount of his ſubſcription. A deſcription and drawing will accompany the models; and they will be contrived ſo as to take to pieces, and its name will be fixed to each part, where this is practicable.

Hereafter, if the undertaking proſpers, the public may be accommodated with ſingle models. But at preſent to ſpare the committee's time, it is propoſed:

I. That no ſubſcription of leſs than 10l. be received.

II. That every ſubſcription not exceeding 10l. be immediately advanced. Larger ſubſcriptions may be paid by inſtalments of 10l.

Theſe terms may ſtartle many, even opulent, parents. And it is true that a ſet of inſtructive models will require a greater ſacrifice than has been cuſtomary in education. But as public opinion on this ſubject is daily becoming more liberal, it may begin to be felt that of all poſſible methods of conſulting happineſs, to ſubtract information for the ſake of adding to fortune, is the moſt prepoſterous. A few additional hundred pounds, well laid out upon the boy, may more improve the condition of exiſtence than the mines of Potoſi, bequeathed to the man. The ill-adviſed niggardlineſs of parents is one principal cauſe why genius has ſo ſeldom been exerted upon the means of early inſtruction, and why the moſt difficult of all occupations—that of forming the ſyſtem of thought and feeling—has been ſo often confided to perſons, whoſe endowments are barely adequate to the rudeſt mechanical labour.

COMMITTEE.

THOMAS BEDDOES, M.D. Clifton.
JOHN BILLINGSLEY, Eſq. Aſhwick-Grove.
WM. CLAYFIELD, Eſq. Briſtol.
BENJ. HOBHOUSE, Eſq. M.P.
JOHN H. MOGGRIDGE, Eſq. Boyce-Houſe.
JAMES STEPHENS, Eſq. Camerton-Houſe.
JOHN WEDGWOOD, Eſq. Cote-Houſe.
WM. WYNCH, Eſq. London.

Subſcriptions are received by

SAVERY and Co. Bankers, Briſtol.
HOBHOUSE and Co. Bankers, Bath.
WM. REYNOLDS and Co. Bankers, Salop.
SIR JOHN CALL PYBUS, Bart. and Co. Bankers, London.
SIR JAMES ESDAILE and Co. Bankers, London.

⁂ Letters (poſt-paid) addreſſed to Mr. WELDON, Pneumatic Inſtitution, Briſtol, will be duly attended to.

Kneading Machine.

Fig. 1

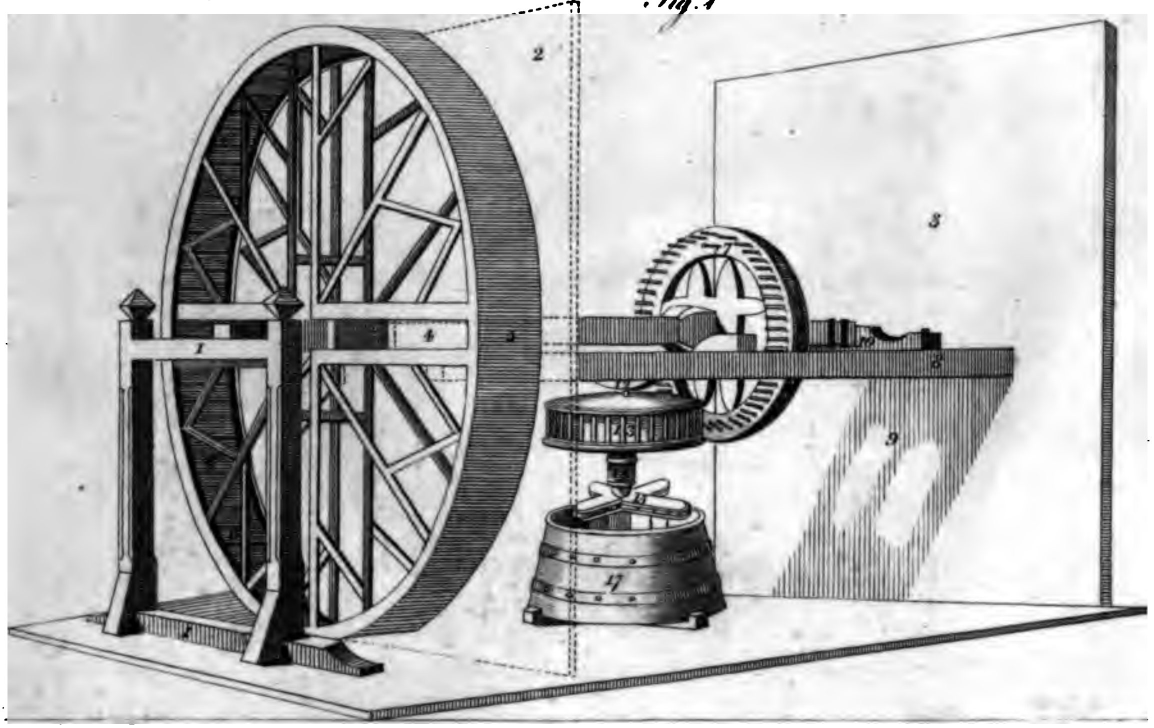
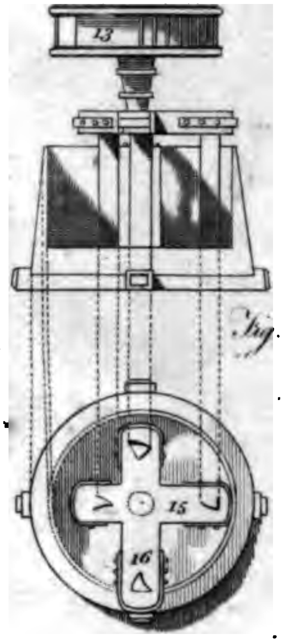


Fig. 2



Mutlow sc Russel Court.

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ASTOR, LENOX AND
TILDEN FOUNDATIONS

Mercurial air pump.

Double barreled air pump.

Fig. 1.

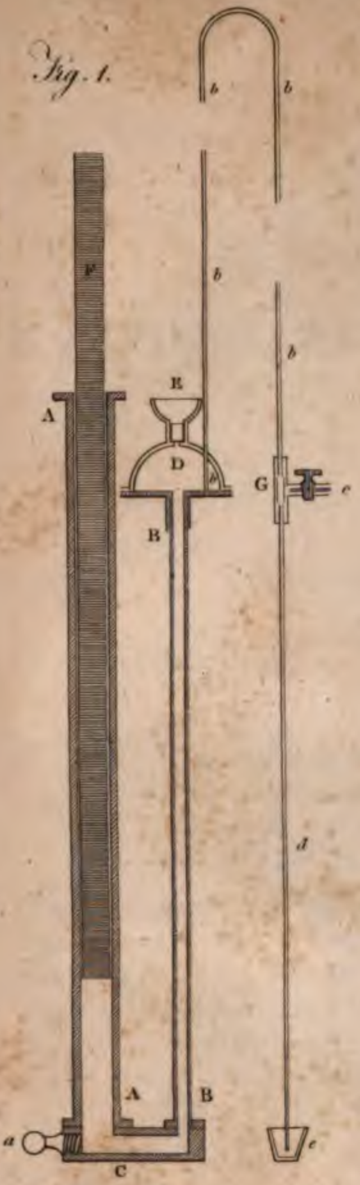


Fig. 2.

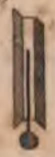


Fig. 3.

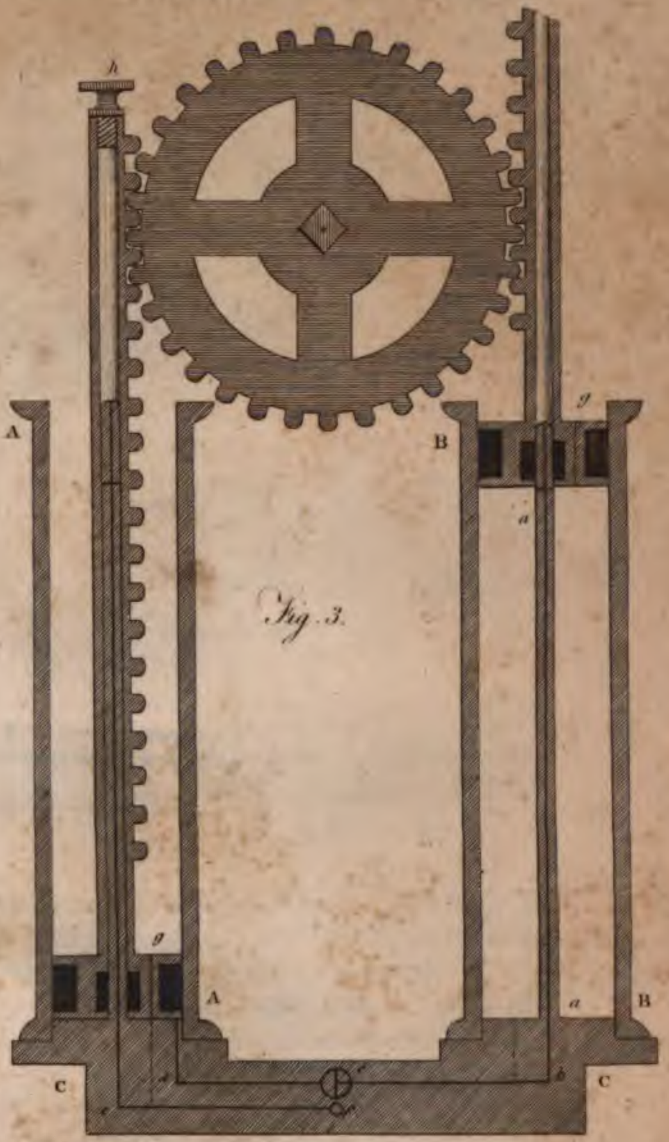
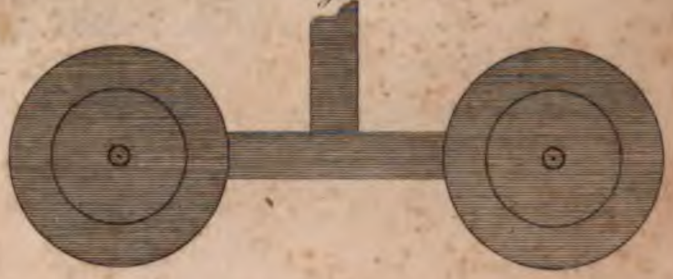


Fig. 4.



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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

OCTOBER 1800.

ARTICLE I.

A Project for extending the Breed of Fine-wooled Spaniſh Sheep, now in the Poſſeſſion of his Majesty, *into all Parts of Great Britain, where the Growth of fine Clothing Wools is found to be profitable*[*].

AFTER experiments had been tried for ſeveral years, by the King's command, with Spaniſh ſheep of the true *Merino* breed, imported from various parts of Spain, all of which concurred in proving, that the valuable wool of thoſe animals did not degenerate in any degree in this climate, and that the croſs of a *Merino* ram, uniformly increaſed the quantity and meliorated the quality of the wool of every kind of ſhort-wooled ſheep, on which it was tried, and more particularly ſo, in the caſe of the South Down, Hereford, and Devonſhire breeds; his Majeſty was pleaſed to command, that ſome *Merino* ſheep ſhould be procured from a flock, the character of which for a fine pile of wool, was well eſtabliſhed.

Application was accordingly made to Lord Auckland, who had lately returned from an embaſſy to Spain; and in conſequence of his lordſhip's letters, the Marchioneſs del

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[*] This paper was drawn up by the Right Hon. Sir Joſeph Banks, Bart. P.R.S. &c. at whoſe expence it has been lately circulated.

Campo di Alange was induced to preſent to his Majeſty five rams and thirty-five ewes, from her own flock, known by the name of *Negretti*, the reputation of which, for purity of blood and fineneſs of wool, is as high as any in Spain: for this preſent, his Majeſty was pleaſed to ſend to the Marchioneſs in return eight fine Engliſh coach horſes.

Theſe ſheep, which were imported in the year 1792, have formed the baſis of a flock, now kept in the park of his Royal Highneſs the Duke of York at Oatlands, the breed of which has been preſerved with the utmoſt care and attention.

The wool of this flock, as well as that of the ſheep procured before from Spain, was acknowledged by the manufacturers who ſaw it, to be to all appearance of the very firſt quality, yet none of them choſe to offer a price for it, at all equal to what they themſelves gave for good Spaniſh wool, leſt, as they ſaid, it ſhould not prove in manufacture ſo valuable as its appearance promiſed; it became neceſſary, therefore, that it ſhould be manufactured at the King's expence, in order that abſolute proof might be given of its actual fitneſs for the fabric of ſuperfine broad cloth; and this was done year after year, in various manners, the cloth always proving excellent; yet the perſons to whom the wool was offered for ſale, ſtill continued to undervalue it, being prepoſſeſſed with an opinion, that though it might not at firſt degenerate, it certainly ſooner or later would alter its quality, much for the worſe.

In 1796 it was reſolved to [illegible]ll the wool at the price that ſhould be offered for it, in order that the manufacturers themſelves might make trial of its quality, although a price equal to its real value ſhould not be obtained; accordingly, the clip of that year was ſold for 2*s.* a pound, and the clip of the year 1797, for 2*s.* 6*d.*

The value of the wool being now in ſome degree known, the clip of 1798 was waſhed in the Spaniſh manner, and it ſold as follows:

The number of fleeces of ewes and wethers was 89;

Which produced in wool waſhed on the ſheeps' backs -	295 lb.
Loſs in ſcowering - - - -	92
Amount of ſcowered wool - - -	203

Which produced,	Raffinos,	167 lb.	at 5*s.* *per* lb.	47*l.* 8*s.*
	Finos,	23,	at 3*s.* 6*d.*	
	Terceros,	13,	at 2*s.* 6*d.*	

The clip of 1799 was managed in the ſame manner, and produced as follows:

The number of fleeces of ewes and wethers was 101;

Which produced in wool, waſhed on the ſheeps' backs -	346 lb.
Loſs in ſcowering - - - -	92
Amount of ſcowered wool - - -	254

Which produced,	Raffinos,	207 lb.	at 5*s.* 6*d.* *per* lb.	63*l.* 14*s.* 6*d.*
	Finos,	28,	at 3*s.* 6*d.*	
	Terceros,	19,	at 2*s.*	

The ram's wool of the two years ſorted, together produced as follows:

Quantity of wool, waſhed on the ſheeps' backs	-	314 lb.
Loſs in ſcowering	- - - -	99
Amount of ſcowered wool	- - -	215

Which produced,	Raffinos,	181 lb.	at 4*s.* 6*d.* *per* lb.	45*l.* 15*s.* 6*d.*
	Finos,	22,	at 3*s.* 6*d.*	
	Terceros,	12,	at 2*s.*	

It is neceſſary to account for theſe extraordinary prices by ſtating, that in the year 1799, when both ſales were effected, Spaniſh wool was dearer than it ever before was known to be; but it is alſo proper to add, that 5*s.* 6*d.* was then the price of the beſt Spaniſh piles, and that none were ſold higher, except as is ſaid, a very ſmall quantity for 5*s.* 9*d.*

The King has been pleaſed to give away to different perſons, who undertook to try experiments, by croſſing other breeds of ſheep with the Spaniſh, more than one hundred rams and ſome ewes: in order, however, to make the benefit of this valuable improvement, in the ſtaple commodity of Great Britain, acceſſible to all perſons who may chooſe to take the advantage of it, his Majeſty is this year pleaſed to permit ſome rams and ewes to be ſold, and alſo to command that reaſonable prices ſhall be put upon them, according to the comparative value of each individual; in obedience to which it has been ſuggeſted, that five guineas may be conſidered as the medium price of a ram, and two guineas that of a ewe; a ſum which it is believed the purchaſer will in all caſes be able to receive back with large profit, by the improvement his flock will derive from the valuable addition it will obtain.

Though the mutton of the Spaniſh ſheep was always excellent, their carcaſes were extremely different in ſhape, from that mould which the faſhion of the preſent day teaches us to prefer; great improvement has however been already made in this article, by a careful and attentive ſelection of ſuch rams and ewes as appeared moſt likely to produce a comely progeny: and no doubt can be entertained, that in due time, with judicious management, carcaſes covered with ſuperfine Spaniſh wool, may be brought into any ſhape, whatever it may be, to which the intereſt of the butcher, or the caprice of the breeder, may chuſe to affix a particular value.

Sir Joſeph Banks, who has the honor of being intruſted with the management of this buſineſs, will anſwer all letters on the ſubject of it, addreſſed to him in Soho-ſquare. The rams will be delivered at Windſor, the ewes at Weybridge in Surrey, near Oatlands.

As thoſe who have the care of his Majeſty's Spaniſh flock, may naturally be ſuppoſed partial to the project of introducing ſuperfine wool into theſe kingdoms, it has been thought proper to annex the following notice, in order to ſhew the opinion held of a ſimilar undertaking, in a neighbouring country, where individuals, however they have miſtaken their political intereſt, are rather remarkable for purſuing and thoroughly weigh-

ing

ing their own perſonal advantage, in all their private undertakings, and for ſagacity in ſeizing all opportunities of improving, by public eſtabliſhment, the reſources of their nation.

FRENCH ADVERTISEMENT.

On the 24th of May laſt, an advertiſement appeared in the *Moniteur,* giving notice of a ſale of two hundred and twenty ewes and rams of the fineſt wooled Spaniſh breed, part of the flock kept on the national farm of Rambouillet; alſo two thouſand pounds of ſuperfine wool, being the preſent year's clip of this national flock, and one thouſand three hundred pounds of wool, the produce of the mixed breeds of ſheep kept at the Menagerie at Verſailles.

This advertiſement, which is official, is accompanied by a notice from Lucien Buonaparte, Miniſter of the Interior, as follows:

" The Spaniſh breed of ſheep that produce the fineſt wool, introduced into France " thirty years ago, has not manifeſted the ſmalleſt ſymptom of degeneration: ſamples of " the wool of this valuable flock, which was brought from Spain in the year 1786, " are ſtill preſerved and bear teſtimony, that it has not in the leaſt declined from its " original excellence, although the diſtrict where theſe ſheep have been kept, is not of " the beſt quality for ſheep farming; the draughts from this flock, that have been " annually ſold by auction, have always exceeded in value the expectation of the " the purchaſers, in every country to which they have been carried, that is not too damp " for ſheep.

" The weight of their fleeces is from ſix * to twelve pounds each, and thoſe of the " rams are ſometimes heavier.

" Sheep of the ordinary coarſe wooled breeds, when croſſed by a Spaniſh ram, produce " fleeces double in weight, and far more valuable, than thoſe of their dams; and if this " croſs is carefully continued, by ſupplying rams of the pure Spaniſh blood, the wool of " the third or the fourth generation, is ſcarce diſtinguiſhable from the original Spaniſh " wool.

" Theſe mixed breeds are more eaſily maintained, and can be fattened at as ſmall an " expence, as the ordinary breeds of the country.

" No ſpeculation whatever offers advantages ſo certain, and ſo conſiderable to thoſe " who embark in it, as that of the improvement of wool, by the introduction of rams " and ewes of the true Spaniſh race, among the flocks of France, whether the ſheep " are purchaſed at Rambouillet, or elſewhere; in this buſineſs, however, it is of the " greateſt importance to ſecure the Spaniſh breed unmixed, and the utmoſt precaution " on that head ſhould be uſed, as the avarice of proprietors may tempt them to ſubſtitute " the croſſed breeds inſtead of the pure one, to the great diſappointment of the " purchaſer.

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* This muſt mean fleeces unwaſhed, or in the yoke, as it is technically termed.

"The amelioration of wool at Rambouillet has made ſo great a progreſs, that in a "circle from twenty-four to thirty-ſix miles in diameter, the manufacturers purchaſe "thirty-five thouſand pounds of wool, improved by two, three, or four croſſes. Thoſe "who wiſh to accelerate the amelioration of their flocks by introducing into them ewes "of this improved ſort, may find abundance to be purchaſed in that neighbourhood at "reaſonable rates."

II.

Deſcription of an Engine for raiſing Water by the lateral Motion of a Stream of Water through a conical Tube In two Letters from Mr. WILLIAM CLOSE.

LETTER I.

To MR. NICHOLSON.

SIR,

Dalton, Aug. 25, 1800.

PART of this town being ſituated upon a dry eminence of lime-ſtone, the inhabitants experience conſiderable fatigue and inconvenience in providing the quantity of water which is indiſpenſibly neceſſary for culinary and domeſtic purpoſes. There are excellent ſprings in the neighbourhood, and at a ſmall diſtance, if taken in a direct line, but the neareſt road to return is ſteep and incommodious. This ſituation has frequently turned my attention to the contemplation of hydraulic machinery to remove the inconvenience, and I doubt not it might eaſily be done by various methods, as a ſtream of conſiderable force runs very near the ſprings, and the height to which water might be raiſed with conſiderable advantage is not great: but this is not the object of my communication.

The experiments of Citizen Venturi on the lateral motion of fluids, induce me to think that an hydraulic engine, operating by the preſſure of the atmoſphere, and in this reſpect ſimilar to that excellent one invented by H. Goodwin, Eſq.* may be brought into action by this principle. With this view I have made ſome experiments, and endeavoured to reduce my ideas into the form of an engine, which I expect to ſee much improved, and of which I ſend you a drawing and deſcription.

A A, in Plate XIII. Fig. 1, repreſents a reſervoir of water, kept conſtantly full, at the ſame time that the jet B is running full under a conſiderable preſſure.

C a tube fixed into the bottom of the ſpherical veſſel D, and paſſing up to ſome height above its middle.

E another tube fixed into the bottom of D, and ending near its top. The lower part of this tube is bended, and the extremity of it is introduced into the ſmaller aperture of the conical tube B.

F a pipe to empty the veſſel D, when filled with water.

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* Philoſ. Journal, IV. 163.

G a ſmall tube paſſing through the tube F, and riſing to near the top of D, for admiſſion of air to quicken the deſcent of water out of that veſſel. Both theſe tubes are cloſed at their lower ends by the lever L, which is fixed upon a cock in the tube E, and has a weight upon one end, in order that the other end may bear againſt the openings of the tubes, at the ſame time that it ſupports the veſſels I, which is ſuſpended upon it by a cord or wire. The upper ſurface of that end of the lever which comes in contact with the tubes, muſt be covered with leather, and muſt preſs againſt them with conſiderable force, that the veſſel D may be perfectly tight when the ends of the other two tubes are placed in water.

H a ſmall ciſtern, provided with a ſyphon, to be filled with water from the reſervoir A, at the ſame time with D; this muſt be done by regulating the cock *k*, upon the pipe which ſupplies H with water.

I a ſmall veſſel and ſyphon ſuſpended from the lever L.

The operation of the engine will be as follows:

The reſervoir A being conſtantly full of water, and the conical tube B completely filled at its wider end by the water which runs out of A; the force of the lateral motion being encreaſed by the form of the tube B, and acting upon the end of E, will rarify the air in D, and the preſſure of the atmoſphere upon the ſurface of the water in the reſervoir, will cauſe part of that water to riſe in the tube C to run over its top and fill D, then to deſcend through E, and enter the ſtream which flows out at B.

When D is full of water, if the tubes F and G are opened, the water will run out; therefore, ſoon after D is full, H is full alſo, and the ſyphon of this laſt veſſel beginning to empty the water, fills I, which overbalances the weight upon the lever L, and opens the pipes F and G, and cloſes the cock in E: the column of water in C immediately deſcends into the reſervoir, and if the ſmall tube G be full of water, it will be emptied by the deſcent of that column, admit air into D, and the water will flow out at F. The veſſels D and H will be empty of water about the ſame time, and the veſſel I, by its ſyphon ſoon after: the weight upon the lever L will then cloſe the tubes F and G, open the paſſage through E, and the water will riſe into D as before.

If the water ſhould deſcend through E, before F and G are opened, it will render the cock in E more tight.

Experiment 1. The veſſel D was conſtructed of a cylindrical form, one inch and three quarters in diameter, and two inches in height, without the tubes F and G, but with two holes, one to let out water, and the other to admit air, both cloſed with wooden ſtoppers. The tube C was three-eighths of an inch inſide diameter, and the tube E, one-fifth of an inch in its internal diameter: this laſt was placed within the tube C for readineſs of conſtruction, its upper end was ſlanted off, and touched the top of D, and it was confined in its place by a wedge of wood thruſt into the bottom of C. A conical tube, nine inches long, one inch diameter at its ſmaller end, and one inch and three quarters at its wider end, was fixed to the ſide of a reſervoir eight inches deep. The depth of the water in the

reſervoir

reſervoir above the ſmaller aperture of the conical tube being ſix inches and a half, and the water which ran through that tube completely filling its wider extremity; the crooked end of the tube E was introduced into the ſmaller aperture of the conical tube, while the bottom of C was two inches below the ſurface of the water in the reſervoir; part of the water immediately roſe fourteen inches in C and filled D; the tubes C and E were then lifted out of the water, and D was emptied by taking out the ſtoppers. The experiment was frequently repeated.

Experiment 2. The veſſel D was made ſpherical, two inches diameter, the ſame tubes C and E adapted as before. The two tubes F and G were added to this, and cloſed by the finger of the operator when water was raiſed. The tube E being lengthened a little, was placed within the ſmall aperture of B, when the charge of the reſervoir was ſeven inches. The bottom of the tube C was juſt below the ſurface of the water. In a very ſhort time part of the water roſe near ſixteen inches through C, and filled D; the tubes *f* and *g* were then opened, and this water ran out.

From ſeveral experiments, I think water will not riſe more than ſixteen inches under ſimilar cirtumſtances.

Upon the admiſſion of air into D, the ſtream of the jet diminiſhed ſo much, that it did not half fill the wider aperture, and it was long after the tubes F and G were cloſed before it could recover its full force. This ſhews the neceſſity of a cock or valve to cloſe E, to prevent the diminution of the jet, when air is admitted into D, if we wiſh to make the engine raiſe much water, and empty itſelf at ſtated times. For no water will riſe ſo long as the conical tube is not full. If a cock be perfectly air tight, it will anſwer the purpoſe very well.

Fig. 2 points out a method of conſtructing a valve for this purpoſe: the veſſel D, in this figure, is cylindrical, the ſmall tube *g* opens very near the top, under that letter, and the dotted line *n* paſſes through the middle of F. The tube C and E are explained before. *m* is a valve which, by its weight, cloſes the upper aperture of E; when the lever L opens the tubes F and *g*, and which immediately before it cloſes theſe pipes it raiſes again: the communication may be made by a piece of ſtrong wire fixed into *m*, and which, paſſing through *f*, will reſt upon the lower valve or lever L. This wire is repreſented by the dotted line *n*.

It perhaps might be advantageous to have a valve in C to ſupport the column of water in that tube alſo.

The power of the engine ſeems to be very conſiderable when conſtructed upon the ſimple plan mentioned in the experiments; it ſeems very deſirable to determine the beſt form of the conical tube, and the method to throw moſt force upon the rarifying tube, ſo as to produce the greateſt effect under a given preſſure of water in the reſervoir. Citizen Venturi has determined the form of a tube to produce the greateſt expenditure*, and it ſeems likely that it may alſo be the beſt form for this purpoſe.

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* Philoſ. Journal, II. 274. This figure is adapted to a certain definite preſſure only, and muſt be varied for other depths.---N.

The lateral motion of a current of air will, to a certain extent, produce the ſame effect as a current of water, but to what height water may be raiſed in this way, I have hitherto made no experiment to determine. The power of an inſtrument in the form of Mr. Boſwell's ventilator * is very conſiderable when made upon a ſmall ſcale, if the ſmall tube be forcibly blown through while the end of the vertical tube is placed in water, one expiration will cauſe the water to riſe in it ſeveral inches.

I am, SIR,

Your's, &c.

WILLIAM CLOSE.

LETTER II.

SIR,

HAVING conſtructed the model of an engine upon the preceding principles, on the 29th of Auguſt laſt, I ſet it to work, and kept it in action for ſeveral hours.

The rarefying tube E, fig. 1. was placed within the aſcending water tube C, until they had both entered the ſpherical veſſel; they were then divided, and the end of the water tube was uſed as the ſupport of a metallic valve, which, by its weight, cloſed the opening of the tube E. To one ſide of this laſt tube was fixed another ſmall one, which opened into the external air by the ſide of the pipe to let out water, and both theſe were cloſed by the end of the lever L, or lower valve. This ſmall tube, at the ſame time that it ſerved for admiſſion of air, to haſten the deſcent of water, ſerved alſo to contain the wire which raiſed the upper valve, *n*, fig. 2.

The lower valve was covered with leather, and when it was well moiſtened, and the engine had been in action for ſome time, I could perceive no fault.

The method of making the engine work itſelf, which I adopted in imitation of the plan propoſed by Mr. Boſwell †, anſwered very well: the lower valve was opened between ſix and and ſeven times in a minute, and water always flowed out; the higher valve completely prevented the diminution of the ſtream through the conical tube, and when it was lifted by the cloſing of the lower valve, no difference could be perceived, although a little air was admitted at each riſe.

The deſcending branch of the ſyphon in the higher veſſel H, was made of conſiderable length, to prevent a conſtant dribble, and to make the reciprocation end at once. The ſyphon of the lower or deſcending veſſel was made full as wide as the other, in order that the weight might preponderate quickly, and cloſe the lower valve immediately after raiſing the higher. The upper veſſel was hung upon the ſide of the reſervoir, and ſupplied with water over the edge by a ſyphon, one aperture of which was made of a proper width. The capacity of the lower veſſel was about five ounces; the higher about ſeven; but was ſometimes diminiſhed, by introducing a ſolid body into its upper part, to make it fill ſooner. Weight, rather above ſix ounces. Length of lever, one foot; it had holes in its under edge to hang the veſſel and weight at different diſtances from its centre. The riſe of the water above the ſurface of water in the reſervoir was one foot. I adopted this height for convenience. There was no valve to prevent the column from deſcending every time the lower valve was opened.

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* Philoſ. Journal, IV. 4.

† Philoſ. Journal, IV. 117.

This particular deſcription may appear tedious, but it will ſave ſome time and conſideration in the repetition of ſuch an experiment.

I entertain no doubt reſpecting the operation of an engine of this kind, and that a column of water may be raiſed to any height, not much exceeding thirty feet, by proportionally encreaſing the preſſure of water in the reſervoir, and the dimenſions of the conical tube: in many ſituations, however, the requiſite quantity for this purpoſe cannot be had, others may not admit of ſufficient deſcent, and in ſome a reſervoir ſo deep may be dangerous and inconvenient.

Where the ſtream has ſufficient deſcent, the water may be raiſed by a number of *lifts* inſtead of one.

Suppoſe 1, 3, 5, fig. 3. to be three reſervoirs with conical tubes, and 2, 4, 6, three ciſterns, through which the raiſed water is carried in ſucceſſion by a ſtream paſſing through the three reſervoirs, and acting upon three engines in the following manner:

The ciſterns 2 and 4 being filled with water, the current through the reſervoir 1 will raiſe a quantity of water into its engine, which will be emptied into the ciſtern 2, at the ſame time the reſervoir 3 raiſes an equal quantity out of 2 into 4, and 5 another out of 4 into 6.

The ſame method may be uſed when there is plenty of water, but not convenience for a deep reſervoir; in this caſe the conical tubes may be fixed to different parts of the reſervoir, and all upon the ſame level. Each engine muſt be provided with a lever and weight, to work its own valves; but they may be all opened at the ſame time by the deſcent of one veſſel connected to all the levers, or each by a veſſel connected to that only.

To quicken the reciprocations of the engine, and encreaſe the quantity of raiſed water, a valve may be made to ſupport the column of water in the aſcending tube; if the column be not heavy, this valve may be contained in a cheſt at the bottom of the tube: it will be eaſy to repair in this place; a piece containing it may be fixed on with ſcrews: it will riſe by the preſſure of the water below.

This kind of machinery, by altering the poſition of the rarefying tubes, will raiſe water from a depth below the ſtream equally as well as to an height above it; and in ſituations where there is plenty of water and convenience for a reſervoir, a lower body of water may be conveyed into a ſtream above by the help of a ſingle tube only; one end of it being placed in the water to be raiſed, the other muſt be introduced into the ſmaller aperture of the conical tube adapted to the reſervoir. A conſtant ſtream will then riſe ſo long as water below can ſupply the tube*.

I am,
SIR,
Your humble ſervant,

Dalton, *Sept.* 1, 1800. W. CLOSE.

My acknowledgements are due to Mr. Cloſe for his obliging private poſtſcript. In reply to his doubt, leſt from the number of hydraulic inventions which have appeared in this Journal, his contrivance might not be inſerted; it ſeems proper to obſerve, that I conſider hydraulic works as of the higheſt importance to ſociety, whether we look to a ſupply of the firſt neceſſity for conſumption, or to the advantageous uſes of neglected, though valuable, firſt movers; that theſe works muſt in moſt caſes be modified by localities and other circumſtances; and, conſequently, that the moſt uſeful practical knowledge will not conſiſt in an acquaintance with one or more of the beſt engines, but with that great variety of happy contrivances which reading and enquiry muſt point out. From reaſons of this ſort, when exerciſing my duty as editor of a work rendered reſpectable by the correſpondence and favor of learned and ingenious men, I have been little ſolicitous of the abſtract comparative merits of the works I receive. If they contain uſeful facts, clear deductions, or new proſpects tending to lead others to improvements in ſcience or the arts, they are certainly well calculated to form part of this miſcellany.—N.

III.

On double Images cauſed by Atmoſpherical Refraction. By WILLIAM HYDE WOLLASTON, *M. D. F. R. S*[†].

IN ſome of the laſt volumes of the Philoſophical Tranſactions, there have been related many inſtances of ſtrong atmoſpherical refraction, by which objects ſeen near the horizon have appeared inverted, and the horizon itſelf either elevated or depreſſed.

Mr. Huddart firſt took notice of a diſtant image, inverted beneath the object itſelf; and, in the Philoſophical Tranſactions for 1797[‡], has deſcribed ſeveral ſuch appearances, accompanied with an optical explanation, wherein he ſhews, that the loweſt ſtrata of the air were at the time endued with a weaker refractive power, than others at a ſmall elevation.

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[*] See the experiments of Venturi on the lateral motion of fluids, before quoted.---C.

[†] Phil. Tranſ. 1800, p. 239.

[‡] Or this Journal, I. 146. See alſo 225, and elſewhere, as by the index.

In the volume for 1799, Mr. Vince has given an inſtance* (tab. 1. fig. 1.) where erect as well as inverted images were viſible above, inſtead of beneath, the objects themſelves; and by tracing the progreſs of the rays of light, in a manner ſimilar to Mr. Huddart's, concludes that theſe phenomena aroſe from "unuſual variations" of increaſing denſity in the lower ſtrata of the atmoſphere.

In the volume for 1795, Mr. Dalby mentions having ſeen "the top of a hill appear "detached, for the ſky was ſeen under it." In this caſe, as well as in the preceding, it is probable that reverſion took place, and that the lower half of the portion detached was an inverted image of the upper, as the ſky could not be ſeen beneath it, but by an inverted courſe of the rays.

Since the cauſes of theſe peculiarities of terreſtrial refraction have not received ſo full an explanation as might be wiſhed, I have endeavoured,

1ſt. To inveſtigate theoretically the ſucceſſive variations of increaſing or decreaſing denſity to which fluids in general are liable, and the laws of the refractions occaſioned by them.

2dly. To illuſtrate and confirm the truth of this theory, by experiments with fluids of known denſity.

And laſtly, to aſcertain, by trial upon the air itſelf, the cauſes and extent of thoſe variations of its refractive denſity, on which the inverſions of objects and other phenomena obſerved, appear to depend.

The general laws may be comprized in three propoſitions.

Prop. 1. If the denſity of any medium varies by parallel indefinitely thin ſtrata, any rays of light moving through it *in the direction of* the ſtrata, will be made to deviate during their paſſage, and their deviations will be in proportion to the increments of denſity where they paſs.

For each ray will be bent towards the denſer ſtrata, by a refracting force proportioned to the difference of the denſities above and below the line of its paſſage; and as their velocities are the ſame, and therefore the times of action of the forces equal, the deviations will be at the refracting forces, *i. e.* as the increments of denſity.

Prop. 2. When two fluids of unequal denſity are brought into contact, and unite by mutual penetration; if the denſities at different heights be expreſſed by ordinates, the curve which terminates theſe ordinates will have a point of contrary flexure.

For the ſtraight lines $d\,a$, $r\,n$ (Plate XIII.) Fig. 1. which terminate the ordinates $r\,x$, $d\,y$, of uniform denſity, will be parallel, and, if not united by contrary curvature, ſome ſtraight line of union, as $a\,o$ muſt be ſuppoſed. But, from whatever cauſe the firſt line $a\,o$ is inferred, by the ſame cauſe other intermediate lines $m\,p$, $t\,q$, &c. will be produced, and curves $d\,e\,f\,m$, $m\,t\,s\,r$, will be ultimately formed, having a point of contrary flexure at m.

The form of the curve does not appear to admit of accurate inveſtigation, nor is it of importance to the ſubſequent reaſoning, if wholly unknown. We may, however, form

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* Ibid. II. 140.

ſome judgment of its nature; for, whether the denſities depend on different ſpecific gravities of different fluids, or on unequal temperatures of different portions of the ſame fluid, the curves will be nearly alike.

In each of theſe caſes, to whatever ſmall diſtance $p c$, Fig. 2, the mutual attraction is ſufficient to occaſion intimate union of the fluids, the denſity $m n$ of the mixture will be an arithmetic mean; and, for the ſame reaſon, at any intermediate ſmaller diſtances, there will be a ſeries of arithmetic means $e f$, $g h$, &c. interpoſed, and the line $a o$, uniting the ordinates, will be ſtraight.

By progreſſive effect of this attraction, and ſucceſſive interpolations, in Fig. 1, curves $d e f m$, $r s t m$, will be formed, of which the ſtraight lines $m p$, $t q$, &c. are tangent.

The attracting denſities $n p$, $o q$, &c. are ſubtangents; and if it be admitted that theſe are every where equal, the curves ſo produced are logarithmic, and the increment of the ordinate greateſt at m, where they meet.

Prop. 3. If parallel rays paſs through a medium varying according to the preceding propoſition, thoſe above the point of contrary flexure will be made to diverge, and thoſe below the ſame point will converge, after their paſſage through it.

For ſince the deviation of each ray depends on the increment of denſity where it paſſes, and ſince the increment of denſity is greateſt at the point of contrary flexure, any rays, as $a b$, Fig. 3, paſſing near to that point, will be refracted more towards the denſer medium than thoſe at $c d$, which move in a higher ſtratum, and will diverge from them, but will be refracted towards and meet thoſe at $e f$, which paſs nearer to the denſer medium, where the increments of denſity are alſo leſs.

Cor. Hence adjacent portions of the converging rays will form a focus, beyond which they will diverge again; and the varied medium will produce effects ſimilar to thoſe cauſed by a medium of uniform denſity[*] having a ſurface ſimilar to the curve of denſities, ſince convergence or divergence will be produced, according as the curve of denſities is convex or concave; conſequently, by tracing backwards, to the extremities of an object, the progreſs of the viſual rays (or axes of the pencils received by the eye) it will be manifeſt that,

Any object ſeen through the inclined concave part $r m$, Fig. 5, would appear elevated, erect, and ſomewhat diminiſhed.

An object ſeen through $m d$, where it is convex and inclined, would be elevated; and, if ſituated beyond the focus of viſual rays from the eye, it would appear inverted. The magnitude would depend on the relative diſtances of the eye and object.

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[*] In the varied medium, $b c$ and $b m$, Fig. 4, the correſponding increments of the abſciſſa and ordinate, are to each other as radius to the tangent of the angle c. Therefore, the tangent of deviation, which is as the increment of the ordinate, varies as the tangent of the angle c.

So alſo, in the uniform medium, ſince the ſines of refraction and incidence are in a given ratio, their differences will bear a given ratio to either of them; and, when the angles are ſmall, the tangent of deviation will vary as the tangent of incidence, or as the tangent of the angle c, which is equal to it.

Below the point *d*, where the curve terminates, viſion would be direct, ſo that an object might be ſituated ſo as to be ſeen in all three ways at the ſame time, direct at O, inverted at I, and erect again at A.

I conſider the foregoing propoſition as applicable to all caſes of varying denſity, whether occaſioned by mutual ſolution of different fluids, or partial rarefaction of the ſame fluid; and, by trial of various fluids, however different in denſity, or even in viſcidity, I find that the refractions obſerve a law agreeable to the theory, as will appear by the following experiments:

Experiment 1. Into a ſquare phial containing a ſmall quantity of clear ſyrup, I put about an equal quantity of water, in ſuch a way that it floated on the ſurface of the ſyrup without mixing. For a ſhort time, the ſtratum of union was ſo thin that nothing could be diſtinctly ſeen through it. But ſoon, by mutual penetration of the water and the ſyrup, the effects repreſented at A, Fig. 6, were produced.

Through the ſyrup, a word written on a card, placed behind, was ſeen erect, and in its place; through the adjacent variable medium, an inverted image was viſible above the true place; and alſo, above that, a ſecond image of the ſame object appeared erect.

When theſe appearances are firſt diſcernible, the variations of denſity are ſo great, that the object to be looked at may be in contact with the phial; but when the variations of denſity become more gradual, and thereby the focus more diſtant, any object ſo near is only elongated, and require to be removed an inch or two, to be ſeen inverted.

Exper. 2. Over the ſurface of the water, in the ſame phial, I next put about the ſame meaſure of rectified ſpirit of wine.

At the ſtratum where the water and ſpirit united, the appearances were the ſame; but ſince the refractive power of ſpirit exceeds that of water, the true place of the object was ſeen uppermoſt; the inverted and erect images are below. Fig. 6, B.

When an oblique line *d e r* is viewed through any variable medium ſo made, it appears bent into different forms, according to its ſituation with reſpect to focal diſtance.

If it be at the diſtance of the principal focus, one point of it is dilated into a vertical line, as *l m*. Fig. 7, A.

If beyond that focus the portion *l m* is inclined backwards, being an inverted image of *d l*; and *m n* is another image of the ſame portion ſeen erect. Fig. 7, B.

On this account, it becomes a convenient object for aſcertaining the ſtate of any medium under examination.

In theſe experiments, the appearances continue many hours, even with ſpirits of wine; with ſyrup, two or three days; with acid of vitriol, four or five; with a ſolution of gum arabic, much longer; but although their diſpoſition to unite is ſo different, the appearances produced are nevertheleſs the ſame in all.

The refraction is greateſt nearly in the plain of original contact of the fluids, diminiſhing from thence both upwards and downwards. The exact rate of diminution above or below this point I had no means of meaſuring with the acccuracy that would be requiſite for determining the nature of the curves of denſity formed according to the firſt propoſition.

But the truth of the ſecond propoſition appeared capable of confirmation by experiment; for the deviation of a ray is there ſaid to depend on the increment of denſity, and time of the ray's paſſage, jointly; accordingly, the deviation cauſed by a given increment ſhould be in proportion to the extent of the medium.

In order to try what effect a greater extent of medium would produce,

Exper. 3. I made a rectangular glaſs veſſel, of which the ſides were in the ratio of 10 to 30,6; and having put into it ſome clear ſyrup, with water on its ſurface, I meaſured the greateſt refractions through it in both directions, and found them in the ratio of 10 to about 29.

In another veſſel, of which the ſides were as 10 to 40,4, the refractions were, on an average, as 1 to 4.

Being now fully ſatisfied of the effect of different fluids, I made the following experiment, whereby it appears, that the variations of denſity occaſioned by difference of temperature between adjacent ſtrata of the ſame fluid, follow a ſimilar law.

Exper. 4. Having put ſome cold water into a ſquare veſſel, I covered its ſurface with a piece of writing paper perforated with a few ſmall holes, and then filled the veſſel cautiouſly with boiling water. The paper nearly prevented any mixture of the hot and cold water; but, by floating gradually up, left them to communicate their heat by contact alone.

While they were in this ſtate, I examined the appearance of remote objects through the varied medium, and found, that when my eye was removed four or five feet from the veſſel, the effects were the ſame as in the preceding experiments with different fluids; above any object viewed through the cold water, I could diſtinguiſh two images of it, the one inverted, the other erect, as uſual; but theſe appearances did not continue more than five or ſix minutes.

Having thus eſtabliſhed, by experiments ſufficiently varied, that the contiguity of two fluids of unequal denſity is capable of occaſioning all the appearances that have been obſerved, I ſhall proceed to ſhew by what means the air may be made to exhibit ſimilar phenomena.

Exper. 5. I heated a common poker red hot, and held it ſo as to look along the ſide of it at a paper 10 or 12 feet diſtant. The rarefaction occaſioned by it cauſed a perceptible refraction to the diſtance of about $\frac{1}{4}$ of an inch from the ſide of the poker. A letter ſeen more diſtant from it appeared as uſual; within that diſtance there was a faint image of it reverſed; and ſtill nearer to the poker was a ſecond image direct, and as diſtinct as the object itſelf, but ſomewhat ſmaller, as in Fig 8, in which a ſection of the atmoſphere ſurrounding the poker is repreſented. At the bottom and ſides it is nearly circular; but upwards the circular form is loſt in undulations, occaſioned by the rapid aſcent of the rarefied air.

The greateſt deviation produced in this caſe meaſured about half a degree.

Exper. 6. By a red hot bar of iron, 30 inches long, the refractions were much greater, the extreme deviations amounting to full $1\frac{1}{4}$ degree.

The refractions obſerved in theſe experiments may at firſt view be thought greater than could be cauſed by difference of temperature alone, being in one inſtance more than double the greateſt horizontal refraction of the heavenly bodies; in which caſe, as the rays enter from a vacuum, the greateſt poſſible effect of the atmoſphere might be expected. But it muſt be remembered, that when a ſtar appears in the horizon, its rays interſect the ſuperior ſtrata of the atmoſphere at an inclination of ſeveral degrees, and that they paſs but once through the variations from rarity to denſity; but, on the contrary, that in the experiments with red hot iron, the rays may paſs actually in the direction of the ſtrata, and that they are refracted not only by their entrance from the denſer into the rarer medium, but the effect is doubled, ſince the refraction cauſed by their emergence is equal to that produced by their incidence.

Although a ſtratum of air, heated by theſe means to ſo great a degree, affords an erect, as well as an inverted, image of objects ſeen through it, the more moderate warmth communicated to it from bodies heated by the action of the ſun upon them, ſeems inſufficient to produce both images; but the inverted image may generally be ſeen when the ſun ſhines upon a brick wall, or other darker coloured ſurface.

While the eye of the obſerver is placed nearly in a line with the wall, if another perſon, at 30 or 40 yards diſtance, extends any object towards the wall, an image ſimilar to it will appear to come out to meet it.

It would be difficult to aſcertain with accuracy the degree of rarefaction capable of ſhewing this appearance, but it may be of ſome uſe to future obſervers to mention the different degrees of heat which I obſerved.

In one inſtance, a thermometer, in contact with the wall, ſtood at 96°; but, at $\frac{1}{8}$ of an inch diſtance, 82°.

One morning, when the ſun ſhone bright, I examined the temperatures and refraction produced at the ſurface of a deal bar painted green, about eight feet long.

A ſmall thermometer, in contact with the bar, roſe to 96°; at $\frac{1}{4}$ of an inch diſtance, it ſtood at 73°.

The refraction at the ſame time exceeded 20 minutes.

To explain why red hot iron occaſions two images, while ſolar heat produces but one, I imagine that the intenſe heat in the former caſe rarefies the air for ſome ſmall diſtance uniformly, and thereby affords the ſame ſeries of variations as between other fluids of uniform denſity; but that, in the latter, the heat is conveyed off as as faſt as it is generated; ſo that, as there is no extent of medium uniformly rare, the denſities correſponding to the concave portion *r m*, Fig. 5, of the curve before deſcribed do not take place, but the phenomena occaſioned by the convex part *m d* are alone produced.

It muſt be remarked, that the vertical poſition of the ſurface contributed greatly to encreaſe the effect; for, ſince the heated air riſes in the direction of the ſurface, its aſcent has in this caſe no tendency to blend it with the adjacent denſer ſtrata, and hence very different degrees of denſity take place in the thickneſs of $\frac{1}{4}$ of an inch; ſo that, as the increments of denſity are great, the refractions will be proportionally ſo; but where the heated ſurface is horizontal, the aſcent of the rarefied air into the ſuperincumbent denſer ſtrata renders the variations far more gradual; conſequently, a heated ſurface of far greater extent muſt be requiſite to produce equal refraction.

However, over extenſive plains, when the ſun ſhines, ſome degree of inverſion is very frequently to be ſeen; but the inverted images are rarely well defined, unleſs over a very even ſurface. One of the beſt ſituations for this purpoſe is over a level open road, with a gentle breeze blowing acroſs it. A current of air brings a cool ſtratum more cloſely in contact with the heated ſurface; and, ſince refraction depends on the increment or difference of denſity in a given ſmall pace, a very moderate breeze will thereby render inverſion more perceptible; but a ſtrong wind will reduce the temperature of the ſurface, and may make the heated ſtratum too thin for any object to be ſeen through it from a diſtance.

In one inſtance, when I ſaw a refraction of about 9 minutes, at the diſtance of about $\frac{1}{3}$ of a mile, a thermometer in the ſand was 101°; at 4 inches above, 82°; and, at 1 foot above, 76°.

Over water, the evenneſs of the ſurface is favourable to the production of ſuch appearances; but, ſince the action of the ſun is weak on a body ſo tranſparent, a far greater extent of ſurface is requiſite to produce any perceptible inverſion.

Being at Bognor one bright morning, when the ſea was calm, I had an opportunity of obſerving the appearance of Selſea Bill, about ſix miles diſtant. The whole extent of coaſt, when viewed with a pocket teleſcope magnifying about ſixteen times, appeared inverted from one end to the other; and the lower part of a brick houſe upon the ſhore was ſeen as diſtinct as the houſe itſelf. I judged the quantity of refraction, in this caſe, to be about two minutes of a degree.

This ſtate of atmoſphere appears to be not very uncommon; for, at Shanklin Chine, in the Iſle of Wight, a few days preceding, ſimilar appearances were viſible in ſeveral directions, but I neglected to make any eſtimate of the quantity of refraction.

In the inſtance of the inverted veſſel ſeen by Mr. Huddart, (Phil. Tranſ. for 1797, Tab. 1, Fig. 3.) at the diſtance of eight miles the refraction ſeems to have been about 3′.

All the appearances deſcribed by him, I am inclined to think, aroſe from difference of temperature alone. He offers a conjecture, that evaporation might occaſion the lower ſtrata of the atmoſphere to have a weaker refractive power; but from the following experiments it ſeems to have a contrary effect:

Exper. 7. I took a plate of glaſs, and, while I looked along the ſurface, I poured upon it a ſmall quantity of ether.

A line upon the oppoſite wall appeared inſtantaneouſly elevated many minutes, and at times above half a degree.

This fluid being the moſt volatile, and moſt ſoluble in the atmoſphere, of any known liquid, produces the greateſt effect; ſince the cold, during evaporation, conſpires with the ether diſſolved in the air to increaſe the refractive power.

Rectified ſpirit of wine alſo produces, from the ſame cauſe, a very conſiderable effect.

Exper. 8. By moiſtening a board, five feet in length, with alcohol, and obſerving the elevation of an object viewed over its ſurface, I found the refraction to be 15′.

Exper. 9. I next made a ſimilar experiment with water itſelf. Of this, the effect was barely viſible, when tried in the ſame way; but, by means of a ſurface of ten feet, and by viewing a luminous point at a greater diſtance, the refraction became evident, and the object elevated above three minutes.

In the courſe of theſe experiments, I tried whether confining the ſaturated atmoſphere, by boards on each ſide, would vary the effect, and found the refraction in all caſes much leſſened; and when water was uſed, it became imperceptible; but as ſoon as the boards were removed, and a free current allowed to paſs acroſs, the full effect was again produced. The reaſon of this difference appears to be, that the quicker evaporation increaſes the degree of cold, and the current brings greater differences of denſity contiguous.

The ſtate of rapid evaporation will fully account for the phenomenon witneſſed by Mr. Latham, who has deſcribed (in the Phil. Tranſ. for 1796, p. 357; or this Journal, II. 417) an extraordinary elevation of the oppoſite coaſt of France, ſo as to be ſeen from the beach at Haſtings, and other parts of Suſſex.

There is a fact of the ſame kind ſtated by De la Lande (*Aſtron.* Tom. II.) who ſays that the mountains of Corſica (though at the diſtance of more than 100 miles) are occaſionally viſible from Genoa.

It is probably owing to the ſame cauſe, that other objects have been ſometimes ſeen, at ſuch diſtances that we ſhould expect them to be intercepted by the curvature of the earth; for it is evident, that whenſoever the evaporation over each mile of ſurface occaſions a refraction of about one minute, the rays receive a curvature equal to that of the ocean, ſo that its ſurface will appear flat; and the ſpherical form of the earth will not obſtruct horizontal viſion of objects at any diſtance.

It ſtill remained to explain the phenomena ſeen by Mr. Vince, as I had not hitherto made an atmoſphere capable of exhibiting images inverted, as well as elevated, by increaſed denſity. For, in the refractions produced in the 7th, 8th, and 9th experiments, by evaporation at an expoſed ſurface, I obſerved the effect was always greateſt in contact with the evaporating ſurface; any lower point *a*, Fig. 9, appeared brought nearer to a higher point *c*, by the pencil of rays from *a* being more refracted at *b*, than the pencil from *c* was refracted at *d*. Therefore, any rays paſſing from the eye at *e*, as a point, through *b* and *d*, would be made to diverge to *a* and *c*; conſequently, viſual rays could not, under theſe circumſtances, interſect each other, and no objects could appear inverted.

But whenever the loweſt ſtrata of the air become ſaturated with moiſture, the variations between the ſaturated ſtratum, and the incumbent atmoſphere of the common denſity, will follow a law ſimilar to what is found at the confines of other fluids of unequal denſity; hence, inverſion will become viſible, as there will be a point below which the increment of denſity will decreaſe, and where the refractions will conſequently be leſs, although through a denſer medium.

Exper. 10. To produce theſe appearances, I procured a trough of thin deal, five feet long, one inch wide, with ſides 2½ inches high, and cloſed the extremities of it with glaſs. A ſection of it is given in Fig. 10.

When the bottom was wetted with ether, the greateſt refraction was, at intervals, more than ¾ of an inch from the bottom of the trough; and, beneath this height, I ſaw a ſecond image inverted, when my eye was removed to 14 or 15 feet diſtance, and the object was about 70 feet. The focus ſeemed at the ſame time to be about nine feet diſtant.

There was not depth enough of uniformly ſaturated atmoſphere for the object itſelf to be ſeen at through it, but its true place, compared with that of the images, is repreſented at *a*.

Exper. 11. When I made uſe of rectified ſpirit in the ſame apparatus, I had alſo ſufficient proof that the laws of evaporation would admit of ſuch appearances being produced; for the ſame object now appeared curved downwards, as in Fig. 11, ſo that rays nearer to the bottom were manifeſtly leſs refracted than ſuch as paſſed at ſome diſtance above. A degree of convergency muſt therefore have been produced, although the diſtance at which the rays would meet was beyond that of my eye, and circumſtances would not admit of my removing beyond 35 feet.

The evaporation of water could not be expected to produce any ſenſible effect of this kind in ſo ſhort a ſpace; but in a view of ſome miles extent, there can be no doubt, from the foregoing experiments, that evaporation from the ſurface of the ſea, in ſuch a ſtate of the atmoſphere as would allow the lower ſtrata to be ſaturated, is capable of occaſioning all the phenomena which have been deſcribed, and probably was the cauſe of thoſe which Mr. Vince obſerved.

Since heat alone tends to depreſs objects, and evaporation produces apparent elevation, it is probable, that in the inſtance of refraction related by Mr. Dalby (Phil. Tranſ. for 1795, p. 587) the heat of the ſun was the principal agent, and that the moiſture rather tended to counteract than aſſiſt its action.

Simple inverſion may generally be ſeen when the ſun ſhines upon a *dry* even road of ⅓ or ½ mile extent; but when the ground has been wet, I have very rarely ſeen it, and have even failed of diſcerning it, when the heat has been ſufficient to raiſe a ſtream from the ground.

The following experiment ſhews that it is not to be expected but by very great extent of ſurface.

Exper. 12. I placed a dark coloured board in the ſun ſhine, and having examined the refraction along its ſurface, I made a wet line along it, with a ſponge dipped in boiling water. Notwithſtanding this additional heat, the refraction, in the direction of the wet line, was far leſs than over the reſt of the board, although I took care to obſerve the effect before the ſurface could be cooled again by evaporation.

I ſhould therefore expect the depreſſion of the horizon at ſea where the refraction occaſioned by heat muſt always be counteracted by evaporation, never to exceed a few minutes; and that any one in a ſituation commanding a view of the ſea, by attention to the various degrees of the dip of the horizon under different circumſtances, might ſoon form ſome eſtimate of the proper allowance to be made for brightneſs of the ſun at the time of an aſtronomical obſervation, or for difference of temperature between the ſea and air.

Having now examined the ſeveral peculiarities of refraction which I propoſed for conſideration, I ſhall, in few words, recapitulate the purport of the foregoing pages.

According to the theory here given, there appear to be two oppoſite ſtates of the atmoſphere, either of which may occaſion objects to be ſeen doubled or tripled, ſince both increaſe and decreaſe of its denſity, when partial, produce the ſame effects.

It has been explained,

1ſt. Why air heated by the moderate warmth of the ſun's rays occaſions objects to appear doubled and inverted.

2dly. Why rarefaction, by a higher degree of heat, gives an additional image which is not inverted.

3dly. In what ſtate of evaporation the increaſe of the air's denſity brings diſtant objects into view by unuſual elevation.

4thly. Under what circumſtances evaporation may alſo produce an inverted image leſs elevated.

And it is probable that the ſame reaſoning will afford a ready explanation to other varieties of terreſtrial refraction, that may have been, or may hereafter, be obſerved.

IV.

A Memoir on the Nature of the colouring Principle of the Lapis-lazuli. By Citizen GUYTON[*].

THE blue ſtone known by the name of *lapis-lazuli* has long been an object of chemical reſearch, with a view more particularly of diſcovering that colouring matter which is the cauſe of its high price, from the luſtre which it gives to works in which it is employed, and on account of the preparation of ultramarine blue, which is ſo highly eſteemed as a pigment.

This colour was at firſt attributed to copper. The celebrated Margraf demonſtrated the error of this opinion, but he found that it conſiſted merely of ſiliceous earth, ſulphate of lime, lime, and a ſmall quantity of iron. Others, ſince his time, have ſuppoſed that oxide of cobalt formed a part of it, whilſt others, like Rinman, imagined the fluoric acid to be one of its ingredients. A more ſtrict examination ſoon deſtroyed theſe conjectures.

The methods of analyſis having of late years been carried to a degree of perfection beyond the moſt ſanguine expectations, it was natural to think that thoſe chemiſts who were the moſt ſkilful in this new art, would not neglect the application of them to the ſolution of this important queſtion. Amongſt others, I ſhall refer to Mr. Klaproth, whoſe works have ſo much enriched the chemiſtry of mineral bodies, and who has paid a particular attention to every kind of foſſil of a blue colour.

In 1784, he publiſhed ſome experiments, which demonſtrated that what is called *native Pruſſian blue*, which is found in peat grounds, and is often of a white colour before it is expoſed to the air, is indebted for its colour to nothing elſe than a combination of iron and phoſphoric acid[†].

At Vorau, in Auſtria, another mineral remarkable for the ſame colour was diſcovered, and was ſucceſſively taken for ſmalt, or native blue oxide of cobalt, for another kind of native Pruſſian blue, and for a blue oxide of copper. It is aſcertained by the examination of the celebrated chemiſt of Berlin, that it contains only ſiliceous earth, alumine, and iron, and though he found that it reſiſted the action of the fire in a leſs degree than the lapis-lazuli, he thinks it might be claſſed among its varieties if it likewiſe contained lime[‡].

This laſt concluſion proves that M. Klaproth had, with his uſual accuracy, determined the conſtituent parts of the true lapis. He, in fact, points them out in the tenth article of his reſearches into mineral ſubſtances, where it is obſervable that the pureſt lapis, which is called oriental, is a compoſition of 46 in the 100 of ſiliceous earth, 28 of carbonate of lime, 14.5 of alumine, 6.5 of ſulphate of lime, 3 of oxide of iron, and 2 of water.

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[*] Read before the philoſophical and mathematical claſs of the National Inſtitute of France 6 Pluvioſe, An. 8, and inſerted in the Annales de Chimie, XXXIV. 54.

[†] Chemiſche Annal. 1784, page 396.

[‡] Beytrage zur Kentniſs der Mineral Korper, etc. Tom. I. p. 197.---Annales de Chimie, XXI. p. 144. (or this Journal, I. 77.)

He therefore corrected the analyſis of Margraff by adding alumine to it, which the other had not noticed, and which more than fifteen years ago I had demonſtrated to be contained in it, by touching a plate of the lapis with ſulphuric acid, which, at the end of ſeveral hours, left very regular cryſtals of alum, which are preſerved upon it.

But by what principle can oxide of iron be tinged with blue without being combined either with Pruſſic acid, or the acid of phoſphorus? M. Klaproth replies that he cannot tell*.

The experiments, of which I ſhall now give an account, appear to me to ſolve this important queſtion; but before I explain the proceſſes and their reſults, I muſt, for greater perſpicuity, take notice of ſome operations which led the way to this concluſion.

In the year 1780, whilſt I was examining a well at Montolier, on the road from Dole to Poligny, in ſearch of coal, I found, at the depth of 35 metres, a bank of gypſum, containing zones of a fine and very lively red. I gave a deſcription and an analyſis of it in the Journal de Phyſique for the month of December in that year, and I inferred from my trials that it was a ſulphate of lime coloured by the oxide of iron.

The late diſcovery of ſeveral new metallic ſubſtances, ſome of which have the property of affording colours of great intenſity, led me to think that the foſſil of Montolier deſerved a re-examination with the view of ſearching for one of theſe oxides. For this purpoſe, at the beginning of the preſent year, I appropriated a piece to thoſe experiments of the mineral analyſis which make a part of my courſe in the polytechnical ſchool. They have been purſued with no leſs accuracy than intelligence by M. Deſormes, formerly a pupil and operator at the laboratory of the ſecond claſs.

After having aſcertained that this mineral does not contain any carbonate of lime, ten grammes of it, reduced to a powder, were ignited in a crucible, by which treatment the colour became deeper, and changed to a yellow brown. There was a loſs in weight of 22.3 per cent.

A. Muriatic acid was repeatedly digeſted on the 77.7 parts remaining after calcination; they did not become diſcoloured, and the acid only deprived it of a very minute portion of iron.

B. The reſidue was then boiled in a ſolution of carbonate of pot-aſh, and a very ſmall part only has yet been decompoſed.

C. What remained was mixed with charcoal, and treated in the crucible. A ſulphuret being formed, it was decompoſed by the muriatic acid, which ſeized the lime and iron. The ſiliceous earth remained mixed with the ſuperabundant coal.

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* Beytrage, &c. I. 201.

D. The liquids of the three preceding operations were put together, and then divided into two equal portions. From the firſt the iron was ſeparated, and was found to weigh 10.5, and it was aſcertained that it contained no other earth except lime. From the ſecond, eight decigrammes or hundredth parts of lime, were obtained.

The whole of the ſolution, therefore, contained:

Oxide of iron	21.
Lime	16.
Sulphuric acid	29.1

the laſt according to the proportions determined by Klaproth.

E. It remained to examine the portion of earth left by the acid, and which was mixed with the ſuperfluous coal. It is here that the operations began to preſent unexpected phenomena, and which, by deviating from the uſual courſe, adviſe the attentive chemiſt that he is on the eve of diſcovery.

This reſidue was at firſt calcined in the open air, in order to burn the coal, but the ſiliceous earth remained black: its weight was 16.5.

It was treated with pot-aſh in a crucible of platina, and yielded a fuſible maſs of a ſuperb blue colour.

Water poured upon it aſſumed the ſame colour.

Nitric acid made it totally diſappear.

The ſiliceous earth ſeparated by evaporation to dryneſs weighed only 8.6.

F. It became of conſequence to know the effect of the different re-agents on the acid which had been applied to the maſs fuſed in the crucible, which might diſcover the ſubſtance which afforded its properties. The following is the reſult of theſe trials:

1. With pruſſiate of pot-aſh this fluid yielded a precipitate of a yellowiſh green colour, which the addition of acids cauſed inſtantly to diſappear inſtead of reviving the blue.
2. With the gallic acid there was no precipitate.
3. With ſulphurated hydrogen there was no precipitate. This, indeed, was to be expected upon the ſuppoſition that iron alone was preſent, but it was proper in this manner to exclude the other metallic ſubſtances which are precipitated by this re-agent.
4. With hydro-ſulphuret of ammoniac there was a fine green precipitate.
5. With ammoniac there was a white precipitate.
6. With pot-aſh there was a light blue precipitate.

The two laſt colours changed, in the drying, to a yellow.

Some comparative experiments were made at the ſame time with a ſolution of nitrate of iron, and the reſult of them was totally different.

G. That there might remain no doubt reſpecting the nature of the ſubſtances by whoſe preſence the reſults of theſe operations was affected; ſyntheſis was called in to the aſſiſtance of analyſis. Sulphuret of iron was prepared in the direct way; a ſufficient quantity of nitrous acid was poured upon it; the filtered liquid was diluted with much water, in order that the exceſs of acid might no longer precipitate the ſulphurated hydrogen which was poured upon it. In this ſtate the ſame re-agents, which had been employed in the former experiments preſented the ſame phenomena. It might be imagined that ſulphate of unoxided iron would, in the ſame circumſtances, afford ſimilar effects; but pruſſiate of iron afforded only a white precipitate, as had been announced by Profeſſor Prouſt.

H. It was now eaſy to draw a concluſion reſpecting the analyſis of the red ſulphate of lime of Montolier. It confirmed the obſervation I had made that it contained nothing but oxide of iron, and determined beſides the quantities of its conſtituent parts, which are as follows:

Sulphuric acid - - -	29.1
Lime - - - -	16.
Oxide of iron - - -	21.9
Siliceous earth - - -	8.6
Water driven off by the firſt calcination -	22.3
Loſs - - - - -	2.1
	100.0

The termination, however, of this ſubject opened a path to new enquiries, of which it is now time to give an account.

I. From obſerving the blue colour which the ſulphate of lime of Montolier had aſſumed by the addition of pot-aſh, the effects of the re-agents upon the acid, in which its iron is held in ſolution, and more particularly the diſappearance of the green precipitate formed by pruſſiates by the addition of an acid, M. Deſormes immediately recollected that he had obſerved phenomena perfectly ſimilar in ſome experiments performed during laſt year with Citizen Clouet upon the lapis-lazuli. This fact already afforded a ſtrong indication that the ſtone in effect contains no other colouring metallic oxide than iron. It remained therefore only to purſue the coincidence of the facts in all their circumſtances, to determine the particular ſtate in which this metal is found when it produces this beautiful blue compoſition. Some experiments added to the obſervations already known upon the properties of the lapis-lazuli will form this connection.

The lapis-lazuli may be urged to a red heat, and even loſe 0.2 of its weight, without any viſible alteration in its colour; but with a ſtronger heat, ſuch as that of the furnace of an enameller, its colour changes to grey. By ſtill encreaſing the intenſity of the fire, it is reduced to a browniſh glaſſy ſcoria, with a diminution of from 10 to 11 hundredths of its weight.

L. When the lapis is pulverized, a ſmell of muſk is ſometimes perceived, which alumine and magneſia equally afford when they are united with a little ſulphur.

M. The lapis is diſcoloured with more or leſs quickneſs by the three acids which are called mineral. The nitrous acid acts almoſt inſtantaneouſly; the muriatic acid is leſs ſpeedy; and the ſulphuric acid acts the moſt ſlowly. Theſe acids often diſengage the ſame ſmell as is produced by trituration.

If the nitrous acid be concentrated, it affords nitrous gas, and ſometimes when the lapis contains carbonate of calx, carbonic acid gas. The liquid, when tried by the pruſſiates, affords a precipitate whoſe colour reſembles that of Pruſſian blue, but viſibly inclining towards a green which is deſtroyed by acids. The hydro-ſulphuret of ammoniac occaſions a precipitate inclining to black.

When nitrous acid, diluted with water, is uſed, there is a diſengagement of a ſmall quantity of ſulphurated hydrogen. The pruſſiates then form only a bright green precipitate in the liquid, which is inſtantly deſtroyed by acids: with hydro-ſulphuret of ammoniac the precipitate is of a fine green.

When the lapis has been previouſly ſubjected to calcination, the dilute nitrous acid diſengages a little of the ſulphureous acid gas.

N. Theſe facts not only prove that the lapis-lazuli contains a ſmall portion of ſulphur, but they alſo demonſtrate the idenity of the colouring principle of this ſtone with every compoſition in which earthy ſubſtances enter into combination with ſulphuret of iron. For we have ſeen (F. G.) the ſulphate of lime containing iron, and converted by charcoal to the ſtate of ſulphuret of iron; and the ſulphuret of iron, prepared in a direct manner, preſent the ſame phenomena under the ſame circumſtances.

O. Before I conclude this memoir, I ſhall make a few obſervations, to guide thoſe perſons who may be diſpoſed to repeat theſe experiments.

The compoſition of every ſpecimen of lapis-lazuli is not eſſentially the ſame. In many pieces, and even in what is called the oriental, we diſtinctly perceive the ſulphuret of iron in cryſtals with the metallic brilliancy; ſometimes it is diſſeminated in ſmall portions; and this undoubtedly is the cauſe which has hitherto prevented the moſt accurate chemiſts from aſcertaining the true colouring principle of this ſubſtance. They have conſidered the ſulphur merely as an accidental product foreign to the ſubject of analyſis, without ſuſpecting that there exiſted a blue ſulphuret of iron. It is obvious that in experiments of this kind the greateſt attention ſhould be paid to the choice of fragments abſolutely exempt of all pyritous admixture, or ſulphuret of iron of a metallic yellow.

The preſence of this laſt ſulphuret is not the only difference which is found in the ſpecimens of the lapis. Out of three kinds which were ſubjected to experiment, one contained ſulphate of lime and ſiliceous earth, with cryſtalized ſulphuret of iron, and the blue ſulphuret of the ſame metal: the ſecond alſo contained barytes: the third, which was abſolutely free from pyritous admixture, contained alſo alumine and carbonate of lime in its compoſition, like that which was analyſed by M. Klaproth.

The fact of cryſtals of alum being rapidly formed upon a fine plate of the lapis, as before remarked, proves beyond all doubt that ſome ſpecimens contain accidentally a ſmall quantity of pot-aſh. It will be proper, therefore, to ſeek the colouring principle amongſt thoſe component parts which are eſſential to the ſtone, and not to be diverted too much by theſe accidental variations. We muſt not, however, ſuppoſe that the affinity of the earths to each other, or to the colouring principle, is without their influence upon the nature of the compound. A very ſtrong proof to the contrary offered itſelf in one of the ſynthetical operations. Sulphate of lime abounding with iron having been treated with powdered charcoal, and then kept in digeſtion in nitrous acid; the pruſſiate of pot-aſh at firſt had no other effect but to turn the fluid green, without affording any precipitate; the addition of a ſolution of alumine immediately determined a green precipitate, which was taken up by acids, and was acted upon in all reſpects like that which was obtained from the decompoſition of the lapis.

CONCLUSION.

I return to the conſequences which appear to me to reſult from the facts laid down in this memoir.

1°. The ſulphate of lime of Montolier is coloured by a red oxide of iron, which adheres ſo ſtrongly to the ſiliceous earth as to reſiſt the action of acids.

2°. This ſulphate, treated with charcoal, produces a ſulphuret of iron, in which this metal is leſs oxided, which being diſſolved in acids, no longer affords a Pruſſian blue by the pruſſiates, but a green precipitate, which is deſtroyed, inſtead of being brightened, by acids, and which preſerves its peculiar blue colour in pot-aſh itſelf, and in the fire which its dry fuſion requires.

3°. In operating upon ſulphuret of iron prepared in the direct way, a product is obtained which manifeſts the ſame properties in the ſame acids, and by the ſame re-agents.

4°. Theſe phenomena are exactly ſimilar to thoſe which the lapis preſents when it is ſubjected to the ſame operations.

5°. So that we can at pleaſure form the blue colouring principle of the lapis, with the only difference which neceſſarily reſults in the natural product, from the ſlow combination of the principle with the earths and the ſulphate of lime.

6°. In a word, the blue ſulphuret of iron is the true and only colouring principle of all the varieties of the lapis, and probably alſo of the mineral known by the name of blue ſtone of Vorau.

V.

Experiments made with the Metallic Pile of Signor Volta, principally directed to aſcertain the Powers of different Metallic Bodies. By Lieut. Col. HENRY HALDANE.

LETTER III.

To MR. NICHOLSON.

SIR,

THE apparatus I have uſed in the following experiments is of a very ſimple conſtruction. A deal board, of about ſix feet long, three inches wide, and one inch in thickneſs. The middle of the board is fluted longitudinally, the channel being 0.6 inch wide. The metallic pieces, which were uſed, being circular, and about 1.3 inch diameter, reſted upon the edges of this groove, and which, with a ſmall block of wood at each extremity or baſe, ſerved to keep them in their places. The board being inclined to the horizon, the channel carried off the water that oozed from the wetted diſcs.

In thoſe experiments in which mercury was combined with other metals, it became neceſſary, on account of the fluidity of the mercury, to adopt ſome other contrivance for the apparatus, by which the flowing of the mercury might be prevented. To effect this, I procured 20 flat pieces of mahogany of two inches ſquare, and of 0.3 inch in thickneſs: in the middle of each of theſe pieces was made a circular hole, of one inch diameter on the lower ſide, and of 1 5 inch diameter on the upper ſide, forming a rabbet at about two-thirds of the thickneſs of the wood from the upper ſurface. The lower ſide was then covered with a piece of leather attached to it with a cement, which was not eaſily acted upon by the water uſed in the experiment; and the leather was turned up the ſides of the wood to make it more ſecure. The cement I uſed is made of melted wax, roſin, and red ochre.

When this apparatus was to be uſed, the pieces of wood were put into pure water for a ſufficient time to ſoak the leather; and when taken from the water, the mercury was poured into the ſmall ciſterns, filling them to the edge of the rabbet; the ſolid metal was then laid upon the rabbet, having its lower ſurface in contact with the mercury. If this ſolid metal was not of ſufficient thickneſs to be level with the ſurface of the wood, a wetted diſc was laid upon it to fill up the vacant ſpace. Theſe ſmall blocks of wood, thus prepared, being piled upon each other, the metallic ſubſtances they contained became capable of acting as a galvanic apparatus.

The order in which theſe experiments are arranged is according to the degrees of influence, the different metals ſeemed to poſſeſs, in forming the oxydating baſes or poles of the apparatus, when any oxydable metallic wires, placed in tubes of water, were ſubmitted to examination; the other metal conſtituting the oppoſite or gaſeous baſe or pole. The diſcs, that were uſed, were ſometimes of card, and ſometimes of leather, and were ſoaked in pure water. Though the epidermis of an animal is obviouſly an inſulating ſubſtance, with reſpect to the galvanic influence (but loſes a part of that property by being wetted) yet I have not diſcovered any difference in the effect, whether wetted card, or wetted leather, was employed; the leather has the advantage of retaining the moiſture, which is ſo abſolutely requiſite in the whole operation of a galvanic apparatus, a longer time than the card; but it diſcolours very much the adjoining ſurfaces of the metallic plates. It ſeems to be only neceſſary that each pair of plates ſhould be ſeparated from the adjacent pairs by ſome ſubſtance that is not a perfect conductor of the galvanic influence, a property which ſeems to be confined to metalline ſubſtances; and that the ſeparating ſubſtance or diſc ſhould be conſtantly wet. The wire uſed in theſe experiments was of copper, and the glaſs tubes were filled with pure water.

Experiment 1.—Zinc, combined with gold—ſilver—iron—copper—lead—tin—mercury.

The reſults of all theſe combinations were:—the cauſtic ſenſation felt by the tongue; the irritation and galvanic ſhock; and the oxydation of the copper wire. The zinc always forming the oxydating baſe or pole, and the other metals conſtituting the gaſeous baſe or pole.

In the combination with gold, (which was 40 guineas, and the zinc, the dimenſions of an half crown piece) the galvanic apparatus ſeemed to act with more vigour than under any other combination, and which was made apparent by comparing it with an apparatus compoſed of ſhillings instead of the half crowns, which were employed in all the other combinations in which ſilver was uſed. In the combination of zinc with ſilver, the apparatus, which was compoſed of 40 pairs of zinc and half crowns, acted with about the ſame power as the gold apparatus; but this difference might be obſerved, that in the gold apparatus the wire connected with the zinc baſe or oxydating pole began, after having depoſited much green oxide of copper, to give out bubbles of air, when, in the ſilver and other apparatus, the wire connected with the gaſeous baſe, after a ſhort time, began to depoſit oxide; that is, in the gold apparatus the power of producing gas ſeemed to predominate, and in the other combinations with zinc, that of producing oxide.

The combinations of zinc with the other metals produced much oxide and gas, and their powers of acting ſeemed to be in the order of iron, copper, lead, tin, mercury. In the combination with mercury, the effects were the moſt feeble. The ſurface of the zinc in contact with the mercury became, of courſe, much amalgamated.

The green oxide of copper in the tubes was examined by putting ſome drops of ammoniac *(aqua ammoniæ puræ)* into each of the tubes; the green oxide was diſſolved, and the water acquired a blue tinge, but ſome flakes of a brown ſubſtance remained in the tubes, not acted upon by the ammoniac. This brown matter was not always formed by all combinations, but the ſilver and copper always produced it.

Experiment 2.—Iron, combined with gold—ſilver—copper—lead—tin—mercury.

In theſe combinations the reſults were nearly ſimilar to thoſe of the firſt experiment, the iron forming the oxydating baſe or pole, and the other metals conſtituting the oppoſite or gaſeous baſe or pole; but theſe apparatus acted more feebly, than the combinations with zinc, excepting when iron was combined with mercury, with which it ſeemed to act as powerfully as any combination in the firſt experiment. With gold and ſilver, iron ſeemed to act tolerably well, produced much oxide, and gas. With copper it acted more feebly, and very minute bubbles only of air were produced. With zinc and tin, it only formed a cloud in the water, and no gas appeared at the extremity of the oppoſite wire. The powers of theſe combinations with iron appeared to be in the order of mercury—gold—ſilver—copper—lead—tin.

The different colours and quantity of ruſt formed upon the ſurfaces of the iron pieces when combined with mercury, gold, and ſilver, compared with what was formed in the other combinations of iron, were worthy of notice. With gold, the ruſt was very red and very bright. The part of the iron in contact with the mercury retained its metallic luſtre.

Experiment 3.—Lead, combined with gold—ſilver—copper—tin—mercury.

In theſe combinations the operations of the galvanic apparatus were very feeble. The lead formed the oxydating baſe. When combined with gold or ſilver, oxide was depoſited from the wire connected with the lead, and the other baſe produced ſome gas. With copper, the oxide was more ſparingly formed, and very minute bubbles of air appeared at the extremity of the wire connected with the copper. With tin, only a ſmall cloud appeared at the extremity of the wire connected with the lead, and no gas was formed by the oppoſite wire. With mercury, 20 pieces of lead acted very feebly; ſcarce any effect was perceptible, a ſmall ſpeck of a cloud was viſible at the extremity of the wire connected with the lead, and no gas at the oppoſite wire: but from the cauſticity which at firſt was perceptible, it appeared that theſe metals in greater quantities might be capable of acting. The lead was, of courſe, much amalgamated by the mercury.

Experiment 4.—Tin, combined with gold—ſilver—copper—mercury.

Theſe combinations acted more feebly than any in the former experiments. The tin, when any effect was apparent, was always the oxydating baſe, but it produced only a cloud in the water in the glaſs tube. With 44 pieces of gold (guineas) a ſmall quantity of gas appeared, but with ſilver, or copper, no gas was viſible at the extremities of the wires connected with thoſe baſes, till the apparatus was increaſed to 160 pairs of metallic plates. With mercury, no effect was produced, not even cauſticity was perceptible, and the tin, which was 20 circular pieces of tin-foil, was ſoon amalgamated, and nearly diſſolved by the mercury.

Experiment 5.—Copper, combined with gold—ſilver—mercury.

In theſe combinations, no effect was viſible, excepting in the combination of 67 pairs of copper and ſilver. The wire which was connected with the copper, was ſurrounded with ſmall bubbles of air, and formed a cloud in the water, but the wire connected with the ſilver baſe, produced no gas. With 56 guineas the copper (penny pieces that have been always uſed) produced no oxyde and no gas, but when the apparatus was put together, a cauſtic ſenſation was perceptible upon touching it with the tongue, and which ſeemed to indicate a tendency to act as a galvanic apparatus if the number of metallic plates had been increaſed. With mercury, 20 plates of copper produced no effects whatever.

Experiment 5.—Silver, combined with gold.

In this combination of 30 pairs of plates, no cauſticity, and no effect whatever, was perceptible.

Judging from the reſults of former experiments, it was not thought neceſſary to combine either ſilver or gold with mercury, as no effect could be expected that was likely to compenſate for the certain amalgamation of thoſe metals; but it would have been very deſirable to have extended theſe enquiries to the effects which the other metallic ſubſtances combined in the form of a galvanic apparatus would produce, particularly *platina*; whether it would act with the other metals in a manner ſimilar to gold or ſilver, forming always the gaſeous baſe of the apparatus; or whether it would be capable of acting with thoſe metals like zinc, iron, &c. and form the oxydating pole of the combinations; but *platina*, particularly in a malleable ſtate, was found, upon enquiry, to be too expenſive an article to be employed by an individual for this purpoſe, eſpecially at a time, when money is ſo frequently required for many other experiments; indeed it might, in ſome degree, be ſubmitted to examination in its granular form, by means of the apparatus which I uſed in the experiments with the quickſilver.

Experiment 6. The object of this experiment, was to examine the effects produced by increaſing the number of pieces that compoſe a galvanic apparatus; and to aſcertain whether its power increaſes in a greater proportion, by augmenting the number of metallic plates, or by extending their ſurface.

The firſt apparatus conſiſted of 200 half crown pieces, combined with 200 pieces of zinc of the ſame diameter; and the diſcs were leather ſoaked in pure water. This apparatus was placed upon the board before deſcribed, extending in length 3 feet 9 inches. The effects of this combination were very feeble in proportion to its dimenſions; when compared with an apparatus of forty pair of plates, its power was not increaſed in the proportion of 5 to 1. It was then re-conſtructed, and put together with leathern diſcs ſoaked in a ſolution of muriate of ammonia (*ſal ammoniac*).

The reſult was, that this apparatus acted very powerfully; the cauſticity, and the galvanic ſhock were more ſevere; the oxidation of the copper wire was more rapid; and the bubbles of air more copiouſly formed in the tubes of water. When the tongue was made part of the circuit, the ſenſation, which in the ſmaller apparatus ſeemed to be rather cauſtic, reſembled in this a pulſation, upon every repetition of the ſhock; when the ſhock was received upon the cheek, a flaſh of light appeared before the eyes, as when the tongue had formed a part of the circuit. When two wires connected with the baſes of this apparatus, were immerged in ſeparate veſſels of water, and the fingers of each hand were brought into contact with the water only, a galvanic ſhock was very perceptible, and a continued numbneſs was felt, if the fingers were permitted to remain in the water. From this experiment it is obvious, that the increaſed effects of a galvanic apparatus, depend more upon the nature of the ſubſtances which enter into its compoſition, than upon the number of metallic plates.

PART II. EXPERIMENT 6.

In this experiment, it was propoſed to examine the effects of a galvanic apparatus compoſed of 20 pairs of metallic plates, each ſix inches diameter, but being diſappointed in obtaining the plates of zinc, the experiment has been deferred. The combinations of lead, iron, lead, and tin, have been examined, and from the increaſed effects which they exhibited, and which are always very feeble, there is little reaſon to expect, that the increaſed ſurfaces of the plates in a galvanic apparatus, will be productive of proportionably increaſed power.

Experiment 7. This experiment is not very diſſimilar to that which you have related in your Journal, (page 185) in which you propoſed to examine the aeriform matter, ſeparately, that was produced by each of the baſes or poles of a galvanic apparatus. In this experiment, an electric exploſion was paſſed through the gaſes which iſſued from the two extremities of gold wire connected with the oppoſite baſes of the apparatus.

The apparatus that was uſed was compoſed of 200 plates of zinc, and 200 half crown pieces, the diſcs were of leather ſoaked in a ſolution of muriate of ammonia; the extremities of two pieces of gold wire were introduced into a ſmall glaſs veſſel filled with pure water; having an interval between them of $\frac{1}{4}$ of an inch. This ſmall veſſel being completely immerged in a larger veſſel of water, was inverted, and the projecting ends of the gold wire attached to the baſes of the apparatus.

The reſult was, that gas iſſued moſt copiouſly from both the extremities of the wire, immerged in the water, and aſcended to the upper part of the veſſel, where it was collected in a large bubble, in the courſe of the time the apparatus continued to act, which was about thirty hours. The wire connected with the zinc baſe was 3 feet 6 inches in length, and the wire connected with the ſilver baſe, about one foot. The former became much tarniſhed, gave out much air, but not ſo rapidly, or for ſo long a time, as the oppoſite wire. The part of the wire connected with the ſilver baſe, that paſſed through the water in the outer veſſel, alſo gave out much air, and became at laſt incruſted with ſome white ſubſtance; upon the bubbles which were conſtantly aſcending from this wire, a ſmall quantity of tincture of turnſole was dropped, but it underwent no change of colour. No oxide, or even a cloud, was produced in the water by either of the wires.

The gas thus collected was decanted into a glaſs tube, which was bent, the branches being about four inches long, and two inches aſunder; the interior diameter was 0.3 inch, and the gas occupied 1.5 inch in the curved part of this tube. The branches being then placed in ſeparate veſſels of water, and two metallic wires having been previouſly fixed in this tube, having a ſmall interval between their extremities at the part where the bubble reſted; the diſcharge of a ſmall electrical jar was paſſed through theſe wires, and the exploſion which took place in the bubble, had ſufficient force to raiſe the tube, the weight of which, with the wire and water it contained, was about three ounces. The bubble of air was reduced to leſs than half its original dimenſions, either by a diminution of its quantity, or of its elaſticity. Many electrical exploſions were then paſſed through the remaining air, and alſo through it, when mixed with atmoſpheric air, but without producing any motion in the tube, or any other effect.

The tube was then removed, and a ſmall quantity of a mixture of oxygenous, and hydrogenous gas, which happened to be ready prepared, was admitted into it, ſo as to form a bubble of the ſame dimenſions as before; and upon paſſing the diſcharge of the electrical jar through it, the effects were preciſely the ſame, as were obſerved in the air extricated from the gold wire, by the operation of the galvanic apparatus.

Experiment 8. By the 7th experiment contained in my letter of the 24th of June, it appears that a galvanic apparatus is not capable of acting in a *Boylean vacuum* *. In this experiment, I introduced into three ſeparate glaſs receivers (of 1 foot 3 inches in height, and 5.5 inches diameter) placed over water, three galvanic apparatus, each of forty half crowns with zinc, and the diſcs ſoaked in pure water. The firſt receiver was full of atmoſpheric air; the ſecond had been filled with oxygenous gas prepared from manganeſe and ſulphuric acid; and the third contained azotic or nitrogene gas, which had been obtained by leaving a mixture of 1.5 ounces of iron filings, with ſulphur in it for five days, during which time the water had riſen in it about four inches. Three glaſs tubes filled with pure water as before, and with copper wires, were joined to braſs wires, which had been previouſly fixed to the baſes of the apparatus, and paſſing under the bottom of the receiver, were extended above the ſurface of the water in the ciſterns.

The reſult of this experiment was, that the copper wires attached to the apparatus in the receiver filled with atmoſpheric air, produced oxide and gas as uſual, but not in ſuch quantities as when the apparatus has free acceſs with the external air. The wires attached to the apparatus in the receiver of oxygenous gas, formed gas and oxide moſt copiouſly, even part of the braſs wire attached to the zinc baſe, depoſited much oxide within the receiver. The wires attached to the receiver of azotic gas, produced neither oxide or gas, and the galvanic apparatus had no apparent effect.

The tubes after remaining twenty hours were removed, and new wire attached to the apparatus in the receivers containing atmoſpheric air, and the oxygenous gas; but they produced only a faint cloud in the water. The water had riſen in the receiver, which manifeſtly indicated a diminution of the original quantity of air.

CONCLUSION.

From this experiment, and from the examination of the ſtate to which atmoſpheric air is reduced in a glaſs receiver, placed over a galvanic apparatus and confined by water, I think we may venture to agree in opinion with Cit. Fabroni, (Phil. Journal, vol. III, p. 308) that the effects of galvaniſm depend on a chemical operation, and are produced principally by the attraction of oxygen from the atmoſphere, and therefore, on the preſent theory, the whole operation can be received only as a combuſtion ſimilar to that which ariſes from the combination of ſulphur, and iron filings with water.

I remain, Sir,
with much eſteem,
your moſt obedient,
humble Servant,

Croydon, Aug. 3, 1800. HENRY HALDANE.

I have ſubmitted to examination ſulphur and iron filings, in the apparatus I uſed for the quickſilver; but I do not perceive that that combination produces any effect as a galvanic apparatus.

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* Vide Mr. Boyle's Letter to Lord Dungarvon, page 10, Quarto.

VI.

Inveſtigation of the Powers of the Priſmatic Colours to heat and illuminate Objects; with Remarks that prove the different Refrangibility of radiant Heat. To which are added, an Inquiry into the Method of viewing the Sun advantageouſly, with Teleſcopes of large Apertures and high magnifying Powers, and Experiments on the Refrangibility of the inviſible Rays of the Sun. By William Herschel, *L. L. D. F. R. S*[*].

THIS eminent philoſopher begins his firſt paper by obſerving, that it is ſometimes of great uſe in natural philoſophy, to doubt of things which are commonly taken for granted; eſpecially as the means of reſolving any doubt when once it is entertained, are often within our reach. Whence it may be affirmed, that any experiment which leads us to inveſtigate what was before admitted upon truſt, may become of great utility to natural knowledge. Thus for inſtance, when we ſee the effect of the condenſation of the ſun's rays in the focus of a burning lens, it ſeems to be natural to ſuppoſe, that every one of the united rays contributes its proportional ſhare to the intenſity of the heat which is produced; and we ſhould probably think it highly abſurd, if it were aſſerted, that many of them had but little concern in the combuſtion or vitrification which follows when an object is put into that focus. From theſe conſiderations, he has thought fit to mention what led him to ſurmiſe, that the power of heating and illuminating objects might not be equally diſtributed among the variouſly coloured rays.

In a variety of experiments occaſionally made, relating to the method of viewing the ſun with large teleſcopes to the beſt advantage, he uſed various combinations of differently coloured darkening glaſſes. What appeared remarkable was, that when he uſed ſome of them he felt a ſenſation of heat, though he had but little light; while others gave much light, with ſcarcely any ſenſation of heat. Now as in theſe different combinations the ſun's image was alſo differently coloured, it occurred to him that the priſmatic rays might have the power of heating bodies very unequally diſtributed among them; and as he judged it right in this reſpect to entertain a doubt, it appeared equally proper to admit the ſame with regard to light. If certain colours ſhould be more apt to occaſion heat, others might, on the contrary, be more fit for viſion by poſſeſſing a ſuperior illuminating power; and at all events, it would be proper to recur to experiments for a deciſion.

In the firſt ſeries of experiments *on the heating power of the coloured rays of the ſun*, a piece of paſteboard was mounted in a frame, ſo that its obliquity could be varied with reſpect to the horizon, nearly in the ſame manner as in the common table looking glaſs. Through this paſteboard was cut a notch, or ſlit, of width a little larger than the ball of a thermometer, and of ſufficient length to permit the whole of one of the priſmatic colours to paſs through; ſo that in the actual exhibition, this ſlit was placed parallel to the axis of a priſm through which the ſolar rays paſſes, and the ſurface of the paſteboard was adjuſted at right angles to the ray itſelf. Three delicate thermometers with their balls blacked, and ſufficiently detached from their reſpective ſcales, were placed on the platform of the frame, at ſuch a diſtance beneath the opening in the paſteboard, that any one, or all of them, might at pleaſure be expoſed to the coloured rays, or ſhaded from the light by the interpoſition of the unperforated ſurface. When one of theſe was expoſed to the rays, its mercury roſe, while that in the other two remaining ſtationary, proved, that the acceſſion of heat was produced by the mere action of the ſolar light. Eight ſets of experiments were tabulated, from which it is ſeen, that the mean riſe in one of the thermometers in red rays, during ten minutes was $6\frac{7}{8}$ degrees; and in the green rays $3\frac{1}{4}$ degrees; and in the violet rays two degrees. With a ſmaller thermometer the ſame effect followed, but the alteration was leſs, probably from the cooling agency of the aſcending ſtream of air, which would act more ſtrongly where the proportion of ſurface to bulk was greater. The mean reſults from both thermometers were, that the degrees of elevation in the red, green, and violet rays, proved nearly as the numbers $3\frac{1}{2}$, $1\frac{1}{2}$, and 1 reſpectively.

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[*] Abridged from two papers in the Philoſophical Tranſactions for 1800, pages 255---292. The titles of both are united in the above, with no other variation than the inſertion of the conjunction *and* before the word *experiments*, which begins the ſecond title; and the neceſſary change of the word *is* for *are*.---N.

The ſecond courſe of experiments deſcribed in this paper, was made *on the illuminating power of coloured rays;* in which the author had two ends in view, the firſt with regard to the illumination itſelf, and the ſecond with reſpect to the aptneſs of the rays for giving diſtinct viſion: properties which, though there did not ſeem to be any reaſon why they ſhould not have the ſame meaſure, appeared nevertheleſs to deſerve to be ſeparately attended to.

The microſcope offered itſelf as the moſt convenient inſtrument for this inveſtigation; and this was uſed as upon opake objects, in order to avoid any effect that might be expected to ariſe from tranſmiſſion through the parts of coloured tranſparent bodies.

The regiſter of a number of experiments, in which different objects were viewed in the coloured rays by a magnifying power of 42 times, is given; among theſe a nail is mentioned as peculiarly ſuited for reſearches of the preſent kind. It was choſen on account of its ſolidity and blackneſs, as being moſt likely to give an impartial reſult of the modifications ariſing from an illumination by differently coloured rays; but on viewing it, Dr. H. was ſtruck with the ſight of a bright conſtellation of thouſands of luminous points, ſcattered over its whole extent, as far as the field of the microſcope could take it in. Their light was that of the illuminating colour, but differed conſiderably in brightneſs: ſome of the points being dim and faint, while others were luminous and brilliant. The brighteſt of them alſo admitted of a little variation in their colour, or rather in the intenſity of the ſame colour; for in the center of ſome of the moſt brilliant of theſe lucid appearances, their light had more vivacity, and ſeemed to deviate from the illuminating tint towards whiteneſs, while on and near the circumference it ſeemed to take a deeper hue.

The obſervations which agreed uncommonly well together, afforded the concluſion that the red making rays were far from poſſeſſing an eminent degree of illumination: that the orange poſſeſs more of it than the red; the yellow ſtill more; the maximum of illumination lies in the brighteſt yellow, or the paleſt green; the green itſelf is nearly as bright as the yellow; but from the full deep green the illuminating power decreaſes very ſenſibly. That of the blue is nearly upon a par with the red; the indigo much leſs than the blue; and the violet is very deficient. With regard to the principle of diſtinctneſs, none of the colours appeared to be deficient; that is to ſay, that though for want of illumination in the leſs powerful colours in this reſpect, fewer bright ſpots could be diſcerned, yet thoſe which were viſible were perfectly diſtinct.

Before the Dr. proceeds to the next part of his ſubject, he digreſſes for a moment to remark, that the foregoing reſearches ought to lead us on to others. "May not" ſays he, "the chemical properties of the priſmatic colours be as different as thoſe which relate to light and heat? Adequate methods for an inveſtigation of them may eaſily be found; and we cannot too minutely enter into an analyſis of light, which is the moſt ſubtle of all the active principles that are concerned in the mechaniſm of the operations of nature. A better acquaintance with it may enable us to account for various facts that fall under our obſervation, but which have hitherto remained unexplained. If the power of heating, as we now ſee, be chiefly lodged in the red-making rays, it accounts for the comfortable warmth that is thrown out from a fire, when it is in the ſtate of a red glow; and for the heat which is given out by charcoal, coke, and balls of ſmall coal mixed up with clay uſed in hot houſes; all which it is well known throw out red light. It alſo explains why the yellow, green, blue, and purple flames of burning ſpirits mixed with ſalt, occaſion ſo little heat, that a hand is not materially injured, when paſſed through their coruſcations. If the chemical principles of colours alſo when aſcertained, ſhould be ſuch, that an acid principle, for inſtance, which has been aſcribed to light in general, on account of its changing the complexion of various ſubſtances expoſed to it, may reſide only in one of the colours, while others may prove to be differently inveſted, it will follow, that bodies may be variouſly affected by light, according as they imbibe and retain, or tranſmit and reflect, the different colours of which it is compoſed."

That radiant heat is of different refrangibility, is alſo deducible from theſe experiments, whether it be one and the ſame thing, or different from light; becauſe, as the author obſerves, if this were not the caſe, the whole of the heat would be confined in a ſpace equal in breadth to the priſm itſelf; the contrary to which is proved by the facts. He alſo ſtates, as is more fully developed in the ſubſequent paper, that as the maximum of light is found at a much higher degree of refrangibility than that of heat in the viſible ſpectrum, the abſolute maximum lies even ſhort of the limits of viſible radiation in the ſpectrum. And admitting, as is highly probable, that the organs of ſight are only adapted to receive impreſſions from particles of a certain momentum, an explanation will be had, why the maximum of illumination ſhould be in the middle of the refrangible rays, as thoſe which have greater or leſs momenta are likely to become equally unfit for the impreſſions of ſight. Whereas in radiant heat there may be no ſuch limitation to the momentum of its particles. From the powerful effects of a burning lens, however, the information is gathered, that the momentum of terreſtrial radiant heat is not likely to exceed that of the ſun; and that conſequently the refrangibility of calorific rays cannot much extend beyond that of calorific light. Hence alſo it is inferred, that the inviſible heat of red hot iron gradually cooled till it ceaſes to ſhine, has the momentum of the inviſible rays, which in the ſolar ſpectrum, viewed by day light, go to the confines of red; and this will afford an eaſy ſolution of the reflection of inviſible heat by concave mirrors.

The reſults of the foregoing inveſtigation, originally ſuggeſted by the phenomena of viſion by teleſcopes directed to the ſun, became uſeful in their turn to direct the proceſſes, by which a method might be obtained of *viewing that luminary with teleſcopes of large apertures and high magnifying powers.* The focal heat in the large teleſcopes uſed by the Doctor, was ſufficient ſpeedily to break the darkening ſlips of wedge-formed glaſſes, commonly uſed with achromatic teleſcopes. It was a Newtonian reflector of nine inches aperture, which he wiſhed to adapt for ſolar inſpection, and his aim was to employ the whole ſurface of the ſpeculum. Two red glaſſes intercepted full as much light as was neceſſary; but the eye could not bear the irritation ariſing from a ſenſe of heat. Green glaſſes, one of which was ſmoked, ſtill gave a brighter illumination than the red, but they remedied the inconvenience of the heat. Various trials for intercepting the red, and other rays from a priſm, with differently coloured glaſſes, were made; of which the particulars are given, and alſo of the effects of a coating of ſmoke and of pitch, and of coloured fluids, the latter of which when denſe enough to ſtop much light, were not found ſufficiently pure to be uſed.

Theſe laſt trials, however, were not ſufficient to aſcertain the very eſſential particular of diſtinctneſs afforded by theſe ſeveral media. It was neceſſary to try the ſeveral glaſſes and combinations in the actual inſtrument, previous to the account of which the Doctor deſcribes an eaſy way of uniformly ſmoking glaſſes, which I ſhall proceed to copy.

"With a pair of warm pliers take hold of the glaſs, and place it over a candle at a ſufficient diſtance not to contract ſmoke. When it is heated, but no more than ſtill to permit a finger to touch the edges of it, bring down the glaſs at the ſide of the flame as low as the wick will permit, which muſt not be touched. Then, with a quick vibratory motion, agitate it in the flame from ſide to ſide; at the ſame time advancing, and retiring it gently all the while. By this method you may proceed to lay on ſmoke to any required darkneſs. It ought to be viewed from time to time, not only to ſee whether it be ſufficiently dark, but whether any inequality may be perceived; for if that ſhould happen, it will not be proper to go on.

"The ſmoke of ſealing wax is bad: that of pitch is worſe. A wax candle gives a good ſmoke: that of a tallow candle is better. As good as any I have hitherto met with, is the ſmoke of ſpermaceti oil. In uſing a lamp you may alſo have the advantage of an even flame extended to any length."

The teleſcopic experiments, with various darkening glaſſes applied in the eye piece, are numbered and deſcribed. Difficulties preſented themſelves, from the heat which paſſed through ſome combinations, and the want of light and diſtinctneſs in others. The heat which was intercepted when the glaſſes were placed near the focus of the pencils, was alſo found to break them by its partial action. To remedy this, one of the glaſſes was fixed near the ſmall ſpeculum, in order that the light might be ſpread over a larger ſurface; but here alſo the heat was too ſtrong, and produced the ſame inconvenience. The Doctor therefore at laſt placed his apparatus cloſe behind the eye glaſſes, as follows in his beſt combination.

"No. 25. I placed a very dark green glaſs behind the ſecond eye glaſs, that it might be ſheltered by both glaſſes, which in my double eye-piece are cloſe together, and of an equal focal length. Here, as the rays are not much concentrated, the coloured glaſs receives them on a large ſurface, and ſtops light and heat in the proportion of the ſquare of its diameter now uſed, to that on which the rays would have fallen, had it been placed in the focus of pencils. And for the ſame reaſon, I now alſo placed a dark green ſmoked glaſs cloſe upon the former, with the ſmoked ſide towards the eye, that the ſmoke might likewiſe be protected againſt heat by a paſſage of the rays through two ſurfaces of coloured glaſs."

"This poſition had moreover the advantage of leaving the teleſcope, with its mirrors and glaſſes, completely to perform its operation, before the application of the darkening apparatus; and thus to prevent the injury which muſt be occaſioned, by the interpoſition of the heterogeneous colouring matter of the glaſſes and of the ſmoke."

"No. 26. I placed a deep blue glaſs, with a blueiſh green ſmoked one upon it, as in No. 25, and found the ſun of a whiter colour than with the former compoſition. There was no diſagreeable ſenſation of heat; a little warmth might be felt."

Theſe two are the combinations through which the Doctor has ſeen uncommonly well, and in a long ſeries of very intereſting obſervations upon the ſun, which will ſoon be communicated, the glaſſes have met with no accident. However, when the ſun has conſiderable altitude, he finds it adviſeable to leſſen the aperture a little in teleſcopes, which have ſo much light as his ten feet reflector, or (which will give more diſtinctneſs) to view the ſun earlier in the morning, and later in the afternoon; becauſe the light intercepted by the atmoſphere in lower altitudes, will reduce its brilliancy much more uniformly than it can be ſoftened, by laying on more ſmoke on the darkening glaſſes. And as few inſtruments in common uſe are ſo large as that to which this method of darkening has been adapted, he expreſſes his hope that it may be of general uſe in ſolar obſervations.

In the ſecond paper *on the refrangibility of the inviſible rays of the sun*, the author firſt deſcribes his apparatus, which is delineated in Plate XIV. where A. B. repreſents a ſmall ſtand covered with white paper, upon which are drawn five lines parallel to each other, at half an inch diſtance aſunder, but ſo that the firſt is only a quarter of an inch from the edge. Theſe lines are interſected by three others at right angles, the ſecond and third of which are reſpectively at 2½ and 4 inches from the firſt; 1, 2, 3, repreſent the thermometers uſed in the former experiments, mounted upon their ſmall inclined planes, and placed ſo that the centres of the ſhadows of their balls fall upon the interſection of theſe lines. C D is the priſm at the window, and E is the ſpectrum thrown upon the table, ſo as to bring the laſt quarter of an inch of the red colour upon the ſtand. In this arrangement all the ſpectrum, except the vaniſhing laſt quarter of an inch, paſſed by the edge of the ſtand, and falling upon the table, could not interfere the experiments. The room was darkened.

The trials of heat were made by cauſing the ſpectrum to fall either upon or oppoſite the outer thermometer, while the other two were kept as ſtandards out of the plane of the refraction; and when it was judged neceſſary, in repetitions, the thermometer, which had been ſubjected to the heat, was changed for one of the others. In three experiments with the ſame thermometer, the heat produced—by rays affording no illumination, but falling half an inch beyond the extreme confine of the red colour, was 6½ degrees in 10 minutes; —by rays falling one inch beyond that confine, it was 5¼ degrees in 13 minutes;—and by rays falling 1½ inch beyond that confine, it was 3⅛ degrees in 10 minutes. At the other extremity of the ſpectrum there was no augmentation of heat produced beyond the confine of violet rays. The diſtance of the priſm was 52 inches.

Hence it followed clearly that there are rays coming from the ſun which are leſs refrangible than any which affect the ſight; and that they are inveſted with a high power of heating, but with none of illuminating bodies, which explains the reaſon why they have hitherto eſcaped unnoticed.

As the heat was before found not to correſpond with the meaſure of illumination in the ſpectrum, and in theſe experiments it extended beyond that limit, it became an object of intereſt and importance to aſcertain the place where the calorific power is greateſt. *This maximum of heat was found by experiment to be about half an inch diſtant from the boundary of the red colour,* and the heat at one inch was fully equal to that of the middle of the red colour itſelf. The boundaries of what may be called the calorific ſpectrum lie between the extreme of violet to an undetermined ſpot at leaſt 1½ inch, or 1½° beyond the boundary of the red colour.

In his concluding ſummary, beſides ſome remarks and recapitulation, which are included in the preceding account, the author adds, that if we may infer the quantity of the efficient from the effect produced, the inviſible rays of the ſun probably far exceed the viſible ones in number; and that if we call *light* thoſe rays which illuminate objects, and *radiant heat* thoſe which heat bodies, it may be inquired, whether light be eſſentially different from radiant heat? In anſwer to which he ſuggeſts, that we are not allowed, by the rules of philoſophizing, to admit two different cauſes to explain certain effects, if they may be accounted for by one. A beam of radiant heat, emanating from the ſun, conſiſts of rays that are differently refrangible. The range of their extent, when diſperſed by a priſm, begins at violet coloured light, where they are moſt refracted, and have the leaſt efficacy. Theſe calorific rays have been traced throughout the whole extent of the priſmatic ſpectrum; and their power was found to be increaſing, while their refrangibility was leſſened as far as to the confines of red-coloured light. But their diminiſhing refrangibility and increaſing power did not ſtop here; for they have been purſued a conſiderable way beyond the *priſmatic ſpectrum*, into an inviſible ſtate, ſtill exerting their increaſing energy, with a decreaſe of refrangibility up to the maximum of their power; and have alſo been traced to that ſtate where, though ſtill leſs refracted, their energy, on account, as may be ſuppoſed, of their now failing denſity, decreaſed pretty faſt; after which the inviſible *thermometrical ſpectrum* ſoon vaniſhed.

If this be a true account of ſolar heat, for the ſupport of which he appeals to his experiments, he concludes that we muſt admit that ſuch of the rays of the ſun as have the refrangibility of thoſe which are contained in the priſmatic ſpectrum, by the conſtruction of the organs of ſight, are admitted under the appearance of light and colour; and that the reſt, being ſtopped in the coats and humours of the eye, act upon them, as they are known to do upon all the other parts of our body, by occaſioning a ſenſation of heat.

VII.

Additional Experiments on Galvanic Electricity. By Mr. Davy, Superintendant of the Pneumatic Inſtitution.

To Mr. NICHOLSON.

SIR,

THE earlier experimenters* on animal electricity noticed the power of well burned charcoal to conduct the common galvanic influence.

I have found that this ſubſtance poſſeſſes the ſame properties as metallic bodies in producing the ſhock and ſpark † when made a medium of communication between the ends of the galvanic pile of Signor Volta.

I have likewiſe found that perfectly well made charcoal, when connected with water or aqueous ſolutions in the galvanic circuit, effects changes in them analogous to thoſe produced by metals; but connected with peculiar appearances.

1. Two long and thin ſlips of dry charcoal were connected with ſilver wires attached to the ends of a galvanic pile of 60 pieces. The points of the charcoal ſlips were immerſed in a glaſs of water, at the diſtance of half an inch from each other; and the globules of air adhering to them being carefully removed, the communication made ſure.

In about a minute, particles of gas began to form and evolve themſelves round the point of the charcoal connected with the ſilver ſide of the apparatus. Near a quarter of an hour

* The inventor of the galvanic pile diſcovered the conducting power of charcoal. His experiments were confirmed by Creve and Schmuck. See Pfaff on Animal Electricity, page 48.

† The ſpark is moſt vivid when the charcoal is hot.

elapſed, before any gas was produced from the zinc ſide; the gas that was produced adhered to the charcoal in large globules, and did not paſs through the water. As long as the communication was kept up, the ſilver charcoal gave out gas very rapidly.

2. Reaſoning from the common phenomena of the action of red-hot charcoal on water, and on the analogous galvanic facts, it was reaſonable to conclude that the gas evolved from the charcoal on the ſilver ſide of the apparatus was hydrocarbonate; and that carbonic acid had been produced on the zinc ſide, which had been chiefly abſorbed by the water.

To aſcertain if this concluſion was true, two ſmall open tubes, about one-fourth of an inch in diameter, and three inches long, were provided. Into one end of each of them a thin piece of hard and poliſhed charcoal was introduced, and faſtened by cement. They were then filled with diſtilled water, and inverted in a glaſs containing that fluid; the tops of the pieces of charcoal being made to communicate with the ends of a pile.

The proceſs was carried on for more than fourteen hours; at the end of which time the quantity of gas produced from the charcoal on the ſilver ſide was at leaſt fifty times greater than that produced on the zinc ſide. The tube from the zinc ſide, with its water and gas, was introduced into a veſſel of lime water. On agitation the water became clouded, but the gas was not perceptibly diminiſhed: mingled with twice its bulk of nitrous gas, it gave ſuch an abſorption, as denoted that it contained nearly the ſame quantity of oxygen as common air.

The gas produced from the ſilver ſide of the pile did not at all diminiſh with nitrous gas; twelve meaſures of it, mingled with eight meaſures of oxygen, in a detonating tube, and acted on by the electric ſpark, inflamed and left a reſiduum equal to rather more than three meaſures. Lime water introduced to theſe became a little clouded, and a ſlight abſorption took place. After this abſorption, at leaſt two meaſures and a quarter of gas remained, which, mingled with nitrous gas, gave red fumes and diminution. Hence they evidently contained oxygen.

3. Surpriſed at theſe reſults, from which it appeared that the gas from the ſilver ſide of the apparatus held very little charcoal in ſolution, and required nearly the ſame quantity of oxygen to deſtroy it as the inflammable air from the metals, I repeated the experiment, making uſe of water that had been long boiled, and was yet warm. In this caſe no gas was given out from the zinc ſide during the whole of the proceſs, and more than half an hour elapſed before any was produced from the ſilver ſide. What was produced, however, gave nearly the ſame diminution, when fired with oxygen, as common inflammable air, and the reſiduum produced but a ſlight precipitate admitted to lime water.

It was eaſy to account for the deficiency of gas on the zinc ſide in this proceſs, by ſuppoſing that the gas produced in the former experiment was air previouſly diſſolved by the diſtilled water, and liberated in conſequence of the ſtronger attraction of carbonic acid for that fluid; but as I had before found that in the common galvanic proceſs with the metals, the hydrogen was immediately evolved, even in boiled water, it was difficult to conceive why ſuch a length of time was required for the production of the inflammable gas. When I introduced charcoal connected with the zinc ſide, and ſilver wire connected with the ſilver ſide, into boiled water, gas was almoſt immediately given out from the wire; though when I connected ſilver with the zinc ſide, and charcoal with the ſilver ſide, no gas was liberated for many minutes.

4. A ſlip of charcoal was connected with the zinc end, and a ſilver wire with the ſilver end, and both plunged into a veſſel of lime water. Gas was immediately given out from the ſilver wire; a few globules only formed round the charcoal: they were apparently covered with a white cruſt. As the proceſs advanced, white clouds fell from the charcoal, and diffuſed themſelves through the fluid.

5. Two pieces of charcoal were connected with the ends of the pile, and plunged into a ſtrong ſolution of cauſtic pot-aſh. During two hours no gas was given out from either of the pieces, and no change of colour was perceived in the fluid, though the communication was perfect. When a ſilver wire was connected with the zinc, and charcoal with the ſilver, gas was produced from the wire, but none from the charcoal. When charcoal was connected with the zinc, and ſilver wire with the ſilver, gas was very rapidly produced round the point of the wire; but not an atom formed round the charcoal.

6. When the ſlips of charcoal connected with the pile were introduced into ſolution of ammoniac, gas was given out from the zinc charcoal; but none from the ſilver charcoal. When ſilver wire was connected with the ſilver ſide, the charcoal being ſtill connected with the zinc ſide, gas was given out from both, but moſt rapidly from the wire. Theſe gaſes were caught and examined. That from the charcoal gave no diminution with nitrous gas. An accident prevented me from examining it by other teſts; the gas from the ſilver equalled twenty times the volume of the other gas, and appeared to be pure hydrogen.

I ſhall, at preſent, offer no theoretical conjectures concerning theſe experiments. The two laſt will probably lead to intereſting concluſions. I am, at this moment, engaged in examining ſmall quantities of ſolution of pot-aſh and ammoniac which have been long galvaniſed in contact with charcoal: the reſult of this examination, if at all important, I ſhall take the liberty of communicating to you at ſome future time.

I remain,
SIR,
Very reſpectfully,
Your's, &c.

HUMPHRY DAVY.

Dowry Square, Hotwells, Sept. 22, 1800.

SCIENTIFIC

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

BOARD OF LONGITUDE OF FRANCE.

Prize of Aſtronomy.

THE tables of the moon are equally intereſting to aſtronomy and navigation. The moſt celebrated geometers have aſſiduouſly cultivated the theory on which theſe tables are founded. The moſt important care of the practical aſtronomer is to obſerve all the motions of this planet, without which there would be no truth in geography, and which furniſhes the navigator with the ſureſt means of knowing the ſituation of his veſſel, of directing his courſe, and ſafely arriving at any determined ſpot of the globe. In proportion as the Newtonian theory has been inveſtigated, and the greater the perfection to which inſtruments and the method of obſerving have been brought, in the ſame proportion have the lunar tables been improved. Mayer, joining his own reſearches to thoſe of the geometers of his time, and making choice of thoſe obſervations which were the moſt to be depended upon, ſucceeded in forming tables which, by ſubſequent compariſon with nearly twelve hundred unpubliſhed obſervations, agreed with the moſt aſtoniſhing preciſion. Maſon, under the direction of Dr. Maſkelyne, rendered them ſtill more perfect, by reſtoring ſeveral equations which had been omitted, though pointed out by Mayer, and by modifying the coefficients of all the others.

In ſpite of all this care, theſe tables, which in the middle of the century were ſo accurate, began progreſſively to loſe their exactneſs. On recurring to the theory, the cauſe of the error and its remedy were ſeen. The prize memoirs, which two years ago were ſent to the National Inſtitute, and were publiſhed in the ſeſſion of the fifteenth of *Germinal* laſt, have placed in the cleareſt point of view both the neceſſity and preciſe quantity of the equations lately diſcovered for the motions of the apogee and the node. Aſtronomers were not then invited to employ themſelves on all the elements which compoſe the lunar tables; the labour was too much diſproportioned to the time appointed for the concurrence. One ſucceſsful diſcovery often gives the wiſh and frequently the means of making another. That which has been ſo happily performed, has proved the poſſibility of doing ſtill more, and of procuring for aſtronomy lunar tables of greater preciſion as well as durability. Nothing more remains after the fixation of the epochs, the ſecular movements, and their inequalities, than to diſcuſs anew, by compariſon with a great number of accurate obſervations, the preciſe quantity of the different equations which enter into the calculation of the moon's place.

This is the problem which the Board of Longitude of France now propoſes for ſolution to the aſtronomers of all countries. The conditions are:

1. To diſcuſs, by compariſon with a great number of good obſervations, the value of the coefficients of the lunar inequalities, and to give for the longitude, latitude, and parallax of that luminary, formulas ſtill more accurate and complete than thoſe which have been uſed as the baſis of the tables at preſent in uſe.

2. To conſtruct, according to theſe formulæ, tables of ſufficient extent for the convenience and certainty of computation.

The prize is ſix thouſand franks (250l.)

The Board of Longitude appoints no term for the deciſion, but will award the prize to the firſt piece which ſhall fulfil the conditions of the program. It declares likewiſe to aſtronomers and geometers that it will not ceaſe to ſolicit the encouragement of the French government in favor of the perfection and theory of the tables of the moon.

The pieces muſt be ſent *au Bureau des Longitude*, Palais National des Sciences et Arts, à Paris.

They muſt not bear the name of the author, but merely a ſentence or device. If the author thinks fit he may ſend alſo a ſealed billet, containing not only the device, but the name and addreſs of the author, which billet will not be opened unleſs the piece itſelf ſhall have gained the prize.

The prize will be given without any formality to the bearer of the receipt which the ſecretary ſhall have given.

(Signed)

DELAMBRE, Preſident.
LALANDE, Secretary.

Tranſlation of a M S. Note of Cit. G. Cuvier, relative to Foſſil Subjects of Natural Hiſtory.

I am engaged on a large work upon the bones of quadrupeds found within the earth. I have already collected a large quantity of theſe bones as well from France as from foreign countries, and I have proved that ſeveral of them belong to ſpecies at preſent unknown to naturaliſts.

This is certainly the fact, not only with reſpect to thoſe foſſils which were before known, ſuch as the crocodiles or cetaceous animals of Maeſtricht, the elephants and rhinoceroſes of Siberia and Germany, the bears of Anſpach, the elks of Ireland; but alſo with reſpect to others which were firſt diſcovered by me, ſuch as the tapirs of the uſual ſize, and the tapirs of gigantic magnitude of France; the animals intermediate between the tapir and the rhinoceros, three ſpecies of which are found buried in the gypſum in the environs of Paris, whoſe figures accompany this note *, to convey an idea of them to thoſe perſons who may intereſt themſelves in my reſearches.

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* The engravings, which are five in number, and will undoubtedly compoſe part of the work of the learned author, could not, for many reaſons, be given with this tranſlation.---N.

Several learned men in Germany, Holland, and Italy, who approve the object of my labours, have ſupplied me with valuable information, accompanied with drawings relating to the foſſil bodies which are found in their reſpective countries, or which are depoſited in their cabinets. On account of the war, I have not, till now, had an opportunity of ſoliciting the like aſſiſtance from the philoſophers of England; but I hope I ſhall experience the ſame liberal aſſiſtance from them.

Under this ſuppoſition, I would requeſt an account of ſuch foſſil bones of quadrupeds, as may be contained in the Britiſh Muſeum, or in other collections in London; whether derived from Great Britain, or from other parts. I am particularly deſirous of thoſe which were buried in the mountain of Gibraltar, of which there muſt be numerous ſpecimens in London. I am ready to make my acknowledgements in return to thoſe who may thus favour me by informing them of every thing which can be intereſting to them in this country on the ſubjects of natural hiſtory and comparative anatomy.

I ſhall be ſtill more obliged if they will, at the ſame time, ſend me drawings of theſe foſſils executed in a manner nearly ſimilar to thoſe which I now ſend as ſpecimens of engraving, and I engage to defray every expence which may be occaſioned by ſuch drawings.

It is needleſs to obſerve that I ſhall, in my intended work, publicly acknowledge my obligations to thoſe who may have enriched it by their contributions.

G. CUVIER,
Member of the National Inſtitute,
and Secretary to the Claſs of
Natural Philoſophy.

Au Jardin des Plantes
à Paris. 15 *Therm.*
An. 8. (3 *Aouſt* 1800.)

Hydraulic Engine operating by Mercury.

The reſpectable correſpondent who has forwarded a deſcription and ſketch of an engine for raiſing water by mercury, has rightly conjectured that the expedient was uſed before. Mr. Joſhua Haſkins, at the beginning of the century, made an engine of this kind, which was improved by Dr. Deſaguliers, and is deſcribed very much at large in the ſecond volume of his Courſe of Experimental Philoſophy. The mercury is merely a ſubſtitute for leathering, and has leſs friction: but it has many diſadvantages, which are evident from that deſcription, particularly that it cannot with convenience be uſed when the column or re-acting force is great; and it may alſo be expected that the mercury would ſoon be converted into a black duſt or oxide, and carrried off in the water. I ſuppoſe this to have been the cauſe of its being abandoned.

Philoſophical Tranſactions of the Royal Society of London for the Year 1800. *Part the Firſt. Quarto,* 238 *Pages, with eight Plates, and a Meteorological Journal.* London, ſold by Elmſley.

This part contains:—1. The Croonian Lecture. On the Structure and Uſes of the Membrana of the Tympani of the Ear. By Everard Home, Eſq. F. R. S. 2. On the Method of determining from the real Probabilities of Life, the Values of contingent Reverſions, in which three Lives are involved in the Survivorſhip. By William Morgan, Eſq. F. R.S. 3. Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, for the Year 1798. By Thomas Barker, Eſq. 4. On the Power of penetrating into Space by Teleſcopes; with a comparative Determination of the Extent of that Power in natural Viſion, and in Teleſcopes of various Sizes and Conſtructions; illuſtrated by ſelect Obſervations. By William Herſchel, LL. D. F. R. S. 5. A ſecond Appendix to the improved Solution of a Problem in Phyſical Aſtronomy, inſerted in the Philoſophical Tranſactions for the Year 1798, containing ſome further Remarks and improved Formulæ for computing the Coefficients A and B; by which the Arithmetical Work is conſiderably ſhortened and facilitated. By the Rev. John Hellins, B. D. F. R. S. and Vicar of Potter's Perry, in Northamptonſhire. 6. Account of a Peculiarity in the Diſtribution of the Arteries ſent to the Limbs of ſlow-moving Animals; together with ſome other ſimilar Facts. By Mr. Anthony Carliſle, Surgeon. 7. Outlines of Experiments and Inquiries reſpecting Sound and Light. By Thomas Young, M. D. F. R. S. 8. Obſervations on the Effects which take Place from the Deſtruction of the Membrana Tympani of the Ear. By Mr. Aſtley Cooper. In a Letter to Everard Home, Eſq. by whom ſome Remarks are added. 9. Experiments and Obſervations on the Light which is ſpontaneouſly emitted, with ſome Degree of permanency, from various Bodies. By Nathaniel Hulme, M. D. F. R. S. and A. S. 10. Account of a Series of Experiments, undertaken with the View of decompoſing the Muriatic Acid. By Mr. William Henry. (Philoſ. Journal, IV. 209.) 11. On a new fulminating Mercury. By Edward Howard, Eſq. F. R. S. Ibid, IV. Appendix. Meteorological Journal kept at the Apartments of the Royal Society by order of the Preſident and Council.

Royal Society of Copenhagen.

The Royal Society of Copenhagen not having received anſwers to its queſtions on hiſtory, mathematics, and natural philoſophy, which were propoſed laſt year, repeat the ſame queſtions for the preſent year.

1. *Hiſtory.*—What people diſcovered America, and travelled into that part of the world before the Norwegians? How far did the diſcoveries of the Norwegians extend in America,

particularly towards the weſt? Proofs and conjectures derived from authors, or from exiſting monuments, ſuch as buildings, language, or the traditions of the Americans muſt be quoted.

2. *Mathematics.* To diſcover the function of all the quantities which, jointly, determine the magnitude of the effect of the caloric afforded by every kind of combuſtible made uſe of in manufactories, whether wood, pitcoal, or charcoal. When the equation is found, it muſt be determined in the four following caſes: 1°. When wood, pitcoal, or charcoal are burnt in a ſtove or veſſel to heat the confined air; for example of a room. 2°. When theſe materials are uſed to boil all ſorts of liquids over a fire place. 3°. When they are uſed to harden ſoft ſubſtances, as, for inſtance, to make bricks. 4°. and laſtly, when employed in melting metals.

Supported by experiments, the authors muſt diſcover and eſtabliſh the different equations analytically; in order that by their application, the effect and œconomy of each of theſe three kinds of combuſtibles, wood, coal, and charcoal, may be calculated.

3. *Natural philoſophy.* To find by experiments the greateſt degree of heat which aqueous vapours, when heated, can communicate to other bodies? Can that part of the water which, in the digeſter of Papin, is not reduced to vapour by the heat, acquire more than 212 degrees of Fahrenheit's thermometer?

4. The Society having had the ſatisfaction to crown an anſwer to its philoſophical queſtion for the preceding year, propoſes the following queſtion for the prize of the preſent year.

What new diſcoveries have been made in philoſophy ſince the time of Plato and Ariſtotle, in its reſearches into, and its explanation of, the nature of human knowledge with reſpect to exiſting beings?

The preſent queſtion here, is not to aſcertain what progreſs has been made in thoſe ſciences which treat exiſtances, whether corporeal or incorporeal; but it relates to our internal knowledge (which ſome philoſophers call *subjective*) to know what, in general, is the nature and the power of this knowledge, its origin and cauſe, the principles and reaſons upon which this manner of knowing is determined and eſtabliſhed, and upon which is founded that truth which is therein found, or imagined to be found. It is expected that an hiſtorical expoſition ſhould be given, of what has been produced on this ſubject by the meditations of philoſophers ſince the time of Plato and Ariſtotle; how much we are indebted to them for new diſcoveries, or for facts better eſtabliſhed and defined; or on the contrary, if it ſhall appear that philoſophy has not made any progreſs in this reſpect, it is expected that this ſhould be demonſtrated from the hiſtory of the dogmas of philoſophers.

The anſwers to theſe queſtions are to be written in Daniſh, in French, or in German, and forwarded with the uſual formalities, before the end of June 1801, to Profeſſor P. C. Abildgaard, Secretary to the Society.

Decompoſition of the fixed Alkalis.

Van Mons, in a Letter to C. de la Metherie, inſerted in the Journal de Phyſique, Vol. VII. p. 390, acquaints him, that *Craaner*, an apothecary at Amſterdam, has made an experiment, by which it appears that the alkalis contain carbon. He includes pot-aſh, or ſoda, moiſtened with water under an inverted glaſs with oxygen gas. The gas is abſorbed, and the alkali becomes effervеſcent. Moreover, he finds that an alkali, treated a certain number of times (*quelquefois*) with this gas, becomes exhauſted of carbon, and can no longer be made effervеſcent.

V. M. long ago publiſhed, that pot-aſh is formed during the combuſtion of hydrocarbon, and during the fermentation of grapes, by the intervention of the azote of the atmoſphere. He adds, that when theſe operations are made in pure oxygen, neither pot-aſh, nor the acidulous tartrite of that alkali are obtained; but the wine becomes too ſour to be drank. He conjectures, that the fixed alkali may be carbonated azote.

But he mentions two experiments which militate againſt this theory. In the firſt he expoſed cauſtic pot-aſh, and the red oxide of mercury by fire, to red heat in a retort, and obtained nothing but oxygen gas, nitric acid and water. In the ſecond, he triturated the oxymuriate of pot-aſh with cryſtals of cauſtic alkali, and poured the mixture into a phial, which he kept exactly cloſed for three days. At the end of this period he poured muriatic acid upon it, but had no effervеſcence.

As carbon is not combuſtible in the air but at a red heat, our author remarks, that if this principle enter into the compoſition of alkalis, it muſt be united with hydrogen, unleſs azote ſerves as the medium of its union with oxygen.

In the ſame Journal for Meſſidor VIII. I find a note, page 82, in which it is aſſerted, that Guyton Morveau, on the 6th floreal, read to the French National Inſtitute a memoir on the conſtituent principles of the fixed alkalis. He made his experiments jointly with Deſormes pupil of the polytechnic ſchool, and their concluſions are,

1. That pot-aſh is compoſed of lime and hydrogen.
2. That ſoda is compoſed of magneſia and hydrogen.
3. That lime is compoſed of carbon, azote, and hydrogen.
4. That magneſia is compoſed of lime and azote, and conſequently of carbon, hydrogen, and azote.

Separation of Butter.

Brugnatelli has ſeparated butter from cream without the aſſiſtance of oxygen. He pours four parts of hot water to one of cream, and paſſes the mixture through a ſtrainer of coarſe cloth in a cloſed veſſel, in which ſeveral ſtrainers of the ſame kind are ſtretched. In twenty-four hours the whey has paſſed the ſtrainers, and the butter is perfectly ſeparated.

J. de Phys. VII. 391.

Beet

Beet Sugar and ardent Spirits.

The National Inſtitute of France having named a commiſſion to repeat the experiment of Achard on Beet Sugar, Citizen Deyeux has lately made their report, the reſult of which is, that the beſt ſugar can be afforded at 15 ſous per livre, or 7d. per lb. avoirdupois. The Inſtitute therefore invites the Government to form an eſtabliſhment on a large ſcale for this important object of economy. Mag. Encycl. An. VIII. tom. ii. p. 97.

Brandy and arrack are made at Berlin from the refuſe of the beet root. Both are highly praiſed, and the former is on ſale in the market.

Fourcroy's Synoptic Tables of Chemiſtry.

The method of exhibiting the whole of a ſubject, with the relations of its parts to the eye in tables, is of moſt admirable uſe for the perſpicuity of communication, and its impreſſive effect on the memory. It ſeems as if the caprice of faſhion, which during the century nearly elapſed was inimical to every appearance of formality, had in a great meaſure ſuppreſſed theſe valuable helps to learning. The Tableaux Synoptiques of Fourcroy will be not only of the higheſt ſervice to the individual ſcience of chemiſtry, but may tend to ſhew the advantages of poſition and arrangement in other ſciences, which greatly want it. Theſe tables are printed on twelve large ſheets, twenty-two inches by thirty, and conſequently with the misfortune of two folds in each. Every ſheet contains one table, and the author's arrangement is governed by the chemical properties of bodies. The firſt table contains the generalities of the ſcience, its order, methods, hiſtory, diviſions, explication to medicine, &c. 2. The ſecond preſents (*a*) the firſt claſs of ſimple, or undecompoſed bodies, as light caloric, oxygen and azote, and others according to their order of combuſtibility, as hydrogen, carbon, ſulphur, diamond, and the metals; and (*b*) theſe bodies burned or united to oxygen, or the ſeries of oxides or acids, claſſed by their attractions to the principle of combuſtion, and the difficulty of decompoſing them. 3. The third exhibits firſt the ſalifiable baſes, namely earths and alkalis; and ſecondly, the firſt claſſes of ſalts. 4, 5, the fourth and fifth tables are alſo employed in the claſſification and deſcription of ſalts 6, 7, 8, 9, 10. The five following tables are engaged on the twenty one metallic ſubſtances hitherto diſcovered. 11, 12. and the eleventh and twelfth preſent ſketches of the vegetable and chemical chemiſtry.

Theſe tables may be conſidered as the ſynopſis of the *Syſteme général des Connaiſſances chimiques* of the author, now in the preſs, and after its appearance he will publiſh a fuller ſeries of tables on the ſubjects of the two laſt.

The preſent tables are on ſale at De Boſſe's, in Gerard Street, London.

Note

Note from a Correſpondent reſpecting the Bolognian Phoſphorus.

To Mr. NICHOLSON.

SIR,

I AM in poſſeſſion of ſome ſtones from Monte Paderno, near Bologna, of which the phoſphorus, called there Spongia di luce, and Pietra cuminabile, is made. I have conſulted all the books of chemiſtry which mention the method of making this phoſphorus, and ſeveral experienced chemiſts have exactly followed their directions, but in vain, as no one has been able to cauſe the ſtone to imbibe light.

Your inſerting this in your valuable Journal, may perhaps be the means of procuring ſome information on the ſubject, and the exact method of preparing it; likewiſe if it can be procured in London. I brought ſome from Bologna ſome years ago, but it ſoon loſt its property of retaining light.

I am, Sir,
Your humble Servant,

Sept. 24, 1800. R. T.

Extract of a Letter from Mr William Henry, dated Sept. 25, 1800, *to correct an Inference in his Paper on Galvaniſm.*

"The inference which I had drawn, from the experiments deſcribed in your Journal for Auguſt, reſpecting the decompoſition of the vegetable alcali, I had found to be erroneous, before the intereſting facts of Mr. Davy were known to me; and I had fully intended a recantation, along with an account of ſome other experiments on the ſame ſubject, in which I was engaged, above a month ago. Theſe, however, various circumſtances have prevented me from completing; and I think it proper, therefore, not to delay making an acknowledgment, and ſtating the cauſe, of my miſtake.

"The fact which firſt led me to ſuſpect, that I had drawn too haſty a concluſion, was, that the black precipitate proved, on examination, to be merely a metallic oxide, and not charcoal, as I had ſuppoſed probable. Varying, therefore, the circumſtances of the experiment, I tranſmitted the galvanic influence through liquid cauſtic alkali, without the contact of mercury, and the black powder then ceaſed to appear. The gaſes alſo turned out to be a mixture of hydrogen and oxygen, in the proportions, pretty nearly, that might be expected from water. This oxygen, the imperfect metals, contained in the quickſilver, had before prevented from appearing in a gaſeous form. From ammonia, the contact of mercury being excluded, gaſes were obtained correſponding, in kind and proportion, with thoſe ſtated in your Journal (p. 261), the quantity of oxygen gas appearing to diminiſh as the alcaline ſolution was more completely ſaturated."

Mr. Close's Apparatus for raising Water by its lateral Action.

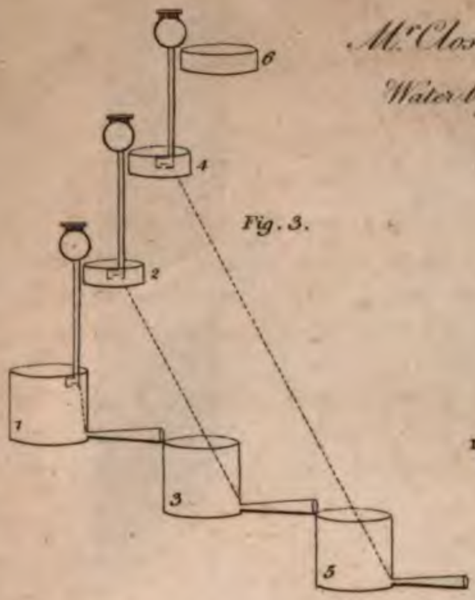
Fig. 3.

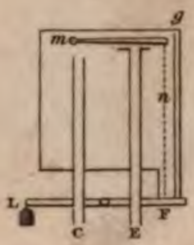
Fig. 2.

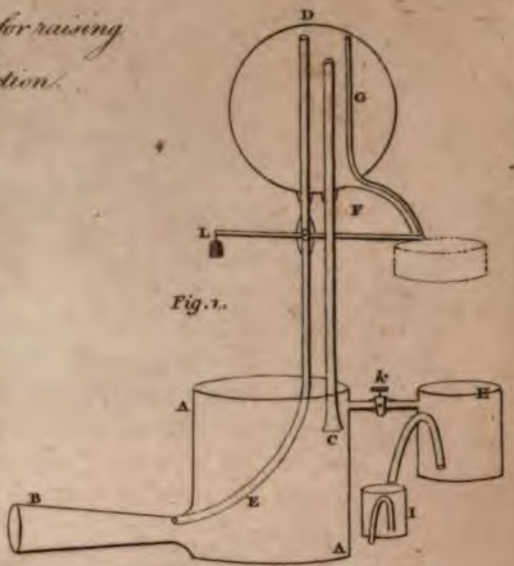
Fig. 1.

Dr. Wollaston's explanation of double images caused by atmospherical Refraction.

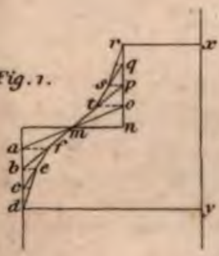
Fig. 1.

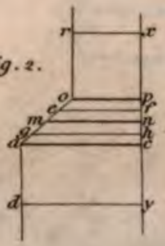
Fig. 2.

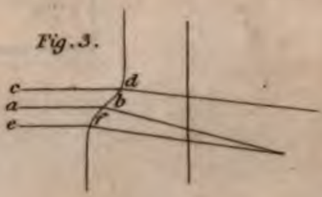
Fig. 3.

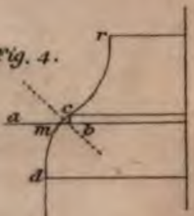
Fig. 4.

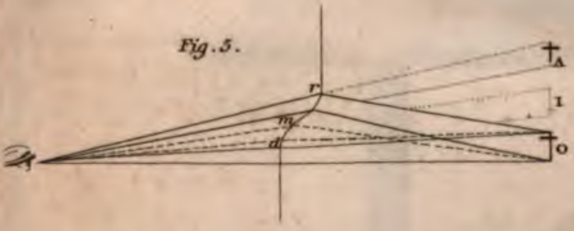
Fig. 5.

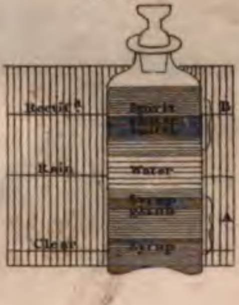
Fig. 6.

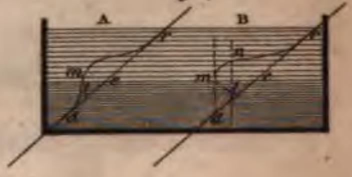
Fig. 7.

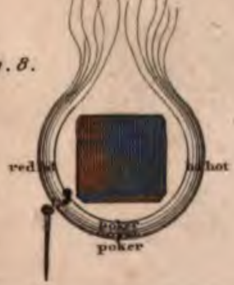
Fig. 8.

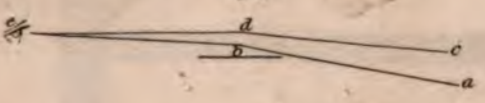
Fig. 9.

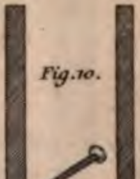
Fig. 10.

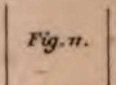
Fig. 11.

Mutlow Sculp.

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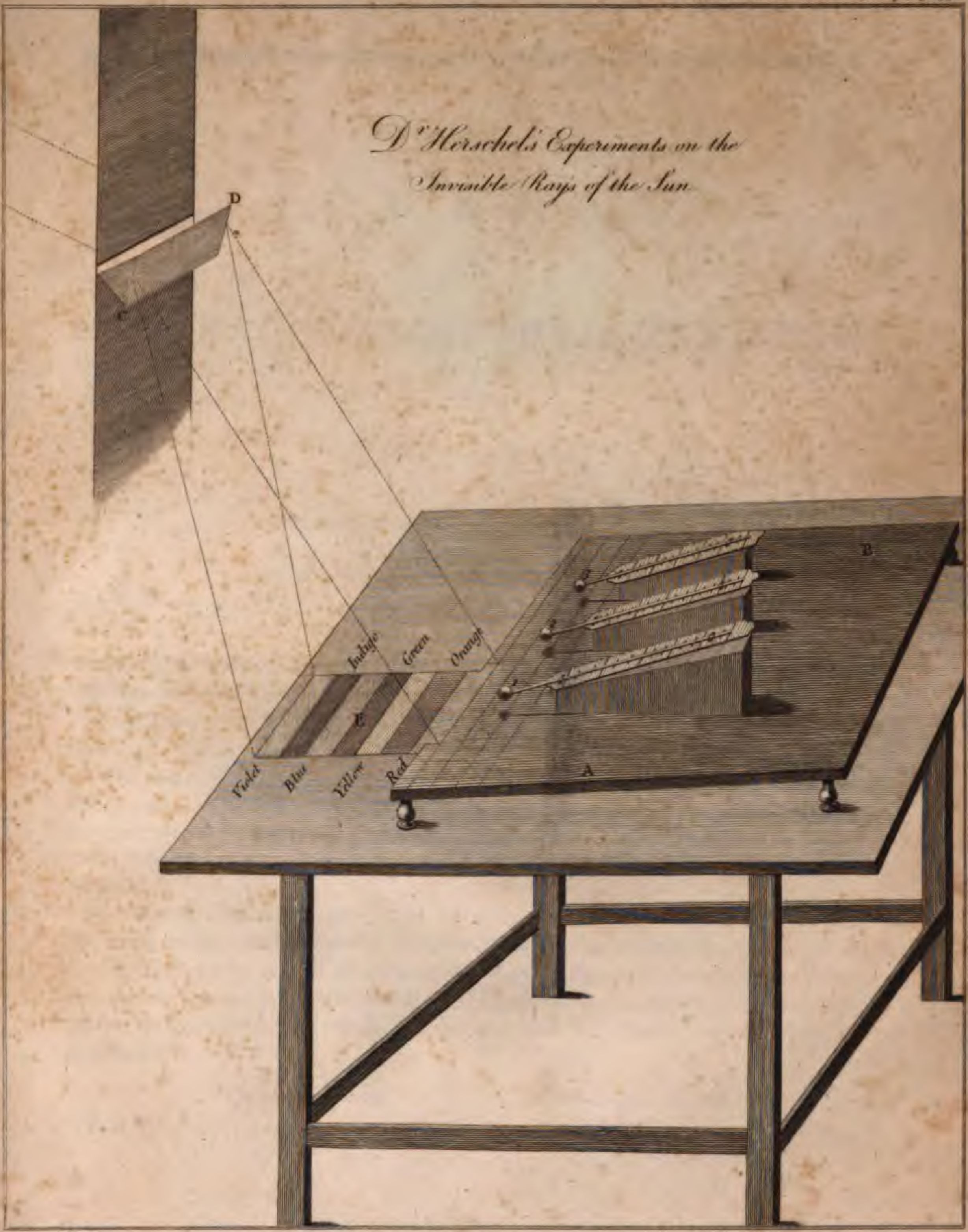

D^r Herschel's Experiments on the Invisible Rays of the Sun

Mutlow fc.

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JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER 1800.

ARTICLE I.

Notice of ſome Obſervations on the Cauſes of the Galvanic Phenomena, and on certain Modes of increaſing the Powers of the Galvanic Pile of Volta. By Mr. DAVY, *Superintendant of the Pneumatic Inſtitution. Communicated by the Author.*

THE nature of this communication is incompatible with a detail of the opinions prevailing amongſt philoſophers, reſpecting the cauſes of the galvanic phenomena: they have been generally ſuppoſed to depend on the different powers of bodies to conduct electric fluid; Fabroni was the firſt who ſyſtematically attempted to prove that they were chemical effects[*].

Immediately after I had peruſed an intereſting obſervation of Lieutenant-Colonel Haldane[†] on the non-excitement of galvaniſm in the vacuum of an air pump, I began an inveſtigation with the view of aſcertaining preciſely the influence of the atmoſphere on the phenomena. In carrying on this inveſtigation, I have met with ſome new facts, which are capable of arrangement, and which will probably lead to a complete explanation of the galvanic effects.

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[*] Phil. Journal, III. 308.

[†] Phil. Journal, IV. 242.

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[*] Phil. Journal, III. 308.

[†] Phil. Journal, IV. 242.

The piles that I employed for aſcertaining the influence of factitious airs on the galvanic phenomena, were erected horizontally in the uſual mode; but to prevent the plates from ſeparating when in an oblique poſition, their ſides were joined together by reſinous cement at two or three points, ſufficient interſtice being preſerved to admit of a free circulation of air. The gaſes, when any were produced, were received in ſmall tubes filled with diſtilled water, containing wires covered externally with wax, and communicating with the ends of the pile. The piles were introduced into the airs through water [*], and elevated above the water by a metallic plate cemented to their lower extremities.

1. *Zinc, whether connected with Silver in ſingle Galvanic Circles, or conſtituting the Plates of the Galvanic Pile, ſeems to undergo no Oxidation at common Temperatures, as long as the Water in contact with it is pure.*

By pure water, is more immediately meant water holding in ſolution no oxygen gas, no nitrous gas, and no acids. It has long been known that certain metallic bodies, which oxidate ſlowly in water expoſed to the atmoſphere, effect no change in pure water [†]: this I have obſerved is particularly the caſe with regard to zinc. That zinc, when in contact with ſilver in the atmoſphere, and forming with it a circuit by means of water, becomes oxidated much more rapidly than when ſimply in contact with water, was obſerved by Dr. Aſh [‡].

Suppoſing the more rapid oxidation to be the effect of a peculiar electrical influence produced by the contact of the metals, it would be reaſonable to conclude, that zinc in ſingle circles with ſilver and pure water, or at leaſt in the galvanic pile erected with cloths moiſtened in pure water, would undergo oxidation. Fabroni [§], however, has advanced that ſingle galvanic circles do not oxidate in water, unleſs it be expoſed to the atmoſphere. That the zinc of the galvanic pile does not oxidate in contact with pure water, will appear from the following obſervations:

a. A ſmall pile of ſilver and zinc, having its paſteboards moiſtened with water, that had been juſt boiled, was introduced into a veſſel of water that had been long boiling, and was yet warm. Reſinous cement was poured upon the ſurface of the water, and faſtened to the glaſs as it cooled, to preſerve it effectually from the contact of air [||]. The apparatus, after remaining for two days, was examined; the zinc plates were ſcarcely at all tarniſhed; no oxide was depoſited in the water, and no gas had been evolved through it. A ſimilar pile, expoſed for nearly the ſame time to water in contact with the atmoſphere, had depoſited much white oxide, and given out ſome gas, and the zinc plates were whitened both internally and externally *.

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[*] Before theſe experiments were made, I had found, by numerous trials, that a pile acted in the atmoſphere immediately after its immerſion in water without being wiped, though more feebly than before: I had likewiſe found that after the firſt immerſion, the powers were not diminiſhed by ſubſequent ones.

[†] See Fabroni, Phil. Journ. III. 309.

[‡] And Humboldt, ſee Reſearches Chem. & Phil. page 568.

[§] Phil. Journ. III. 309.

[||] Oil of turpentine, and even common oil, as will be ſeen hereafter, is ineffectual for this purpoſe.

b. No influence can be communicated from an elaſtic atmoſphere, enabling the zinc of the galvanic pile to decompoſe pure water; for piles were introduced into hydrogen, nitrogen, nitrous oxide, and hydro-carbonate, and ſuffered to remain in them for different lengths of time; but in none of theſe gaſes was the zinc more oxidated than if the pile had been immerſed in pure water.

c. A galvanic pile was ſuffered to remain in vacuo for fourteen hours, the gage being about $\frac{9}{10}$, the zinc plates were ſlightly tarniſhed, but no white oxide had formed upon them.

2. *The Oxidation of the Zinc Plates of the Galvanic Pile takes place whenever the Water in contact with them holds Atmoſpheric Air, or Oxygen, or Nitrous Gas, or Nitrous Acid, or Marine Acid, &c. in Solution.*

a. The oxidation of the zinc of the pile in the atmoſphere, and the protuſion of carbonate of ſoda, has been accurately deſcribed by Meſſrs. Nicholſon and Carliſle. I have found that the phenomena of oxidation takes place much more rapidly in pure oxygen than in the atmoſphere. I have likewiſe found that it takes place more ſlowly in nitrous gas than in the atmoſphere. That zinc is oxidated in ſolutions of nitrous and marine acids, is a well known fact.

3. *When the Zinc in contact with Water, holding in ſolution Subſtances containing looſe Oxygen, or Acids, is oxidated, theſe Subſtances are altered, or they exert ſome Chemical Affinities.*

a. A ſmall galvanic pile, expoſed to about twenty-one cubic inches of oxygen gas for ſix hours, effected a diminution of it which might have equalled one-fourth.

b. A ſimilar pile, expoſed to an equal quantity of atmoſpheric air for two days, diminiſhed it one-ſixth: the reſiduum being ſuffered to paſs into the atmoſphere, gave a ſmell which ſtrongly reſembled that of ammoniac; and the pile held over marine acid produced denſe white clouds.

In another experiment, an equal quantity of air, expoſed to the pile for three days, gave but a very ſlight diminution with nitrous gas, and could not be inflamed with atmoſpheric air.

c. I have noticed, *Reſ. Chem. & Phil.* that wetted zinc, placed in contact with nitrous gas over mercury, ſlowly converts it into nitrous oxide and ammoniac. A ſmall pile, expoſed to about twenty-two cubic inches of nitrous gas for three days, diminiſhed it one-fourth, and ſome of the gas was rendered abſorbable by water.

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* The temperature in theſe experiments was from 54° to 61°. It is impoſſible to free water of *all* its diſſolved air by boiling: it is likewiſe impoſſible to prevent a few globules of air from paſſing through the cement whilſt it is cooling: the very ſlight tarniſh of zinc kept in boiled water may be owing to the minute quantities of common air exiſting in the water from both theſe cauſes.

d. The formation of nitrous gas, nitrous oxide, and ammoniac, when very weak ſolutions of nitrous acid are made to oxidate zinc, demonſtrate both the decompoſition of the acid and the water.

e. The oxidation of zinc in ſolutions of marine and ſulphuric acids in water, appears to be owing to the affinity which has been called prediſpoſing.

4. *The Galvanic Pile of Signor Volta ſeems incapable of acting when the Water between the Pairs of Plates is pure.*

The word *pure* is here uſed in the ſame ſenſe as in 1.

a. I have found, by numerous experiments, that the galvanic pile introduced into hydrogen, through common water, ceaſes to evolve gas in the tube, and to act in about five or ſix minutes, *i. e.* in about the time required to conſume the atmoſpheric air diſſolved in the common water between its plates. The phenomenon is exactly the ſame in nitrogen, nitrous oxide, and hydro-carbonate. The action of a pile, as known by its power of evolving gaſes from water, is diminiſhed immediately on its introduction into thoſe gaſes. It ceaſes in them nearly in the ſame time, and cannot be reſtored by admitting freſh gaſes of the ſame kind, though, as will be ſeen in the next ſection, it is immediately reſtored by immerſing the pile for a moment in water ſaturated with atmoſpheric air.

b. I have found, by numerous experiments, made with the aſſiſtance of my friend, Mr. King, that the galvanic pile ceaſes to act in vacuo when the gage is at about $\frac{6}{10}$ of an inch, even though the poles are connected by wires with the atmoſphere, and the water it is made to act upon in contact with the atmoſphere.

5. *The Pile acts when the Water between the double Plates holds in Solution Atmoſpheric Air, or Oxygen, or Nitrous Gas, or Nitrous Acid, or Marine Acid.*

This fact I have proved by numerous experiments.—*a.* A pile acted in atmoſpheric air, included in a glaſs cylinder over water for two days, till nearly all the oxygen of the air was conſumed.—*b.* A pile decompoſed water much more rapidly in oxygen than in common air, and leſs rapidly in nitrous gas than in common air.—*c.* That the influence of an elaſtic oxygenated atmoſphere is not eſſential to the galvanic effects, is evident from the fact of the conſtant reſtoration of the powers of a pile after they had ceaſed to appear in the hydrogen, nitrogen, &c. by momentary immerſion in water ſaturated with atmoſpheric air. In theſe experiments the piles were plunged into the water confining the gaſes, and again immediately elevated into the gas without being expoſed to the atmoſphere: and the phenomenon could be owing to no other cauſe than the impregnation of the water with atmoſpheric air, becauſe when piles were plunged into water ſaturated with nitrous oxide*, their

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* This gas expels much common air from water; ſee *Reſearch. Chem. & Phil.* R. II. S. 2.

their powers were not reſtored.—*d.* I have proved by many experiments [*] that water, deprived of air, is capable of attracting it from the ſpirit of turpentine when the laſt is in contact with the atmoſphere. The galvanic pile acts in ſpirits of turpentine for a great length of time, and nearly as well as in the atmoſphere, the water between its plates being conſtantly ſupplied with air from the ſpirits. It acts but for a ſhort time in ſpirits of wine, on account of the combination of this fluid with its water.—*e.* After a pile had ceaſed to act in hydrogen, its powers were uniformly reſtored by a momentary immerſion in very diluted marine acid. They were likewiſe reſtored, and rendered more intenſe than in the atmoſpher ebymomentary immerſion of the pile in diluted nitrous acid, though they continued to be exerted for a ſhort time only, *i. e.* till the acid was decompoſed or ſaturated.

6. *The Power of Action of the Pile of Volta appears to be proportional to the Power of the conducting fluid Subſtance between the double Plates to oxidate the Zinc.*

This ſeems ſufficiently proved from the facts in the following ſections. The zinc oxidates leſs rapidly in nitrous gas than in atmoſpheric air, and leſs rapidly in atmoſpheric air than in oxygen: and the power of action of the pile as known by its evolving gas from water is greater in oxygen than in atmoſpheric air, and greater in atmoſpheric air than in nitrous gas. The power of the pile to decompoſe water, and to give the ſhock is wonderfully increaſed after it has been dipped in marine acid, and ſtill more increaſed after it has been dipped in weak nitrous acid; and theſe bodies only enable the zinc to oxydate itſelf more rapidly. A ſeries of plates, in which the oxidating conducting fluid was ſtrong nitrous acid, acted, as will be ſeen hereafter, infinitely more powerfully than any other combination; ſo that it would ſeem that the power of a pile is not much connected with the evolution of hydrogen from water.

7. *Concluſions.*

Of two phenomena, or of two ſeries of phenomena, we can only affirm that the one is the cauſe of the other when it uniformly precedes it, and when their modifications are connected. But it appears from all the foregoing facts, that the galvanic pile of Volta acts only when the conducting ſubſtance between the plates is capable of oxidating the zinc; and that in proportion as a greater quantity of oxygen enters into combination with the zinc in a given time, ſo in proportion is the power of the pile to decompoſe water, and to give the ſhock greater. It ſeems therefore reaſonable to conclude, though with our preſent quantity of facts we are unable to explain the exact mode of operation, that the oxidation of the zinc in the pile and the chemical changes connected with it are *ſome how* the cauſe of the electrical effects it produces.

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[*] Theſe experiments will be hereafter detailed.

8. *Of a new Mode of conſtructing a Pile.*

Aſſuming the truth of this concluſion it was eaſy to conceive, that a pile much more powerful than any hitherto conſtructed might be made, particularly ſuppoſing that the decompoſition of water was not eſſential to the proceſs, plates of zinc and ſilver, 1.2 inches ſquare, were faſtened in pairs by reſinous cement: eighteen of theſe pairs were connected to each other by cement, and ſo incloſed by it as to leave water-tight partitions open at one ſide only between each pair of plates. When muriatic acid was poured between the partitions of this machine, the plates being perpendicular, it acted very powerfully; its capability of decompoſing water and giving the ſhock, being at leaſt equal to that of a common pile of ſeventy plates. Diluted nitrous acid made it act ſtill more powerfully. When the partitions were filled with water, its action was barely perceptible. Concentrated nitrous acid was poured into them. In this caſe the firſt ſhock was ſo powerful as to benumb my fingers for ſome ſeconds, and I did not dare to take another. I was almoſt immediately obliged to throw the pile into water to prevent it from being deſtroyed, ſo that there was no time to aſcertain its power of decompoſing water.

In a ſecond experiment, with ſtrong nitrous acid, I uſed only five pairs of plates, when the ſhock was full as powerful as from the common pile of thirty plates.

Three pairs of plates, with nitrous acid, gave a very ſenſible ſhock.

I have procured (on account of the loſs of the ſilver when this ſubſtance is part of the pile with nitrous acid) a number of plates of copper, iron, and zinc. They have not yet been diſpoſed in the apparatus; but I expect by means of nitrous acid, to produce effects from them, equal to thoſe of the ſtrongeſt electrical battery.

II.

*On raiſing Water by the Engine** *of H.* Goodwyn, *Eſq. through double or treble the Space of the deſcending Column, and on the proper Arrangement to make it require no Attendance. By a Correſpondent.*

To Mr. NICHOLSON.

SIR,

AFTER conſidering various methods of raiſing water with little trouble and expence, I am decidedly of opinion that Mr. Goodwyn's engine will anſwer this purpoſe the beſt. It is formed upon a very elegant principle, and operates by the aſſiſtance of only a ſmall quantity of water: it may be made in various forms, either to raiſe the fluid above the deſcending column, or from below it to a level with the bottom; and the height may be doubled or trebled, by proportionably encreaſing the deſcending maſs, and raiſing ſeveral columns of water from different elevations at the ſame time.

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* Deſcribed by the inventor, in this Journal IV. 163.

To extend therefore the utility of the invention by illuſtrating what has been here premiſed is the intention of the preſent communication.

A and B, in Pl. XV. Fig. 1. repreſent the veſſels which contain the riſing and deſcending bodies of water: they are ſpherical in the original drawing, (Pl. VIII.) but to leſſen the loſs of ſpace in deſcent, they are here made flat and cylindrical.

E the higher, and C the lower ciſtern of the original figure.

F, a veſſel the ſame as A, with tubes 1, 3, and 6; it communicates with the veſſel B, by the tube 1, and is intended to raiſe water out of the ciſtern E into a higher and additional ciſtern G.

2. enlarged tube of the original drawing.

5. hole in the top of B, inſtead of a tube. This hole and the tubes 2, 4, 6, 6, muſt be provided with valves, all which, while water is riſing muſt be kept cloſe by weights or ſprings, except the valve to tube 2, which muſt be open.

The tubes 3, 3, may alſo have valves to ſupport the raiſed columns.

To make the engine operate without attendance, a veſſel containing a ſyphon muſt be fixed to the ſide of the ciſtern C, and ſupplied with a proper quantity*. Every external valve, except the loweſt, muſt have a lever joined to it; and while cloſing their reſpective tubes, they ſhould be connected together, and to the lever of the valve to pipe 4, which muſt ſuſpend another empty veſſel and ſyphon.

The valve of pipe 2, muſt alſo be ſuſpended by a chain or wire from the underſide of valve to pipe to 4, and when one cloſes the other muſt open.

Fill now the ciſterns C and E with water, and let the lower be conſtantly ſupplied: the veſſel on the outſide will ſoon become full, and the ſyphon will empty the water into the lower veſſel, which, by its weight will open the valves of the tubes 4, 5, 6, 6, and cloſe the valve of tube 2: the veſſel B, being filled through the hole 4; the deſcent of water through the higher ſyphon having ended; and the lower veſſel alſo being nearly emptied by its ſyphon; the valves will cloſe the tubes 4, 5, 6, 6, and the valve of tube 2 will open; the water will then begin to deſcend out of B, and raiſe one body of water out of C into A, and another out of E into F. When B is nearly empty, or A and F are full, water will again deſcend through the ſyphon of the higher veſſel into the lower, which will open the tubes 4, 5, 6, 6, and cloſe 2: B will fill a ſecond time, and the veſſels A and F will empty themſelves into their reſpective ciſterns E and G. And thus the reciprocations continue without interruption.

Another body of water may be raiſed out of G into a higher ciſtern by additional apparatus, and by proportionally encreaſing the dimenſions of B and the tube 2.

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* The author evidently means to refer to the papers of Mr. Boſwell and Mr. Cloſe, at pages 117 and 293 of our preſent volume. N.

The dotted lines repreſent apparatus for raiſing water below the bottom of tube 2, to be uſed inſtead of thoſe above the ciſtern C.

This arangement places the utility of the engine in a very ſtriking point of view, particularly when we wiſh to employ a ſmall ſtream to operate upon machinery; where the deſcent is great, but the quantity of water inadequate to the purpoſe even with the aſſiſtance of a reſervoir. In this caſe let nearly the whole of the water deſcend through the veſſel B and tube 2, before it enters the reſervoir; and let the water after it has turned the wheel be collected in a lower baſon. If the deſcent from the higher reſervoir be ſo great as to allow part of the lower body to be raiſed into it again at once, the quantity raiſed may be made nearly equal to that which has deſcended through the tube 2; if it can be done at twice, it will be leſs than half that quantity. Still however I think it will be very advantageous to employ ſuch a method, as the operation of the engine is conſtant and diminiſhes no power of the machinery[*].

In ſituations where this machine can be erected it may be of conſiderable uſe, for raiſing water out of mines, for draining pieces of ground, or elevating the water employed in domeſtic purpoſes.

I am Sir,
Your humble Servant,

Sept. 16, 1800. L.

III.

Obſervations and Experiments on Light and Heat, with ſome Remarks on the Enquiries of Dr. Herſchel, reſpecting thoſe Objects. In a Letter from Mr. John Leslie.

To Mr. NICHOLSON.

SIR,

London, *Oct.* 9, 1800.

I AM induced to trouble you with this communication in conſequence of the account inſerted in your laſt number, of two papers lately read at the royal ſociety. Aware that the celebrity which their author has ſo juſtly acquired as a ſkilful and indefatigable obſerver will, from the indolence of our habits, confer a certain extrinſic weight, even when he embarks in philoſophical ſpeculation,—I cannot help feeling, anxious, leſt *that* authority ſhould in the preſent inſtance retard the progreſs of ſcience by giving currency to opinions which, I am fully convinced, are inacurate or unfounded. It is not my intention to cenſure the efforts of genius; but I would ſuggeſt how liable are men of talents to commit miſtakes in matters of experiment, eſpecially if they diſregard analogy and the general concurrence of eſtabliſhed facts. It would appear that this able aſtronomer, on entering into a new line of enquiry has neither employed apparatus ſuited to the nicety of the ſubject, nor guarded ſufficiently againſt the numerous and latent ſources of error. I conſider myſelf entitled to ſpeak with the greater confidence, becauſe I had long directed my reſearches in the ſame channel, and had fortunately hit upon a ſimple contrivance, improved ſince by perſevering attention which afforded to ſuch inveſtigations a degree of preciſion and facility not hitherto attained. I do not heſitate, therefore, to maintain that Dr. Herſchel's capital propoſition originates in fallacious obſervations. To ſupport this aſſertion I will briefly and with reluctance ſtate the reſult of my enquiries, and will then offer ſome remarks or ſtrictures on the article in queſtion.

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[*] The reaſoning in this paragraph appears to be defective, becauſe the machine engages part of the fall, and thus diminiſhes the effect more than the raiſed water can augment it. N.

In deſcribing my *photometer* I noticed, in a curſory manner, the ſeveral points in optics and chemical philoſophy for the examination of which it was, by its peculiar delicacy, ſo happily calculated. It was not then my deſign to enter into details, or relate the ſucceſs of the experiments. Thoſe intereſting deductions I reſerved for a work which has been ſeveral years in contemplation, but of which I have deferred the publication from time to time as the materials accumulated, from a deſire ſtill to extend and correct my views, and perhaps from a growing indifference to the voice of public opinion, which I am ſorry to obſerve is, in this country at leaſt, notoriouſly ſervile and undiſcerning. Nor will I at preſent anticipate more than what immediately concerns the ſubject under diſcuſſion.

After conſtructing the inſtrument one of my firſt objects was to determine, by their calorific effects, the relative intenſities of different colours. The problem had indeed been attempted before, by Franklin, Watſon, and others; but from ſuch rude trials nothing in any degree accurate could be expected. I therefore coated the abſorbent ball of a photometer ſucceſſively with a ſeries of water-colours, and, after allowing it to dry, I placed it with another photometer in the ſame ſteady light, whether expoſed to the diffuſive influence of the ſky, or to the direct impulſion of the ſolar beams. The ratio of the quantities marked by the two inſtruments gave the proportional abſorption of the coloured ſubſtance, and conſequently its power of reflection. Inſtead of paint, I ſometimes covered the ball with bits of coloured ſilk; and when two colours ſhowed nearly equal effects, I had recourſe to an expedient which rendered their difference very ſenſible. I made the balls of the photometer larger than uſual, brought them to the ſame elevation, and bent them back in oppoſite directions; the glaſs-caſe conſiſted of the larger ſegment of a hollow ſphere ſurmounting a cylinder; the zero of the ſcale was placed about the middle of one of the branches, and the diviſions were marked both in aſcent and deſcent: on applying the approximate colours to thoſe counteracting balls the difference, whether in exceſs or defect, of their abſorbent powers was eaſily determined. Thoſe experiments were likewiſe varied by reflecting the rays of the ſun, at a given angle from a coloured ſurface, againſt the dark ball of a photometer. I need not deſcend to particulars, but I was ſtruck with the copious reflection from ſcarlet, little inferior to that from a white ſurface. Blue, and next to it green, reflected the ſmalleſt quantities of light. Hence the explanation of ſeveral facts which occurred in the application of the photometer. Expoſed to the ſun-beams only, it indicated about one hundred degrees at noon during the ſummer months in our climate; ſheltered from thoſe direct rays it gives ſeventy or eighty degrees, when the ſky is covered with fleecy clouds, but only ten or fifteen degrees if the atmoſphere is clear and of azure hue. On the ſummits of elevated mountains, the ſun would be more reſplendent while the vault of heaven, of the deepeſt blue, would emit a proportional feeble light. The red gloſs of a fire was found to make at leaſt three or four times as much impreſſion as the flame of a candle, under the viſual angle. And language, that grand monument of human thought and feeling, teſtifies the conformity of thoſe remarks with common ſentiment. Red and orange are termed *warm* tints, and we admire the *glow* of the Italian landſcape. Among the colours ſcarlet is reckoned glaring, green, ſoft, and blue, faint. The pleaſurable ſenſation excited by the ſight of green, independent of its aſſociation with verdure and vegetable life, has been referred to the circumſtances of its occupying the middle rank of the ſyſtem of colours. But green has more affinity to the dark and abſorbent ſhades, and the eye ſeems more gratified with deficiency than exceſs of light.

On a haſty trial, I perceived that, the coloured ſpaces of the priſmatic ſpectrum manifeſted analogous properties; but as the quantities were ſmall and required nicer obſervation, and as the ſeaſon was already far advanced (it was in October 1797), I reſolved to poſtpone the whole till the next ſummer. I prepared accordingly to reſume the inveſtigations with every advantage; but I am ſorry to ſay that my plan was not accompliſhed ſo ſoon as I expected, owing partly to our variable ſky, ſo diſcouraging for optical experiments, and partly to various unforeſeen interruptions, eſpecially that of undertaking a tour through the north of Europe. In the beginning of laſt ſummer, however, my wiſh was realized, yet not without a tedious waſte of time; for ſuch is the nature of this inſular climate, that the ſky at mid-day is ſeldom cloudleſs, and, even then, its azure is generally adulterated by a milky haze. I cut a broad horizontal ſlit in the window-ſhutter, ſufficient to admit freely an excellent priſm of flint glaſs, which was fixed with wedges at the ends in a poſition to produce the greateſt refraction. Behind was placed a ſtand about $1\frac{1}{4}$ inches broad, $\frac{3}{4}$ of an inch thick, and of the height of 5 or 6 feet, to which an arm projecting 2 or 3 inches from the edge was adapted to ſlide upwards or downwards at pleaſure, and to this the photometer was attached; and, to avoid as much as poſſible every chance of deception, I annexed to that arm a ſlender branch ſtretching three or four inches towards the window, and bearing a ſmall ſcreen capable of being raiſed or depreſſed as occaſion required, ſo as to intercept at a diſtance the whole of the coloured rays but thoſe ſubmitted to examination.

I ſhall omit mentioning other precautions uſed; to thoſe who are acqainted with accurate experimenting it would be ſuperfluous, and to the reſt of your readers it would appear diſguſtingly minute. The ſtand was approached as near to the window as was compatible with the developement of the ſeveral coloured ſpaces, which was at the diſtance of about two feet; and though the ſhutter was cloſed, ſufficient light entered by the edges of the priſm for obſerving the inſtrument. In two or three minutes, as the colour travelled along the black ball, the fluid generally attained the point of equilibrium. Photometers of both conſtructions were employed with ſimilar reſults. The obſervations were frequently repeated, and from the mean of them all it appears; that, diſtinguiſhing the ſpectrum into four equal portions, which coincides with the blue, the green, the yellow, and the red the correſponding intenſities or calorific meaſures, are 1, 4, 9, and 16 degrees, as in fig. 2. Pl. XV. Hence the gradation of thoſe intenſities will be denoted by a parabola referred to its vertical tangent. A variety of curious propoſitions may hence be deduced. Thus, ſince the complimentary ſpace of the parabola is the third part of the circumſcribing rectangle, it follows that the intenſity of the red rays ſeparated by a priſm of flint glaſs is triple that of the compound white beam; and for the ſame cauſe the blue rays are more than five times feebler than the common ſtandard. It is not without reaſon that I ſtate the particular ſort of priſm uſed, becauſe with one of another ſubſtance the colours might be expanded after proportions materially different, which would of courſe give different reſults; for the force of a pencil of coloured light muſt evidently depend on the denſity, as well as on the ſpecies of the rays.

The important diſcovery of the achromatic teleſcope deciſively evinces that refrangibility is not a property inherent eſſentially in the particles of light, but reſults from the ſpecific relation or affinity between thoſe various particles and the refringent medium. In what proportions the ſeveral rays enter into the compoſition of the ſolar beam has not been determined, nor does any mode of ſolving the problem eaſily occur.

It is remarkable that all what is really valuable in Newton's optical diſcoveries was announced at an early age, but encountered ſuch oppoſition or experienced ſuch neglect, as to affect that great man with chagrin, and to extinguiſh completely the deſire of communicating his views at large to the world. Nor did he reſume that taſk till near the cloſe of life, when worn down with infirmities, and his choiceſt faculties waſted in the compoſition of laborious works, which, for his credit with poſterity, had better been ſuffered to reſt in oblivion. Yet even theſe (ſuch is the effect of exalted reputation!) were now accepted with indiſcriminate applauſe. Had his treatiſe of optics been publiſhed at an earlier and happier period, it would certainly have been more ſober and correct. The ſeptenary and muſical diviſion of the priſmatic ſpectrum is devoid of foundation, and too plainly betrays a tincture of the myſticiſm of the age. It is equally ſtrange and mortifying to obſerve the moſt objectionable part of that ſyſtem, and which is directly confuted by the theory of achromatic glaſſes, ſtill repeated in popular books, and even admitted by ſome authors of a higher claſs.

While I was occupied with thoſe obſervations in Fifeſhire, ſome vague accounts reached me of a paper communicated to the Philoſophical Society at London. And whatever my ſentiments were reſpecting the validity of the concluſions, I reſolved calmly and impartially to ſubject the pretended facts to the teſt of experiment. When a photometer

tometer was placed beyond the ſpectrum, though it approached the margin, whether above, below, or at the ſides, no effect whatever was perceived. And as that inſtrument greatly ſurpaſſed, in ſenſibility and regularity of performance, the fineſt thermometer, it is only what might be expected, that I could not perceive any alteration, if I employed thermometers of uncommon delicacy and of various conſtructions, with black or pellucid bulbs, having their ſcales ſubdivided into tenths of a degree, and capable of indicating ſtill ſmaller differences. There is a circumſtance, however, which being overlooked, might lead to egregious errors. If the ſpectrum be received on the ſtand, the inſtrument brought near it will be very ſenſibly affected; owing partly to the light reflected, but chiefly to the action of the heated air accumulated over the illuminated ſurface. In ſhort, the operations are deranged in every caſe where the inſtrument is not completely inſulated, and is remote from all ſolid materials which might detain the light. And it is worthy of remark, that the afflux of light muſt raiſe the temperature of a flat ſurface of imperfect conducting ſubſtance, ſuch as wood or paſteboard, more than four times as much as it will the blackened ball of a thermometer; for, in the former caſe, the heating and cooling cauſes are exerted on the ſame ſpot, but in the latter, the quantity of light or heat received is only as the central ſection of the ball; while the whole ſurface, which is quadruple that ſpace, is expoſed to the cooling influence of the ambient air. There are other circumſtances, too, which tends to augment that difference of effect, but which I ſhall not ſtop to mention. On the whole it is moſt difficult, where an active ſource of heat exiſts, to obtain an uniform temperature, particularly in a ſmall room; and I might point out cauſes, not yet apprehended, which will notably affect a thermometer.

Theſe hints will ſerve as a clue for detecting Dr. Herſchel's miſtakes. The firſt circumſtance that begets ſuſpicion is the large quantities marked by his thermometers, and which are not much inferior to the full force of the ſun-beam. To procure the colours of the ſpectrum diſtinctly evolved, the ſolar image muſt be extended to ten times its breadth; nor can it be imagined that rays ſo attenuated could produce ſuch palpable effects. The Doctor, indeed, admits that a ſmall thermometer was leſs affected. Yet it is obvious that, if the action was due merely to the afflux of light, the ſize of the ball ought to occaſion no alteration, ſince, if it preſents leſs ſurface to the beam, it likewiſe, in the ſame ratio, expoſes leſs ſurface to the ſurrounding air. A very curious reaſon is aſſigned—"the *cooling* agency of the *aſcending* ſtream of air, which would act more "ſtrongly where the proportion of ſurface to bulk was greater[*]." The idea of an

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[*] I muſt in fairneſs take notice that the words here quoted from Philoſ. Journal IV. 321, are not Dr. Herſchel's but mine, as the abridger or narrator of the contents of his paper. The Doctor's obſervation (Philoſ. Tranſ. Y. 1800. page 261.) ſtates in very general terms that the *cooling cauſes* may, he ſuppoſes, have a ſtronger affect on the ſmall than the large thermometer. I do not here quote more fully, becauſe the point ſeems not of ſuch importance as to demand it, and becauſe the only miſtake reſpecting it appears to lie with the ingenious author of the preſent communication. His ſecond reflections will not, ſurely, permit him

aſcending cold ſtream is certainly original; at leaſt, it ſtands in direct oppoſition to all the eſtabliſhed principles of philoſophy, though it is not calculated to inſpire the moſt favourable opinion of the author's acumen in phyſical inquiries. The tardy progreſſion from violet to red compared with the numbers which I have aſſigned, ſeems to betray the acceſſion of extraneous influence. Beſides, the dark ball of a thermometer will, for the moſt part, receive its full impreſſion in the ſpace of one or two minutes; but the Doctor's thermometers required, or at leaſt were allowed, an expoſure of ten or thirteen minutes. This fact affords a ſtrong preſumption that other matters were brought into action, capable of a more prolonged abſorption, and of a greater accumulation of heat, than a mercurial bulb. Nor can I heſitate to impute the principal derangement to the platform on which the thermometers were laid, which, receiving light, though partially, would, as before obſerved, acquire heat in a quadruple ratio, and communicate this to the contiguous ſtratum of air. The ſame conſideration will explain the origin of the miſtake on which the paradoxical aſſertion is grounded, of "inviſible refrangible light." Indeed, a more objectionable plan of conducting experiments could ſcarcely have been deviſed. The priſm was fixed in an inverted poſition, to direct the rays downwards upon a table, which was covered with white paper; and the thermometer was placed a little beyond and above the edge of the red light. In ſuch a ſituation no wonder that the bulb was notably affected, being immerſed in a warm atmoſphere which extends to a certain diſtance over the illuminated ſpace, and receiving likewiſe a large portion of the ſeveral coloured rays reflected from the paper. That joint effect would be ſtill further increaſed by the thermometer having its bulb directly oppoſed to the aſcent of the heated air. The action might even be greater than if the bulb were placed in the courſe of the red rays, ſince it would then intercept that light which would have produced an accumulated effect on the wood below. But why is the *maximum* of inviſible light ſtated at half an inch beyond the red rays? Does it move in ſtraight lines? Does it diverge at a certain angle, or has it various degrees of refrangibility? Theſe were queſtions to be reſolved. And after all, what has *inviſible* matter to do with that peculiar ſtructure of the ſurface of bodies which conſtitutes black or white?

Should any doubt remain after this long examination, there is a ſingle fact which at once demoliſhes the whole fabric. If thoſe inviſible calorific rays had any real exiſtence, the action of a burning glaſs would be moſt powerful, not at the proper focus, but a conſiderable way beyond it. For the ſame reaſon, the hole burnt in a black piece of cloth would not be confined to the lucid focus, but would include a ring ſwelling all round to more than double that diameter.

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him to deny that every current of air which aſcends, in conſequence of the proximity of an heated body, muſt be cold when compared with that body itſelf; or (to keep more ſtrictly to the expreſſion alluded to) that it cannot receive the heat which makes it aſcend, without cooling, *or exerting a cooling agency on*, the body which heats it. N.

I have already extended this paper beyond the limits I firſt propoſed: I will not, therefore, fatigue your readers by attempting the refutation of ſome obſcure and verboſe arguments advanced by Dr. Herſchel, which involve a ſingular ſpecies of logic, and ſeem repugnant to the fundamental principles of dynamics. The hypotheſis of inviſible light is not new in the ſcientific world. It was ſuggeſted by the radiant heat of Scheele and the radiant cold of Pictet. It was propoſed or adopted by the late very ingenious Dr. Hutton. The facts, indeed, on which it was grounded may be ſatisfactorily explained from known principles. It was not conſiſtent with ſtrict metaphyſics; and to beſtow the property of being reflected on inviſible light, was ſurely ſtretching the limits of probability. Yet was the hypotheſis, in ſome degree, plauſible and alluring. The little improvement of aſcribing to it likewiſe refrangibility, by rendering the whole abſurd, has diſſolved the charm. What is the eye itſelf but a compound priſm? And is not the expanſion of the optic nerve adapted by its conſtitution to receive impreſſions through the diaphanous ſubſtance of the humours and coats, and to convey their appropriate ſenſations? But thoſe are only ſenſations of light. Senſations of heat are confined to no particular claſs of nerves. Refrangibility is therefore correlative with viſion, and "inviſible refrangible light" ſeems a contradiction of terms. But all metaphyſical conſiderations apart; if the image of the ſun be not encircled with a broad lucid ring, on fixing our eye on that luminary, the ſenſation of heat ought not to reſide in the correſponding ſpot on the retina, it ſhould be more intenſely felt over the ſurrounding ſpace.

Let me conclude by recommending to your inquiſitive readers two works of very ſuperior merit. I mean the *Traité d'Optique* of Bougner, and the *Photometria* of Lambert. The public has a right to expect that authors have previouſly ſtudied the labours of their predeceſſors; but it would be charity to believe that ſome late writers had not conſulted thoſe excellent models, which might have prevented much unneceſſary repetition, and corrected ſeveral groſs errors.

I am, SIR,
Your moſt obedient ſervant,
JOHN LESLIE.

IV.

Account of a Memoir of M. PROUST, on ſeveral intereſting Points in Chemiſtry[*].

THE firſt article relates to the *converſion of camphor into oil* by repeated diſtillations from a bolar earth. This operation, he obſerves, was long ſince deſcribed by Neumann, and he appears to reproach Citizen Lagrange that he has not quoted this author in his Memoir upon Camphoric Acid*, in which he has treated of the converſion of camphor into oil. Excepting this, nothing is ſaid upon the ſubject by M. Prouſt but what is already univerſally known.

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[*] Tranſlated from the Report made to the firſt Claſs of the Phyſical and Mathematical Sciences of the Inſtitute of France by Cit. Vauquelin; Guyton and Vauquelin being commiſſioners. The report is inſerted in the Annales de Chimie, XXXV. 32.

In the ſecond article, which is more intereſting than the firſt, he communicates ſome methods, more ſimple than thoſe which he had previouſly pointed out, of *obtaining pure tannin*, the principle of which he has obtained from the excellent Memoir of Citizen Deyeux upon the Nut-gall.

The proceſs conſiſts in pouring a ſolution of carbonate of pot-aſh into an infuſion of galls. By the mixture of theſe two liquids, a yellowiſh white precipitate is formed, in the form of a cogulum, which Ribaucourt took for an earth, and which it is only neceſſary to waſh with a little cold water to obtain the pure tanning matter. But care muſt be taken not to waſh it with too much, nor with warm water; for however little ſoluble this matter may be, it is neverthcleſs enough ſo, to diſappear totally in a tolerably large quantity of water. Hence it follows, that in order to enſure complete ſucceſs in the operation, the infuſion of galls muſt not be too much diluted with water, ſince in that caſe there will be no precipitation.

It is no leſs eſſential that the alkali ſhould be perfectly ſaturated with the carbonic acid, becauſe an exceſs would favour the ſolution of a certain quantity of the tannin, which would be greater in proportion as the alkali was more cauſtic. Thus it is ſeen that the tannin is ſeparated from its ſolution by an alkaline carbonate, not as might be ſuppoſed by ſaturating the gallic acid but by engaging its water of ſolution; for all the ſalts which have a certain degree of affinity with this liquid in the cold produce the ſame effect.

But however ſmall the quantity of water that may be uſed, and whatever its temperature may be, ſome tannin always remains in ſolution; becauſe it is ſoluble in a certain proportion even of the coldeſt water. The greateſt part of this matter, which thus remains in ſolution, may be obtained by evaporating any portion of the liquid, and part of the tannin ſeparates by cooling, which is more or leſs in proportion to the quantity of water evaporated. The tannin, when it has been thus ſeparated from the other ſubſtances which accompanied it in the galls, has the appearance of a glutinous paſte, of a yellowiſh grey colour, the parts of which adhere to each other in a conſiderable degree. It dries with difficulty; but when it is ſpread in thin layers upon earthen plates, and afterwards expoſed to the heat of a ſtove, it firſt melts, then becomes dry, and at laſt acquires the appearance of a kind of yellow reſin, with a vitreous fracture. This tannin, ſubmitted to diſtillation, affords a ſaline liquid, in which the ſmell of ammoniac is diſtinguiſhed, and which poſſeſſes the property of blackening ſolutions of the red oxide of iron; a property which muſt be attributed to a ſmall quantity of tannin volatilized without alteration, and not to the gallic acid, becauſe it does not afford a green colour with alkalis.

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* Philoſ. Journal, II. 97.

The oil which tannin affords by diſtillation is very ſmall in quantity, and ſo thick, that it remains attached to the neck of the retort. Its coal is bulky, and forms $\frac{1}{38}$ or 0.026 part of the diſtilled maſs.

The liquid, from which the tannin has been ſeparated by the carbonate of pot-aſh, ſoon becomes green in the air, the oxygen of which it abſorbs.

Sulphurated hydrogen deſtroys this green colour by uniting itſelf, in its turn, to the oxygen. Though the gallic acid renders tannin more ſoluble in water, as was remarked by Citizen Deyeux, nevertheleſs M. Prouſt does not believe that it is by combination with this acid that alkaline carbonates precipitate the tannin, ſince they alſo ſeparate this matter from a ſimple ſolution in water, and becauſe moſt of the very ſoluble neutral ſalts alſo precipitate it. It is, therefore, ſimply from a ſtronger affinity of theſe ſalts to water that theſe different ſubſtances ſeparate the tannin from that fluid. The gallic acid poſſeſſing, however, the faculty of diſſolving a certain quantity of this matter, and the carbonate of pot-aſh poſſeſſing, at the ſame time, the double power of ſaturating this acid and of uniting itſelf cloſely to the water, it ought, under like circumſtances of ſolubility, to leave leſs tannin in ſolution than the other ſalts.

M. Prouſt obſerves, that theſe facts, unimportant as they may ſeem to be, ſhould put us upon our guard againſt the effects which take place between the ſolution of the tanning principle and animal fluids; indeed, ſince the ſaline materials, of which theſe liquids are never deprived, are able to precipitate the tanning principle, it ought not henceforth to be concluded from the appearance of the precipitate, at leaſt not till the nature of the depoſition has been examined, that animal fluids contain gelatin.

A piece of the muſcle of an ox, or of raw hide, ſpeedily diſcolours the infuſion of gall-nut, and leaves nothing but gallic acid in the liquid. When theſe ſubſtances are ſaturated with the tanning principle, they eaſily dry without putrefaction; their fibres then ſeparate and crumble into duſt between the fingers, like wood which is worm-eaten. M. Prouſt very rationally conſiders the galling of wool, ſilk, and cotton, as a ſpecies of tanning; in which, by taking care not to carry the combination too far, the animal ſubſtance is made to acquire conſiderable hardneſs and reſiſtance to the action of water, without loſing too much of its flexibility, beſides rendering it unfit to become the food of inſects. He alſo demands whether the ſolution of tan would not anſwer the purpoſes of embalming better than any of the ingredients which have yet been made uſe of.

Citizen Dizé is the firſt perſon who obſerved that by pouring ſulphuric or muriatic acid upon a rather ſtrong decoction of galls, a conſiderable depoſit is formed. M. Prouſt availed himſelf of this property to ſeparate the tanning principle from the gallic acid with which it is mixed in the galls. It is, in fact, this ſubſtance which, by combining itſelf with the acids, becomes inſoluble, and is precipitated from the water in the form of an adheſive pitch. When the precipitation is complete, the liquid which contains the gallic acid is decanted; the precipitate is waſhed with cold water; it is then diſſolved in boiling water; the ſulphuric acid is ſaturated with carbonate of pot-aſh, and the tanning principle becomes precipitated as the pot-aſh unites itſelf to the acid, and the ſulphate of pot-aſh, which has been formed, diſſolves in the water. In order to obtain the whole of this ſubſtance, the liquid muſt be reduced by evaporation, and the depoſit muſt be ſuffered to become cold, and waſhed with cold water. It is the pure tanning principle.

After deſcribing the proceſſes, which we have juſt related, for ſeparating the principles of the nut-gall, namely, the tanning principle and the gallic acid, M. Prouſt next examines what takes place between theſe ſubſtances and iron in the making of ink. He remarks, firſt, that the reaſon why ink is not formed with a ſolution of iron at the minimum of oxidation is, becauſe the tanning principle and the gallic acid have leſs affinity with the iron than the ſulphuric acid has. 2dly, That when a ſolution of iron at the maximum of oxidation immediately produces a black colour with an infuſion of galls, the reaſon is, that in this caſe the red oxide of iron has a greater attraction to the principles of the gall-nut than to the ſulphuric acid. He proves this by diſſolving the combination of iron with the tanning principle in ſulphuric or muriatic acid, which gives a blueiſh tranſparent liquid, which would loſe its colour if decompoſition took place. He, therefore, conſiders ink as a ſolution of the tannate and the gallate of iron in ſulphuric acid. 3dly, That when ink, ſpread upon paper, quickly becomes black, it is becauſe it abſorbs an additional quantity of oxygen, which renders it more inſoluble in water. The combination of the oxide of iron with the gallic acid and with the tanning principle, which forms the baſis of ink, contracts only a ſlight adherence with the acids, and ſeparates itſelf from them by repoſe at the end of ſome time. In this caſe it is affected nearly in the ſame manner as metallic ſalts diſſolved in a foreign acid; and for this reaſon it is that when a plate of iron is put into a ſolution of ink, this metal precipitates the black particles, abſolutely in the ſame manner as iron ſeparates the phoſphate of iron from acids.

Alkalis, cautiouſly added to the ſolution of ink, precipitates its black parts, but an exceſs of alkali diſſolves the atramentary combination, and produces a fluid of the colour of wine, more or leſs intenſe.

Gallic acid produces no effect on ink, becauſe the oxide of iron is ſaturated. The ſame acid is not very active in ſhewing the preſence of red oxide in a ſolution containing an exceſs of acid, becauſe the combination is diſſolved in proportion as it is formed. But by carefully ſaturating the exceſs of acid, the liquid is rendered black.

The combination of the gallic acid, and the tanning principle with iron, does not become changed into pruſſiate of iron by means of pruſſiate of pot-aſh, even with the aſſiſtance of heat. The nitric acid diſſolves the ink, and effects no change in its nature in the cold; but by heat it deſtroys it, and the yellow oxalate of iron is precipitated by the addition of ammoniac. Whenever iron filings and an infuſion of galls are mixed together, a diſengagement of hydrogen gas takes place; which proves, ſays M. Prouſt, that the oxidation of iron begins at the expence of the water, and that it is completed by the air, an event which alſo takes place with vegetable acids, which, not ſufficiently attracting the oxide of iron oxided to the minimum, cannot counterpoiſe the affinity of the oxigen which tends to advance it to the maximum; and this is the reaſon why the acetates of iron paſs ſo rapidly to a red colour, that it is impoſſible to obtain them green in a ſolid ſtate.

M. Prouſt prefers, both for durability and beauty, ink which is made by a ſolution of iron in an infuſion of the gall-nut, to that which is prepared from ſulphate of ſteel. His reaſons are plauſible. The infuſion of acorns, concentrated and digeſted with iron filings, preſented the ſame phenomena as the infuſion of the gall-nut, and afforded M. Prouſt an ink, which, when mixed with a ſmall quantity of vinegar and a ſufficient quantity of gum, was inferior to none in its good qualities.

From the experiments of M. Prouſt, it follows, that there is nothing except the red oxide of iron which can make ink with the principles of the gall-nut; that the ſulphates of iron, which are made uſe of for that purpoſe by ink-makers, containing always different quantities of red oxide, there as many different kinds of ink as there are recipes for making of it; but let theſe inks be only once ſpread upon paper, and the air reduces them all to nearly the ſame ſtate.

Every one is acquainted with the property poſſeſſed by inks which are too much diluted, of becoming black after they are ſpread upon paper by the pen; but this phenomenon may be produced in a much more ſtriking manner. It is only neceſſary to paſs ſome ſulphurated hydrogen gas into the ink, and the black colour will inſtantly diſappear; but afterwards, in writing, it is very pleaſant to obſerve this clear liquid become very ſpeedily black.

Such are the principal facts which have been communicated by M. Prouſt reſpecting the combination of iron with the principles of the gall-nut; namely, the tanning principle and the gallic acid. They ſerve to illuſtrate the theory and the practice of the arts of dyeing and of ink making. He has promiſed the Inſtitute to purſue his reſearches on this ſubject ſtill further.

In another article, M. Prouſt propoſes *a method of obtaining ſulphuric acid from the reſidues of the diſtillation of ſulphuric ether.* This ſimple proceſs conſiſts in diluting the reſidue with two parts of water, filtering through a cloth to ſeparate the coaly ſubſtance from it, and ſubjecting the liquid to diſtillation in a glaſs retort. When the acid has acquired 1.84 of ſpecific gravity, from 4 to 6 grammes (parts) of ſaltpetre to each demikilogram (five hundred parts) are added; and the diſtillation is continued till the fluid is perfectly white, and weighs from 1.86 to 1.87, water being 1.00.

M. Prouſt prefers an iron frame to a ſand bath to place the retort in for theſe diſtillations; and he gives very good reaſons for the preference. He computes that the profit of purifying the reſidues of ether in this manner will be conſiderable; ſince it affords between a fourth and a third part of ſulphuric acid concentrated to 1.87 ſpecific gravity.

The ſulphuric acid, ſeparated from the butuminous matter before it is rectified, has been ſubjected to ſome experiments by M. Prouſt. He obſerved, that alkalis, as well as the pruſſiates, did not precipitate any thing. It abundantly decompoſes hydro-ſulphurated water by virtue of the ſulphureous acid it contains: oxigenated muriatic acid diſcolours this brown liquid; which, in his opinion, proves that this colouring material reſembles the juices of vegetables. The coaly ſubſtance which is ſeparated during the formation of ſulphuric ether has alſo been made an object of M. Prouſt's reſearches: when dried it reſembles lamp black (*noir de fumée*) as well in its black colour, its brittleneſs, and its ſhining colour. If expoſed to heat, it emits a ſmell of ſulphureous acid mixed with that burning lac. Boiling water diſſolves 0.24 parts of this ſubſtance, alcohol as many as 0.44; and this ſolution is not rendered turbid by water. Theſe two ſolutions are proper for the formation of brown dyes by means of the muriate of tin.

The ſolution of the coaly ſubſtance of the ether in alcohol affords, by evaporation, a black friable and nearly inſipid powder, 5.31 grammes (100 Fr. grains) of this ſubſtance diſtilled produced an aromatic water, light and empyreumatic oils, vegetable acid mixed with a little ſulphuric acid, and carbonated oily hydrogen gas, mixed with about a third of carbonic acid. The coal which remained in the diſtilling veſſel weighed 0.64 grammes (12 Fr. grains); it left, after combuſtion, ſome aſhes, in which analyſis diſcovered the preſence of lime and magneſia. M. Prouſt, indeed, ſuſpects that there is a ſmall quantity of ſiliceous earth and alumine in it. Here, then, obſerves this ſkilful chemiſt, we have earthy aſhes, which accompany carbon even into alcohol.

M. Prouſt concludes that the principles of the alcohol in forming ether by the action of the ſulphuric acid become united in other orders, and produce a ſubſtance analogous to the juices of vegetables, ſince, during the ſummer, it is covered with mouldineſs like a vegetable decoction.

M. Prouſt alſo diſcovered that, during the diſtillation of phoſphorus, a combination of this ſubſtance with charcoal is conſtantly formed, and that it is this combination which remains in the chamois leather after phoſphorus has been paſſed through it for its purification, after the manner of Pelletier. It is of a red colour, and does not melt like pure phoſphorus. If it be diſtilled by a gentle heat, a portion of phoſphorus is ſeparated, which exceeded the point of ſaturation; but the true combination is not decompoſed, unleſs the degree of heat be conſiderably augmented. When the veſſels become cool, a powder remains of lively orange red, light, floculent, and very homogeneous through its whole maſs: M. Prouſt conſiders this product as an intimate union of carbon and phoſphorus. The following are ſome of its properties which he deſcribes. If, whilſt it is ſtill in the retort, the temperature be ſufficiently raiſed to ignite the bottom, a freſh quantity of phoſphorus is ſublimed, and the reſidue is then mere coal. It burns rapidly when placed upon a plate of hot metal; but the coal, by imbibing phoſphoric acid, eludes the combuſtion. By expoſure to the air, it ſoon loſes this diſpoſition to take fire, and may then be preſerved without any danger. It is without ſmell or taſte. It is this combination, according to M. Prouſt, which forms the red reſidues which remain without alteration in the apparatus in which phoſphorus is prepared. Its properties ſerve to explain the origin of a black powder which ſome chemiſts have obſerved in phoſphorus. There cannot be a ſtronger proof, continues M. Prouſt, that the phoſphuret of carbon is a combination formed by virtue of the laws of affinity, than its unchangeableneſs by the ſolution of cauſtic pot-aſh, even with the aſſiſtance of heat. Time did not permit the author to determine the proportions of the elements of the phoſphuret of carbon. He promiſes a plentiful harveſt of new facts to thoſe who may be diſpoſed to purſue this ſubject, and follow its combinations with metals and other combuſtible bodies.

On this occaſion M. Prouſt corrects or recalls an opinion which was given in the Journal des Mines, reſpecting the ſmell of hydrogen gas which becomes diſengaged during the ſolution of certain kinds of caſt iron and ſteel in acids. He conceives that it is owing to the preſence of an eſſential oil rather than to that of phoſphorus, as had been ſuſpected. He gives the following obſervations in ſupport of his preſent notion: 1°. The neck of the matraſſes, retorts, and receivers, in which the inflammable gas is prepared, are greaſed with little drops of this oil. 2°. If 15 ounces of black crude iron be diſſolved in ſulphuric or muriatic acid, drops of oil are obſerved floating on the water of the receiver. 3°. If the waſhed carburets, which are obtained from crude iron, be gently diſtilled, water comes over along with an oil of the ſame ſmell; and alcohol, in which ſuch crude iron has been macerated (in powder) becomes white on the addition of water. M. Prouſt thinks that if the inflammable gas, which is obtained from crude iron, were carefully examined, it would be found to hold oil in ſolution, and that it is this ſubſtance which gives it its fetid ſmell. If the fact be accurately ſo, of which we have no doubt, it is, as M. Prouſt obſerves, a grand ſtep towards the explanation of the phenomena of vegetation, and the underſtanding the tranſition of mineral ſubſtances into vegetable products.

M. Prouſt thence paſſes to the examination of the native of the iron of Peru. This ſpecies of iron, which has been noticed by Rubin de Celis, is of a greyiſh white colour. It bears a rather ſtrong reſemblance to certain native ſilvers, for which it was for ſome time miſtaken. It poſſeſſes a conſiderable degree of ductility, does not ruſt like common iron, and has nearly the ſame habitude under the file.

5.31 grammes (100 grains) of this iron, diſſolved in ſulphuric acid, only afforded 3520 centimetres (176 Fr. inches) of hydrogen gas; whilſt the ſame quantity of common iron wire gave 200 inches. At the commencement of the ſolution, a ſmall quantity of carbonate of iron was ſeparated, which diſappeared towards the end.

The colour of the ſolution, which was much greener than uſual, led M. Prouſt to ſuſpect that there was copper in this iron; but ſulphurated hydrogen not having precipitated any thing, and the liquid having preſerved the ſame colour as before, it appeared to him that no other ſubſtance but nickel could produce theſe effects. In order to have a certain proof of this, he oxided the iron ſtrongly by means of nitric acid, and afterwards precipitated it by the gradual addition of pot-aſh. The iron being thus ſeparated, the green colour of the liquid aſſumed a greater degree of intenſity, and by completing the precipitation, after filtering, he obtained a ſubſtance which, with ſulphuric acid, yielded ſulphate of nickel. This method of analyſis, which M. Prouſt has indicated, in combination with the uſe of ſulphurated hydrogen, was found by him to be equally ſucceſsful in the complete ſeparation of nickel from iron, arſenic, and copper, which are almoſt always met with in the ores of this metal.

The native iron of Peru is therefore, according to the experiments made by M. Prouſt, an alloy of iron and nickel; a new diſcovery of the moſt intereſting nature. The preſence of nickel in this alloy, obſerves the author, appears to announce that it is the product of art; but when it is conſidered that there exiſts a maſs of more than 1363 myriagrams (300 quintals) in a plain of more than 100 leagues in circumference, where there is neither mountain nor water, nor ſcarcely a ſtone is to be found, the difficulty of the problem ſtill remains in all its force. Laſtly, adds M. Prouſt, if the power of uniting theſe metals in ſuitable proportions can be obtained by metallurgiſts, they will have obtained an alloy which will poſſeſs many advantages over other iron, and more particularly that of not being liable to ruſt.

M. Prouſt's memoir is concluded with ſome detached facts, as well on animal as on mineral ſubſtances. 1ſt. A myriagram (20 Fr. pounds) of beef, of which 25 hecotograms (5 pounds) were bone, produced only one pound of common brown extract, which was elaſtic, and of a taſte ſimilar to concentrated or very ſtrong ſoup. It would be impoſſible to convert this extract into portable paſtels without the addition of a rather large quantity of the jelly of bones. Whenever beef is boiled in ſilver, this metal becomes black, as is the caſe when it is uſed for the boiling of whey, freſh urine, &c. 76 hectograms (15 Fr. pounds) of meat were reduced, by boiling, to ten pounds; but the bones loſt none of their weight: whence it follows, that in order to extract as much nutriment as is poſſible from theſe ſubſtances they muſt be broken to pieces.

The freſh decoction of fleſh is acid, and reddens the tincture of turnſole. The acid which it contains appears to be the phoſphoric; for lime water and ammoniac form a rather abundant precipitate with it. Alcohol diſſolves a portion of the extract of fleſh, and this part being extremely ſalt, M. Prouſt ſuſpected the preſence of muriate of ammoniac; but lime in powder not having ſeparated any ammoniac, he mixed a ſolution of platina with it, which immediately furniſhed him with muriate of platina and pot-aſh. The decoction, therefore, contains muriate of pot-aſh in abundance. M. Prouſt had not time to examine the other ſalts contained in the decoction, nor the ſubſtance ſoluble in alcohol; and he, with reaſon, complains that a ſubſtance ſo neceſſary for the nouriſhment of mankind, has not been made the object of ſerious chemical reſearch.

He made ſome trials upon the pyrites of the Incas, to diſcover if it did not contain gold or ſome other ſubſtance which might ſerve to explain the cauſe of the pale colour which diſtinguiſhes it. He only obtained, as the reſidue of his ſolution, a black powder mixed with ſand. This powder was coal, which cauſed a ſtrong detonation with nitre.

It has been thought, and it is ſtill generally ſuppoſed, that the black duſt which is depoſited during the ſolution of zinc in acids is carburet of iron; but M. Prouſt found that it was a mixture of arſenic, copper, and lead which the diſoxidating action of the zinc precipitates in the metallic ſtate. This alſo happens to theſe metals when a ſolution is made of tin with which they are alloyed. Arſenical lead, ſuch as is made uſe of at the mines of Limares for the making ſmall ſhot, depoſit on the immerſion of a ſheet of lead, part of the metallic arſenic they contained. Theſe are not the only metals which adulterate the purity of the zinc. Iron and manganeſe are ſometimes found in great quantities: whence it is not ſurprizing that the art of watch-making has ſo much reaſon to complain of the bad qualities of braſs made in the common way. The following facts will ſhew the enormous difference between purified zinc, and that of commerce. To purify zinc, Mr. Prouſt propoſes diſtilling it in a ſtone retort, the neck of which muſt be inclined at leaſt 45 degrees, that the metal may flow with greater eaſe as ſoon as it becomes volatalized. A mixture of ſand, and of the oxides of iron, lead, copper, and zinc remain in the retort, the oxidation of which is attributed by M. Prouſt to the poroſity of the veſſels.

The zinc thus purified does not differ either in colour or in weight from the zinc of commerce: 55 decigrams (100 Fr. grains) of the latter yield by ſolution in ſulphuric acid, in leſs than hour, 3440 centimetres (from 172 to 174 Fr. inches) of hydrogen gas; whereas more than 8 days are requiſite to obtain the ſame reſult from diſtilled zinc: This is the difference which M. Prouſt obſerved between the two metals. The black matter which ſeparates from the zinc, during its ſolution in ſulphuric acid, is ſoluble in nitric acid; and its ſolution on the addition of hydro-ſulphurated water affords arſenic; and if that arſenic had been mixed with copper or lead, the ſame re-agent would have equally diſcovered it. For by adding it gradually to the ſolution of thoſe three metals, the copper is ſeen to precipitate itſelf firſt of a brown colour the lead, the next of a black colour, and the arſenic laſt of a yellow colour; and by a careful management they may be ſeparated by filtration.

M. Prouſt juſtly conſiders the purification of zinc by ſulphur as an abſolute deception.

55 decigrams (100 Fr. grains) of zinc, diſſolved in nitric acid, leave, after the decompoſition of the nitre by fire, 69 decigrams (from 125 to 126 Fr. grains) of oxide of a light yellow colour. A ſolution of the ſame quantity of zinc, decompoſed by carbonate of pot-aſh, furniſhed 99 decigrams (180 Fr. grains) of carbonate of zinc, which alſo left 125 grains of oxide after calcination.

The ſolution of this metal by ſulphuric acid gave the ſame reſults; which proves that zinc, in all caſes, combines itſelf with a ſpecific quantity of oxygen. M. Prouſt obſerves how ſingular it is that copper and zinc, which attract the oxygen with ſuch different degrees of ſtrength, ſhould, nevertheleſs, in their combination with acids, abſorb exactly the ſame proportion of this principle.

M. Prouſt, in the next place, propoſes the following formula to ſeparate zinc from copper. Suppoſe the ſame ſolvent to contain lead, copper, and zinc. Sulphate of pot-aſh will precipitate the lead, if the (nitric) ſolution does not contain too much acid. Hepatic water ſeparates the copper a long time before the zinc. The filtered liquid may be examined by ſulphurated hydrogen. When it no longer becomes coloured, a greater quantity of hydroſulphurated water muſt be added to it, when the zinc will in its turn fall down of a light yellow colour. If there had been iron, cobalt, nickel, or manganeſe in the ſolution none of them would have been precipitated by this re-agent.

From the preceding experiments on zinc, M. Prouſt draws the following concluſions. 1ſt. In whatever acid this metal be diſſolved, it conſtantly abſorbs the ſame quantity of oxygen. 2d. If it contain metals capable of ſuperoxydation, they paſs to this ſtate when the ſolution has been effected either by nitric or oxygenated muriatic acid. 3. In a muriatic or ſulphuric ſolution this metal is as much oxided as it can be, but iron on the contrary is only at its minimum; and this is the reaſon why this ſolution does not become coloured with gallic acid, unleſs it has been expoſed to the air. 4th. To demonſtrate the preſence of iron ſome drops of oxygenated muriatic acid muſt be added, or it may be boiled with a ſmall quantity of nitrous acid. 5. The carbonate, which is very white whilſt under water, becomes yellow as ſoon as it is brought into the air, becauſe the iron paſſes quickly to the maximum of oxidation. 6th. Sulphate of zinc, which has had time to abſorb in the atmoſphere as much oxygen as is neceſſary to elevate the iron to its maximum, immediately affords yellow carbonate of zinc. 7. Zinc, which is purified by diſtillation, or by the following humid proceſs, is alone proper to afford the true zinc-white for painting.

Repeated criſtallization of the ſulphate of zinc, and immerſion of plates of this metal into its ſolution appeared to M. Prouſt, to be very inſufficient means to ſeparate the foreign metals. To accompliſh this object, 30.57 grams (one Fr. ounce) of nitric acid are put into about 2 pounds of the ſaturated ſolution of ſulphate of zinc, and the mixture ſlightly boiled. Pot-aſh is then mixed with it to ſaturate the exceſs of acid, and to precipitate nearly 15.28 grams (3 or 4 gros) of the matter; this mixture is boiled again, and the precipitate is ſoon obſerved to paſs from white to yellow. If after ſome minutes of ebulition ſome white particles are remarked amongſt the yellow depoſit, it is a certain ſign that not an atom of iron remains in the ſolution of zinc. But if by this method the iron has been entirely ſeparated from the zinc, manganeſe may yet be preſent, if any portion exiſted in the ore; and M. Prouſt has frequently met with it.

To clear it of this new oxide the ſulphate of zinc is diſſolved in boiling water, and precipitated by carbonate of pot-aſh, ſo as ſtill to leave a ſmall quantity of oxide of zinc in ſolution. The depoſit is kept ſeveral days in the liquor, in order that the oxide of manganeſe, which has been precipitated, being more ſtrongly attracted by the acid than the oxide of zinc, may precipitate that portion of the latter which had been left in the fluid, and become diſſolved in its place. Then the ſulphate of zinc affords an oxide of the moſt perfect whiteneſs which may be employed, very advantageouſly, for painting.

Theſe are the facts contained in M. Prouſt's Memoir. They are numerous and intereſting. Some of them are new to us; the moſt part, though already known, are preſented under new relations and rendered applicable to the arts. The experiments, by the help of which he has diſcovered theſe facts, are ingenious. His explanations, though conciſe, appear to us to be clear, and deduced immediately from the reſults of the experiments. They may therefore tend to the advancement of philoſophical chemiſtry, and to the perfection of the arts and manufactures. We, conſequently, recommend to the claſs (of the national inſtitute) to order them to be printed in the volumes which it propoſes to publiſh. We, cannot, however, agree with M. Prouſt in ſome points. 1ſt. That nickel, cobalt, and manganeſe are not precipitated by ſulphurated hydrogen. 2. That the black duſt, which ſeparates during the ſolution of zinc, does not contain carburet of iron. 3d. That zinc is ſeparated from arſenic by diſtillation. The contrary is ſo well known by chemiſts, that it requires no proof.

V.

Experiments on the ſolar and on the terreſtrial Rays that occaſion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occaſion them, are ſubject, in order to determine whether they are the ſame or different. By WILLIAM HERSCHEL, *LL.D. F.R.S*[*].

THE word heat is moſt commonly uſed to denote a certain well known ſenſation. It is alſo employed to ſignify the cauſe of that ſenſation, as well by the vulgar as by men of ſcience: ſome of whom have added certain diſtinctive terms, ſuch as latent, abſolute, ſenſible, radiant, while others have treated of the matter of heat, and caloric. Dr. Herſchel has not thought fit to adopt any of theſe terms in his Memoir, but has choſen to treat of *the rays that occaſion heat*; not meaning either to ſtate that theſe rays are heat, or the manner in which they produce it. In his prefatory obſervations he alſo in effect remarks that his preſent reſearch is confined to the agency of heat in its ſtate of radiance; without entering into any conſiderations reſpecting the general nature of heat itſelf, whether it be matter or modification, or of radiance, whether it be the projection of particles, the undulation of a fluid, or any other habitude or thing.

His ſubject is reduced to three general heads: the firſt relates to the heat of luminous bodies in general as it comes directly from the ſun, or from the terreſtrial flames of torches, candles, lamps, blue lights, &c.—the ſecond includes the heat of coloured radiants as from the ſun, when its rays are ſeparated by the priſm, or from culinary fires openly expoſed;—and the third diviſion relates to heat obtained from radiants, where neither light nor colour in the rays can be perceived. This is to be had as before ſhewn[†], directly from the ſun, by means of a priſm applied to its rays, and alſo from fires incloſed in ſtoves, or from red-hot iron, cooled, till it can no longer be ſeen in the dark.

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[*] Abridged from the Phil. Tranſ. for 1800, page 293.

[†] Philoſ. Journal IV. 320.

Beſides the foregoing general arrangement, the author gives a previous comparative view of facts relating to light and heat as follows:

1. LIGHT, both ſolar and terreſtrial, is a ſenſation occaſioned by rays emanating from luminous bodies which have a power of illuminating objects; and according to circumſtances of making them appear of various colours. 2. Theſe rays are ſubject to the laws of reflection. 3. And alſo thoſe of refraction. 4. They are of different refrangibility. 5. Are liable to be ſtopped in certain proportions when tranſmitted through diaphanous bodies. 6. And to be ſcattered on rough ſurfaces. 7. And laſtly, they have hitherto been ſuppoſed to have a power of heating bodies, but this remains to be examined.

1. HEAT, on the other hand as well terreſtrial as ſolar, is a ſenſation occaſioned by rays emanating from candent ſubſtances which have a power of heating bodies. Theſe rays have the ſame affections as have been enumerated in the 2, 3, 4, 5, and 6th diviſions of the laſt paragraph, and 7. They may be ſuppoſed, when in a certain ſtate of energy, to have a power of illuminating objects; but this remains to be examined.

The paper before us contains the firſt part of the experimental inveſtigation of the ſubject under the three firſt heads. The three next, and a diſcuſſion which will be brought on by the ſeventh article, are reſerved for the ſecond part.

Experiment 1. *Reflection of the heat of the ſun.* The ſun's rays were received in a ten foot Newtonian teleſcope, with a camera eye piece, with no eye glaſs. By proper adjuſtment the focus was made to fall on a ſmall thermometer, which was raiſed 58 degrees. The rays, therefore, whether of light or not, did occaſion heat after three regular reflections.

Experiment 2. *Reflection of the heat of a Candle.* At the diſtance of 29 inches from a candle a ſmall ſteel mirror, 3, 4 inches diameter, and about 2, 75 inches, focal length, was placed. Two thermometers were diſpoſed, one in the ſecondary focus, and the other very near it, but out of the courſe of the reflected light. In five minutes that in the focus roſe 3¼ degrees, which it loſt again in ſix minutes when the candle was covered, and again recovered it in five minutes when the rays were ſuffered to fall on the mirror. The other thermometer remained ſtationary throughout.

Experiment 3. *Reflection of the heat that accompanies the ſolar priſmatic colours.* The ſolar ſpectrum given by a priſm was admitted through paſteboard (which limited the viſible colours) upon the ſteel mirror. The thermometer in the focus was raiſed 35° in two minutes.

Experiment 4. *Reflection of the heat of a red hot poker.* The mirror was placed at the diſtance of 12 inches from a red hot poker, and the thermometer placed in its ſecondary focus. A ſmall paſteboard ſcreen was placed to guard the thermometer from the direct heat. In one minute and a half it roſe 38½°, and fell 28° in the next minute and a half, when the mirror was covered.

Experiment 5. *Reflection of the heat of a coal fire by a plain mirror.* A ſmall ſpeculum (Pl. XVI. fig. 1.), ſuch as the Doctor uſes with his ſeven feet reflectors was placed upon a ſtand, ſo as to make an angle of 45° with the front of it, and conſequently with the bars of a grate oppoſite which it was placed. At a diſtance of 3½ inches from the ſpeculum, on the reflecting ſide of it, was placed a thermometer, and another cloſe by it out of the courſe of the reflected rays. The whole was guarded in front againſt the influence of the fire by an oaken board, 1½ inch thick, in which was a circular opening of 1¼ inch diameter, oppoſite the plain mirror, to permit the fire to ſhine upon it. There was alſo a wooden partition between the mirror and the thermometers in which was a hole that allowed the rays to fall on the firſt mentioned thermometer, but prevented their acceſs to the latter. When this apparatus was expoſed to the fire the firſt thermometer roſe 7 degrees in five minutes, while the other indicated a change in the temperature of the place, amounting to no more than half a degree.

Experiment 6. *Reflection of fire-heat by a priſm.* When the laſt deſcribed experiment was repeated by ſubſtituting inſtead of the mirror a priſm whoſe angles were 90°; 45°; and 45°, ſo placed that the radiation from the fire ſhould paſs through one ſide, and make its incidence on the hypothenuſe within the priſm, the riſe was 4½ degrees in 11 minutes; but the temperature of the place, as ſhewn by the other thermometer, having been raiſed 1¾ degrees in the ſame time, the effect of the reflection was only 2¾ degrees.

Experiment 7. *Reflection of inviſible ſolar heat.* On a board of about 4 feet, 6 inches long, a ſmall plain mirror was fixed at one end, and at the other, two thermometers, as ſhewn in fig. 2. Pl. XVI. where for the convenience of inſertion the board is repreſented as broken. The diſtance of one thermometer from the face of the mirror was 3 feet 9¾ inches; and the other thermometer was put at the ſide of it facing the ſame way, but out of the courſe of the rays, that were to be reflected from the mirror. The colours of the priſm were thrown on a ſheet of paper, having parallel lines drawn upon it at half an inch from each other. The mirror was ſtationed upon the paper; and was adjuſted in ſuch a manner as to preſent its poliſhed ſurface in angle of 45°, to the incident coloured rays, by which means they would be reflected towards the ball of the firſt mentioned thermometer. In this arrangement the whole apparatus might be withdrawn from the colours to any required diſtance, by attending to the laſt viſible red colour, as it ſhewed itſelf on the lines of the paper. When the thermometers were properly ſettled to the temperature of their ſituation, during which time the mirror had been covered, the apparatus was drawn gently away from the colours ſo far as to cauſe the mirror, which was now open, to receive only the inviſible rays of heat which lie beyond the confines of the red. The reſult was, that in ten minutes the firſt thermometer received four degrees of heat reflected to it in the ſtricteſt optical manner, and the great regularity with which theſe inviſible rays obeyed the law of reflection was ſuch that the very ſenſible thermometer, which was choſen as a ſtandard, and was within an inch of the other, remained all the time without the leaſt indication of any change. The mirror was then taken away, and in ten minutes the meaſuring thermometer again loſt the heat it had required. *This therefore*, as the Doctor obſerves, *is a moſt deciſive experiment in proof of the exiſtence of inviſible rays, of their being ſubject to the laws of reflection, and of their power of occaſioning heat.*

Experiment 8. *Reflection and condenſation of the inviſible ſolar rays.* The ſmall ſteel mirror before deſcribed was by a ſuitable apparatus placed to receive the rays of the priſmatic ſolar ſpectrum perpendicularly to its diameter or ſubtenſe. Half the mirror was covered by a ſemicircular paſteboard with lines drawn thereon parallel to the diameter, by means of which the laſt viſible red might be made to fall at any required diſtance from the uncovered portion of the ſpeculum. The thermometer was placed in the focus, and the whole ſurface of the mirror kept covered till the inſtrument became ſtationary at the temperature of the place. At this period the apparatus was moved till the laſt viſible red fell on the graduated paſteboard at the diſtance of one-tenth of an inch from its diametrical edge, when the other part of the reflecting ſurface being uncovered for the admiſſion of the inviſible rays, the thermometer in one minute was raiſed 19 degrees. The mirror was then again covered, and in 3 minutes the thermometer fell 16 degrees.—Again, the mirror was opened, and in 2 minutes the thermometer roſe 24 degrees. And on covering the mirror once more, in one minute the thermometer fell 19 degrees.

By this alternate riſing and falling our author obſerves, that there are three points clearly aſcertained. Firſt, that there are inviſible rays of the ſun. Secondly, that theſe rays are not only reflexible in the manner proved in the foregoing experiment, but that by the ſtrict laws of reflection they are capable of being condenſed. And thirdly, that by this condenſation their heat is proportionally increaſed.

Experiment 9. *Reflection of inviſible culinary heat.* The ſteel mirror was fixed upon a ſmall board, and before it was a wooden ſcreen, half an inch thick, which was of an height to intercept the rays which might elſe have fallen on the lower half of the mirror. Behind this ſcreen were placed two thermometers; one of which was in the axis of the mirror, and the other, which was near it, was ſcreened on the ſide towards the mirror, by a ſmall ſlip of paſteboard tied to the ſcale. This apparatus being duly placed oppoſite a cloſe ſtove, well heated, the inviſible rays reflected from the mirror raiſed the thermometer in its axis 39 degrees in one minute, while the other, which had exactly the ſame expoſure to the ſtove, but was defended from the reflected rays of the mirror, roſe only one degree.

Experiment 10. *Reflection of the inviſible rays of heat of a poker cooled from being red-hot till it could no longer be ſeen in a dark place.* A poker of a proper black heat was placed at the diſtance of 12 inches from the ſteel mirror, and the effect of its condenſed rays was received upon the thermometer placed in the focus. The mirror was uncovered and covered alternately, one minute at a time, for ſix minutes, and the alternations were, riſe 7°; fall 7°; riſe 3°; fall 5°; riſe 2½°; fall 6½°.

Dr. Herſchell concludes his narration of facts concerning reflected heat with the following remarks:

“ From theſe experiments it is now ſufficiently evident, that in every ſuppoſed caſe of
“ ſolar and terreſtrial heat, we have traced out rays that are ſubject to the regular laws
“ of reflection, and are inveſted with a power of heating bodies; and this independent of
“ light. For though, in four caſes out of ſix, we had illuminating as well as heating rays,

" it is be noticed that our proof goes only to the power of occaſioning heat, which has been " ſtrictly aſcertained by the thermometer. If it ſhould be ſaid that the power of illuminating " objects, of theſe ſame rays, is as ſtrictly proved by the ſame experiments, I muſt remark " that from the caſes of inviſible rays brought forward in the four laſt experiments, it is " evident that the concluſion that rays muſt have illuminating power, becauſe they have a " power of occaſioning heat, is erroneous; and, as this muſt be admitted, we have a right " to aſk for ſome proof of the aſſertion, that rays which occaſion heat can ever become viſible. But, as we ſhall have an opportunity to ſay more of this hereafter, I proceed now " to inveſtigate the refraction of heat-making rays."

Experiment 11. *Refraction of ſolar heat.* The ſun's light was received in a new ten ſeet Newtonian teleſcope; the mirror of which has 24 inches in diameter of poliſhed ſurface. The rays were made to paſs through a day-piece with four lenſes, and thence to the ball of the thermometer placed in their focus. As ſoon as the rays were brought to the thermometer it roſe almoſt inſtantly through 70°, when the teleſcope was turned away to avoid the danger of cracking the glaſſes. Here the rays which occaſioned this ſudden augmentation of heat had undergone eight ſucceſſive refractions, ſo that their being ſubject to its laws cannot be doubted.

Experiment 12. *Refraction of the heat of a candle.* A lens of about 1.4 inch focus, and 1.1 diameter, mounted on a ſmall ſupport was placed at the diſtance of 2.8 inches from a candle, and a thermometer was very carefully placed in the ſecondary focus of the candle behind the lens. Not far from the lens towards the candle was a paſteboard ſcreen with an aperture of nearly the ſame ſize as the lens; and the lens itſelf was ſupported on an eccentric pivot, ſo that it could be turned away from its place and reſtored at pleaſure. This arrangement being made, the thermometer was for a few moments expoſed to the rays of the candle till it had aſſumed the temperature of its ſituation. Then the lens was turned on its pivot, ſo as to intercept the direct rays which paſſed through the opening in the paſteboard ſcreen, and to refract them to the focus in which the thermometer was placed. In three minutes the thermometer received 2½ degrees of heat by the refraction of the lens, which it again loſt in the ſame time when the lens was turned away, and in three minutes more the ſame increaſe of heat was produced by replacing the lens in its ſituation. A greater effect could be produced by a different arragement of the diſtances. Thus when the lens was placed at 3½ inches diſtance from a wax candle, the thermometer in the ſecondary focus was raiſed from 5 to 8 degrees, according to the burning of the candle and the accuracy of the adjuſtment.

Experiment 13. *Refraction of the heat that accompanies the coloured part of the priſmatic ſpectrum.* The coloured part of the priſmatic ſolar ſpectrum being admitted through a perforated ſcreen of paſteboard, upon a burning glaſs, raiſed the temperature of a thermometer 112 degrees in one minute.

Experiment 14. *Refraction of the heat of a chimney fire.* The ſame burning glaſs which is nearly 9 inches in diameter was placed before the clear fire of a large grate, at the diſtance of 3 feet from the bars, and a thermometer was placed in the ſecondary focus. Another thermometer was alſo placed behind a wooden ſcreen at the ſame diſtance from the fire. The ſituation of the ſeveral parts of the apparatus being thus adjuſted the thermometers were taken away to be cooled, and then reſtored to their places. The ſtandard thermometer in the courſe of the firſt ſeven minutes acquired a heat of 3¾ degrees, after which it remained ſteady for the ſubſequent 32 minutes duration of the experiment. The other thermometer was treated in the ſame manner as that which received the refracted heat from a candle in experiment 12; that is to ſay, the lens was alternately placed and removed, and the variations were—riſe in the firſt 9 minutes, 9¾ degrees; in the next 5 minutes, before the uncondenſed rays of the fire, fall 2¼ degrees;—in the next 10 minutes riſe 1¾ degrees; in the next 5½ minutes, fall 3 degrees; and in the next 4½ minutes, riſe 1½ degrees.—So that in the courſe of 35 minutes, the thermometer was raiſed and depreſſed 5 times by rays which came from the culinary fire, and were ſubject to laws of refraction, not ſenſibly different from thoſe which affect light.

Experiment 15. *Refraction of the heat of red hot iron.* A lump of iron was forged into a cylinder 2½ inches in diameter, and 2½ inches long. This being made red hot, was ſtuck upon an iron handle fixed on a ſtand, ſo as to preſent one of its circular faces to a lens of the dimenſions uſed iu the 12th experiment, at the diſtance of 2,8 inches. Before the lens, at ſome diſtance, was placed a ſcreen of wood, with a hole of an inch diameter in it, to limit the object, and to keep the heat from the thermometers. One of theſe was placed in the ſecondary focus of the lens, and the other, within $\frac{3}{10}$ of an inch of that focus, and at the ſame diſtance from the lens, ſo that if there was any difference in the expoſure, it was in favour of the laſt thermometer, which ſtood oppoſite a thinner part of the lens. During the experiment, the thermometers were alternately ſcreened from the effect of the lens, and expoſed to it for the ſame length of time, and the effects were, that both underwent variations of increaſed and diminiſhed temperature; but that which was in the focus gained 6 degrees in the firſt two minutes, whilſt that near the focus gained only 4; and the firſt loſt 3 degrees in the ſecond two minutes during the time the lens was ſcreened, whilſt the ſecond loſt only 2; and ſo in three other alternations the alternations of temperature in the focus were 2°, 2¾°, and 1¼°, whilſt the correſponding changes out of the focus were 1°, 1¼°, and 0½. To remove all doubt on the ſubject, the thermometers were left in their reſpective ſituations, and a plain glaſs ſubſtituted inſtead of the lens. In theſe circumſtances the laſt related proceſs of alternate covering and uncovering the glaſs was repeated, and both thermometers were found to be affected alike.

Experiment 16. *Refraction of fire heat by an inſtrument reſembling a teleſcope.* This inſtrument conſiſted of a concave mirror, which received the heat from the fire, a plain mirror which reflected it to one ſide, where there was a lens at the uſual eye aperture. A thermometer was placed in the focus of this lens, and another near the focus, and the ſame method of comparative examination was adopted as in the laſt experiment. The reſult alſo was ſimilar, for which reaſon I ſhall conſult brevity ſo far as to avoid repeating the particulars in detail.

Experiment 17. *Refraction of the inviſible rays of ſolar heat.* One half of the before mentioned burning lens was covered, and the priſmatic ſpectrum thrown upon the cover, ſo that the laſt viſible red colour falling one-tenth of an inch from the margin of the paſteboard, the inviſible rays beyond the ſpectrum were ſuffered to fall upon the lens. In the focus of the red rays, or a very little beyond it, was placed the ball of one of the thermometers, and the other thermometer as near to it as convenient. The thermometer in the focus was raiſed 45° in one minute, whilſt the other thermometer underwent no change.

As a little of the red colour was diſcernible on the ball of the thermometer, the Doctor was induced to try if the inviſible rays could not be rendered perceptible to the ſight by condenſation; and with this intention the next experiment was made.

Experiment 18. *Trial to render the inviſible rays of the ſun viſible by condenſation.* The laſt deſcribed experiment was repeated by withdrawing the lens till the extreme viſible colour was $\frac{2}{10}$ from the margin of the ſemicircular paſteboard cover. In theſe circumſtances there was no longer any tinge of colour or veſtige of light to be ſeen on the ball of the thermometer; but, nevertheleſs, the effect of the inviſible rays was ſuch, that the thermometer in the focus received 21° of heat, whilſt the other near the focus remained ſtationary. The colour ſeen in the 17th experiment muſt therefore, as the Doctor obſerves, have ariſen from the imperfect refraction of a lens of ſo great a diameter; the difficulty of aſcertaining the termination of a priſmatic ſpectrum in a room not perfectly dark, and the gradual diffuſion of rays of the ſame kind over a conſiderable ſpace in conſequence of the breadth of the priſm.

Experiment 19. *Refraction of inviſible culinary heat.* This experiment is attended with ſome difficulties, on account of the feeble ſtock of heat in a red hot lump of iron, or other ſimilar thing, and the ſpeed with which it is carried off. The Doctor was prevented from adopting ſome contrivance to keep up this heat from the conſideration that the alternate riſing and falling of a thermometer in the focus of a lens, ſucceſſively covered and uncovered, muſt be aſcribed to the refraction of heat. The lens of 1,4 focus was placed before the cylinder of iron of experiment 15, made very red hot, and the thermometer placed in its focus was for each ſucceſſive two minutes firſt expoſed to the heat, and then ſcreened from it by a ſmall piece of paſteboard. The iron was at the beginning very red hot. The changes, after the iron had ceaſed to emit any light perceptible in a darkened room, were eight in number, and regular, amounting to one degree at the beginning, and at the end the effect of the condenſed rays exceeded but half a degree the loſs of thoſe which were ſtopped by the lens.

Experiment 20. *Confirmation of the laſt experiment.* The 19th experiment was repeated with an aſſiſtant thermometer. The principal thermometer being placed in the focus, as before the aſſiſtant thermometer was placed $\frac{3}{4}$ of an inch diſtant on one ſide, and, conſequently, was expoſed to the direct action of the heated cylinder. The ſcreen was never advanced before this laſt. During eight minutes, the thermometer in the focus being uncovered for the firſt minute, covered for the ſecond, uncovered for the third, and ſo forth, came to its maximum by alternate greater elevations and ſmaller depreſſions, and afterwards loſt its heat by greater depreſſions and ſmaller elevations. But the other thermometer being out of the reach of refraction, acquired its maximum of heat gradually, in conſequence of an uniform expoſure to the heated cylinder, and afterwards began to decline.

After the firſt eight minutes, the aſſiſtant thermometer was removed to one ſide of the focus, ſo as to participate of the alternate ſcreening, and alſo to receive a ſmall ſhare of one ſide of the inviſible heat image, which muſt be formed in the focus of the lens. Here, in confirmation of the reaſoning flowing from the general facts and doctrines, the aſſiſtant thermometer ſhould be affected by alternate riſings and fallings, but leſs conſiderable than thoſe in the focus. The changes of the focal thermometer were for ſix alternations in ten minutes $-\frac{3}{4}+\frac{1}{2}-1\frac{1}{4}+\frac{3}{4}-1+1$, and thoſe of the aſſiſtant thermometer were $-\frac{1}{4}+\frac{1}{4}-\frac{3}{4}+\frac{1}{4}-\frac{1}{2}+\frac{3}{4}$, all which, as our author obſerves, ſo clearly confirm the effect of the refraction of the lens, that it muſt be evident that there are rays iſſuing from hot iron, which, though in a ſtate of total inviſibility, have a power of occaſioning heat, and obey certain laws of refraction, very nearly the ſame with thoſe that affect light. From the whole retroſpect of his paper, he adds, that it will be eaſy to perceive that he has made a good proof, in this diviſion of his memoir, of the three firſt of his propoſitions.

VI.

On the Arragonite of Werner. By Cit. Hauy[*].

THE mineral which is the object of this Memoir preſents itſelf in the form of hexahedral cryſtals of a dirty, uneven, violet colour; ſome of them have their baſes ſmooth, but commonly tarniſhed, whilſt others inſtead of baſes have a multitude of angular projections. Their ſpecific gravity is 2,9455; their refraction double in a very ſtriking degree. They eaſily ſcratch carbonated lime, and appear to poſſeſs the ſame hardneſs as fluated lime; their powder thrown upon burning coals gives a violet coloured phoſphoreſcence. They are entirely ſoluble with a lively efferveſcence in nitric acid. They are found in Spain between the kingdoms of Arragon and Valencia, which is the reaſon of their being called Arragonites, by Werner. There are ſome alſo to be met with near the Pyrenean Mountains.

Their phoſphoreſcence, together with their colour, induced Born to ſuſpect the preſence of phoſphate of lime; but Klaproth found only carbonate of lime.

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[*] Bulletin de la Soc. Philomath. No. 33. tome 2.

Hauy having endeavoured to divide ſome of theſe cryſtals mechanically, obſerved that they had natural planes of junction ſituated parallel to their priſms which indicated a ſtriking difference between their ſtructure and that of carbonated lime. He has alſo remarked, that there is a difference of ſeveral degrees in the mutual incidences of the faces of the priſm, and that this inequality varies in different cryſtals. This variation of the angle would be inexplicable on the hypotheſis of a ſimple cryſtal where there ſhould be unity of ſtructure; but the aſperities diverging from the center towards the faces, and the ſtriæ which render the ſurface unequal, prove that they are formed of ſeveral cryſtals grouped together in the following manner.

The cryſtal which may be conſidered as the element of the group is a cuneiform octahedron. The four trapeziums, which form the four large faces, incline 116° on one ſide, and 64° on the other. The incidence of the triangular faces upon each other, at the place where they re-unite in a common edge perpendicular to the axis, is about 70°. Theſe octaedrons are divided by ſections, parallel to their larger faces. Conſidering theſe octaedrons as quadrangular priſms with dihedral ſummits; and ſuppoſing that their planes incline to each other in an angle of 120°, it is evident that three of theſe priſms applied together will form a regular hexaedral priſm whoſe baſe, inſtead of being plane, will preſent three ſaliant edges which will unite at the axis of the priſm.

But ſuppoſing the faces of the priſm to incline in angles of 116° and 64°, as is the caſe in nature, then three of the priſms cannot exactly fill a ſpace; but two of them, for example, A and C, fig. 3. Pl. XV. will leave a concave ſpace between them of 12 degrees.

To fill this ſpace the cryſtallizing proceſs employs a fourth priſm D, which appears to penetrate in part the priſm C, ſo that in the ſolid which reſults from this re-union the two faces which cut each other in the centre make an angle of 128°; but as real penetration is impoſſible the ſame thing probably happens here as in thoſe groups of cryſtals which croſſed each other, and which at the place where they ſeem to enter each other, have a plane of junction ſituated parallel to one of the faces which would be produced by virtue of a law of diminution.

The cryſtals whoſe baſe is covered with cuneiform projections are nothing elſe but much more numerous groups of quadrangular priſms, whoſe reſpective arrangement is ſubjected to the ſame conditions.

With reſpect to thoſe cryſtals which have four angles of 116°, and two of 128°, the order of their ſtructure will be underſtood by the bare inſpection of fig. 4. In this caſe the aſſemblage is compoſed of four priſms, A, B, C, D, which ſuppoſing no modification of form, would leave in the interior of the priſm an empty ſpace, indicated by the rhomb, *b*, *z*, *u*, *r*, but this is filled by an extenſion which each priſm receives always in conſequence of a law of diminution; ſo that the priſm A increaſes in a quantity repreſented by the rectangular triangle, *b*, *o*, *z*, &c.

In the cryſtals whoſe baſes are ſmooth a diminution takes place by a ſingle row on the terminating edge of the component priſms.

The vertical ſection of the priſms of the arragonite preſent another ſingularity noticed by Born; it is a kind of croſs, compoſed of four triangles, amongſt which thoſe whoſe baſes are horizontal, are of a pale colour; and the other two, whoſe baſes are vertical, are of a more intenſe violet colour.

Fig. 2. repreſents a priſm of arragonite, where the four cuneiform octahedrons which compoſe it, are very diſtinct.

VII.

On the true Origin of Reſin, known by the Name of Sandarac, *and on that of Gum Arabic,*

by M. Schousboe[*].

SANDARAC is an article of commerce, which is brought from the weſtern provinces of the kingdom of Morocco. From ſix to ſeven hundred quintals are annually ſhipped in the ports of Santa Cruz, Mogador, and Saffy. This reſin is called, in the language of the country *el graſſa.* The tree which produces it is a thuia, which was alſo found in the kingdom of Tunis by M. Vahl, who has given a complete deſcription, and a good figure of it in his work, entitled, Symbol. Botan. part the 2nd, page 96. Pl. XLVIII. by the name of Thuia Articulata. Shaw had formerly made it known, and called it *Cypreſſus, fructu quadrivalvi, foliis equiſeti inſtar articulatis;* but neither of theſe learned men were acquainted with the œconomical uſe of this tree, probably becauſe as it is ſeldom met with in the northern parts of Barbary, too little advantage can be derived from the reſin which flows from it.

Till the preſent time this reſin was attributed either to the Juniperus Communis, or to the Juniperus Lycia, or laſtly to the cedar of Libanus, without conſidering that the Juniperus Communis is not found in Africa; and that the Sandarac appears to come excluſively from that part of the world. M. Schouſboe, who has ſeen the ſpecies of thuia here alluded to, ſays, that its height never exceeds 8 metres (26¼ feet), and that the diameter of its trunk is not more than 20 or 22 (7 inches) centimetres. It is diſtinguiſhed at firſt ſight, from the two other ſpecies of the ſame kind of tree, which are cultivated in our gardens, by having a diſtinct trunk, and the appearance of a real tree, whilſt in the others the branches iſſue out from the root, which gives them rather the reſemblance of buſhes. Its branches alſo are more articulate and more brittle; its bloſſoms, which are not very apparent, diſplay themſelves in germinal, (April), and its fruits, the form of which is nearly ſpherical, ripen in Fructidor, (September).

By preſenting a branch of this vegetable to the light, it is obſerved to be covered with a multitude of tranſparent veſicles, which contain the reſin. After theſe veſicles have burſt in the ſummer months, a reſinous juice iſſues from the trunk and branches by exudation, as in other coniferous trees. This is ſandarac. The inhabitants of the country collect it and carry it to the ports, whence it is conveyed to Europe. We make uſe of it in the compoſition of ſealing wax, and for different kinds of varniſh. In 1793 an hundred pounds weight of it coſt in the ports of Morocco from 13 to 13½ piaſtres, which is equal to about 75 (7½d.) centimes of our money per pound. The duty upon exportation was about 9 francs per quintal.

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[*] Extracted by Ch. Coquebert, from the Daniſh Journal, entitled, Bibliot. de Phyſique, medicine et economie, 3d cahier, 1799, and inſerted in the Bulletin de la Soc. Philom. No. 31.

Sardarac, if good, is of a clear yellow, pure and limpid. It is a commodity rather difficult to adulterate. Care however muſt be taken that the Moors do not mix too much duſt with it.

It is probable the ſame tree which produces ſandarac at Senegal, whence it is exported in rather large quantities.

Another article of commerce, of which the kingdom of Morocco alſo partakes with Senegal, is the gum called Arabic, which bears the name of al leilk. The tree which affords it grows only in the weſtern provinces of that ſtate. The exportation of this ſubſtance to the different ſtates of Europe, amounts, throughout the ports of Morocco, to 8 or 9 thouſand quintals. M. Schouſboe ſays, that this tree is the *mimoſa nilotica*, called by the natives *al thlah*, which is no reaſon why in the ſouthern parts of Africa, it may not be gathered, as authors aſſert that it is, from the *mimoſa ſenegal*, and even from other ſpecies of that genus.

In Barbary, a diſtinction is made between the gum of Senegal and that of the country. The firſt has the preference, on account of its purity, its clearneſs, and its whiteneſs, which are the qualities in general ſought for in this commodity.

The gum which I myſelf collected in the province of Mogador, obſerves M. Shouſboe, exudes from the trunk and the branches of the tree, like that of our fruit trees. It is in round pieces, of the bigneſs of a hazle nut, or at moſt of a walnut. Theſe pieces, indeed, by adhering to each other, ſometimes form maſſes of the bigneſs of the fiſt, or even of the head; but that is only owing to the adheſion which takes place between the gum, ſtill freſh, after it had been ſeparated, and principally from the part which adhered to the bark, where the gummy juice has not had time to harden. When earth, ſmall ſtones, or other foreign bodies, are ſometimes found in theſe maſſes, it is the effects of fraud. M. Shouſboe ſuſpects that it is this circumſtance which has given riſe to the opinion that gum was found at the foot of trees, and that it exuded from their roots; (ſee Bulletin des Sciences, No. 8.) for which he thinks there is no foundation. If that was the caſe, it appears to him that beſides the ſand and the earth, with which the maſſes of gum are accidentally contaminated, ſome of it would be found in the interior part of the globules, and would be even ſo much blended with the mucilaginous ſubſtance, that it would not be poſſible to purify it completely; whereas, on the contrary, the gum which comes from Senegal is ſtill purer than that from Barbary.

M. Schouſboe, nevertheleſs obſerves, that ſandarac, and the gums which are exported through the port of Saffi are of a brown or red colour; but he attributes that colour to the quantity of red oxide which is mixed with the ſoil of the province of Abda, in which this part is ſituated. This oxide communicates its colour even to the whiteſt wool, and the inhabitants of this province are known by the red tinge of their cloaths, which no proceſs can totally deſtroy.

In the months of Meſſidor and Thermidor, when heavy dews fall, the gum loſes much of its clearneſs and other qualities for which it is deſirable.

An hundred pounds weight of this ſubſtance coſt, in the year 1793, at Mogador, about 48 francs of our money, excluſive of 5 francs 70 cents for cuſtom-houſe duties.

The inhabitants of Morocco do not appear to apply this gum to any purpoſe whatever; they ſell all that they can gather to the commercial nations of Europe.

VIII.

Remarks on the Memoir in which M. Girtanner examines whether Azote be a ſimple or a compound Body. By Cit. BERTHOLLET*.

SOME experiments were long ſince made by Dr. Prieſtley, which, at firſt, induced him to believe, that water diſtilled with lime, and particularly with clay, or indeed without any addition, in an earthen retort, or a retort of glaſs, deprived of its poliſh, became changed into air. But he found that this deduction was erroneous, and that it was merely a tranſmiſſion of the ſurrounding air which gave riſe to the circumſtance. Guyton added ſome obſervations to thoſe made by Prieſtley (Encyclop. Vol. I. p. 674), and the fact appeared to be clearly accounted for.

Neverthelеſs, Wiegleb publiſhed a memoir, in which he pretended to prove, by his own, and by ſome other experiments, that the gas which is obtained by paſſing water through red hot earthen tubes might be attributed ſolely to the combination of the aqueous vapour with the matter of heat, and that this combination formed azote gas.

This memoir occaſioned a reply which was publiſhed by Dieman, Van Tſootwyk, and Lauwerenburg. Theſe learned chemiſts carefully examined every circumſtance which had apparently eſtabliſhed the pretended production of azote gas from water; and I ſhould have thought that there could exiſt no doubt reſpecting the conſequence which they drew from their experiments, which was, "that the azote gas which, in ſome caſes, is obtained by paſſing water in vapour through ignited tubes, proceeds merely from the "external air, deprived of its oxygen gas by the fire in which the tubes are placed; and "that, thus, the pretended converſion of water into azote gas, by its combination with "the matter of heat, is overthrown."

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* Annales de Chimie, XXXV. 23. The memoir referred to is inſerted in our Journal, and is concluded at p. 275 of the preſent volume.

After all theſe diſcuſſions, Girtanner ſupports the new ideas which he preſents in his memoir, upon ſeveral facts, for the moſt part borrowed, and already accurately diſcuſſed, amongſt which, the following muſt be conſidered as fundamental. "When water is boiled "in a retort of glaſs, or other material, azote gas is obtained." The author preſcribes theſe precautions: "In order to obtain azote gas in the greateſt quantity, the water muſt "evaporate ſlowly, and over a very gentle fire, which muſt not be raiſed."

"It is invariably obſerved in all theſe experiments, that when the laſt drop of water "evaporates, the azote gas is no longer produced, though the heat be kept up."

An aſſertion ſo poſitive engaged me to repeat the experiment, with the precautions preſcribed by Girtanner. It was made with water newly diſtilled, and with the well waſhed precipitate of alum by pot-aſh. It was likewiſe made with a very white clay, with which I was ſupplied by Cit. Guyton; but though the quantity of water was conſiderable, and, conſequently, theſe two experiments were of long duration, no gas became diſengaged, and the reſult was the ſame as that of the Dutch chemiſts.

It is, nevertheleſs, upon this fact, which is ſo eaſy to be repeated, that Girtanner principally eſtabliſhes the compoſition of azote, and thence that of the atmoſphere, "which "is not, as till now has been imagined, a mixture of oxygen gas and azote gas, but rather "a mixture of oxygen gas and hydrogen—water in the form of gas, if I may be allowed "to make uſe of ſuch an expreſſion. When, by thoſe chemical experiments which very "improperly have been called eudiometrical, the oxygen is ſeparated from the hydrogen, "this ſeparation can never be entirely or completely made; a part of the oxygen remains "united to the hydrogen, and forms the chemical combination which we call azote, and "which we obtain in theſe experiments."

So that when we make a mixture of oxygen and hydrogen gas, we form atmoſpheric air, and the differences of ſpecific gravity and other properties in all our experiments, as well as the products of combuſtion, are only ſlight circumſtances, of which Girtanner takes no account.

The memoir of Girtanner induced Citizen Bouillon la Grange to make experiments more numerous than mine, but which lead to the ſame concluſions. I ſhall here preſent a ſhort account of them, which he has kindly communicated to me.

He did not obtain any azote gas by boiling diſtilled water either alone, or with clay or alumine in a glaſs retort, to which he applied a tube of glaſs or of porcelain.

In like manner no diſengagement of azote gas took place when the ſame experiment was made with ſiliceous earth obtained by fluoric acid.

Having placed lime, obtained from white marble, in a tube of the ſame kind, he paſſed the vapour of water through this tube, which had its extremity immerſed in lime water. A ſmall quantity of carbonic acid was diſengaged, which formed carbonate of lime, but no azote gas was produced.

If, inſtead of lime, clay is made uſe of with the ſame apparatus, a little carbonate of lime becomes formed, but there is no diſengagement of azote gas.

If water be paſſed through a tube of porcelain containing lead, this metal is converted partly to the ſtate of yellow oxide, and becomes vitrified; but there is no diſengagement of azote gas.

If, inſtead of lead, tin is put into the tube, that metal becomes oxidated, and hydrogen gas is obtained. Zinc affords the ſame reſult.

To aſcertain whether this hydrogen gas did not contain azote gas, Citizen Bouillon la Grange cauſed it to paſs over ſulphur in fuſion, and he obtained ſulphurated hydrogen gas, but no azote. He afterwards ſubjected it to another trial.

He mixed the hydrogen gas which he had obtained by means of zinc, with oxygen gas; he ſet the mixture on fire by an electric ſpark; water was formed, but there was no azote gas.

The opinion of Girtanner agrees with that of Humboldt, reſpecting the abſorption of oxigen by ſimple earths, and particularly by alum; there is, nevertheleſs, this difference, that Humboldt conſiders the ſuppoſed phenomenon as a ſimple ſeparation of the oxygen which becomes fixed from the azote which remains in the gaſeous ſtate; but according to Girtanner, "the azote which is obtained in our experiments being always produced by " operation itſelf, and having no exiſtence in the form of azote before the experiment in " the air which is examined, Von Humboldt, who is fond of drawing general concluſions " from ſolitary facts appears to be deceived, when he aſſerts that earths may be uſed for " the purpoſe of determining the quantity of azote contained in the atmoſpheric air: for the " earths do not indicate that azote is contained in atmoſperic air; they change this air into " azote."

The younger Sauſſure has formerly contradicted the reſult announced by Humboldt, (Journ. de Phyſ. frim. an. 7.) He allows that the humus, which is the reſult of the mixture of decompoſed vegetables with other vegetables which are not yet decompoſed, abſorbs the oxigen gas, and that this is a known fact; but he atteſts, " that this effect never takes " place when the earths are pure and deprived of every vegetable ſubſtance." He deſcribes ſeveral experiments which he made with alumine, lime, and ſilex.

In the journal of Pluvioſe, of the ſame year, there is a reply from Humboldt, written in a magiſterial tone. It contains nearly the ſame aſſertions as are contained in the firſt Memoir with the addition, however, (as a kind of ſanction), that his obſervations were made in the laboratories of Vauquelin and of Fourcroy. This authority would undoubtedly be of great weight, if theſe two learned chemiſts had co-operated in the obſervations; but very far from that being the caſe it is viſible from the account that the only experiments, which were attempted in their preſence, did not ſucceed.

I have been aſſured that the celebrated Fabroni, about the ſame period, repeated the experiments of Humboldt, at Florence, without any ſucceſs.

Champy, the ſon, a very accurate obſerver, repeated them at Cairo upon alumine, upon lime, and upon the mud of the Nile, with a temperature which varied from 30 to 36° degrees of the centigrade thermometer, but he obtained no abſorption. It muſt be remarked, that the mud of the Nile contains ſome remains of vegetable ſubſtances; ſo that, when it is diſtilled a certain quantity of carbonic acid and of carbonated hydrogen gas is obtained.

Chaptal repeated them at Montpelier, without obtaining abſorption.

I have kept moiſt alumine for a conſiderable time in contact with atmoſperic air and oxigen gas, without obſerving the leaſt abſorption. It having been mentioned by a chemiſt that abſorption was cauſed by agitation; the effect of agitation was patiently tried, but the reſult was always the ſame. The operation has been renewed with the white clay which had been ſent to me by Guyton, but it alſo produced no abſorption. This clay, nevertheleſs, when urged by the fire, yielded a little carbonic acid and a ſmall quantity of carbonated hydrogen gas.

IX.

On the Decompoſition of the Muriatic Acid. By Mr. JOHN PITCHFORD, *Junior.*

To Mr. NICHOLSON.

SIR,

IF you think the following obſervations of ſufficient importance, you will oblige me by inſerting them in your uſeful miſcellany.

I am, Sir,
Your's, &c.

Norwich, Aug. 16, 1800. JOHN PITCHFORD, Jun.

I have long conſidered the analyſis of the muriatic acid to be a point of the firſt importance in the preſent ſtate of chemical knowledge. Among many experiments to aſcertain its compoſition, which have occurred to me, one was founded on Dr. Fordyce's experiments, recorded in the Philoſophical Tranſactions for 1792, the object of which was to diſcover whether, in the ſolution of zinc in dilute ſulphuric acid, the metal derives the oxygen neceſſary to render it ſoluble from the acid, or from the water. The reſult of a ſeries of very accurate experiments is well known to have been, that the water furniſhes the whole of the oxygen, and that no portion of the acid is decompoſed. The proof of this is that it requires as much pot-aſh to precipitate the zinc from a certain quantity of acid as to ſaturate an equal quantity of acid in which no zinc has been diſſolved.

Wiſhing to aſcertain whether this would alſo be the caſe with muriatic acid, I diſſolved one ounce of tin in four ounces of this acid of ſpecific gravity, 1178. During the ſolution a conſiderable quantity of hydrogen gas was evolved. When the ſolution was completed I added pot-aſh till the whole of the tin was precipitated, and the acid ſaturated, which took place when 1412 grains of pot-aſh had been added. I next tried how much pot-aſh four ounces of the ſame muriatic acid would require for ſaturation, and found that 1717 grains were required. Now allowing that ſome ſmall portion of the muriatic acid, may have been volatilized during its ſolution of the tin, and carried off by the hydrogen gas during its eſcape, this cannot be ſuppoſed to account for more than a very ſmall proportion of the acid which diſappeared. I therefore infer that when tin is diſſolved in muriatic acid, a part of the acid is decompoſed, and I think it not improbable that it ſupplies the tin with oxygen, and that its other component part hydrogen flies off in the form of hydrogen gas. I am well aware that many more experiments ought to be tried before it can be eſtabliſhed that oxygen and hydrogen are the conſtituent principles of muriatic acid; but one thing is I think fully eſtabliſhed by this experiment, namely, that ſome portion of this acid is decompoſed during the ſolution of tin. For if in Dr. Fordyce's experiment, in which two equal portions of ſulphuric acid, in one of which zinc had been diſſolved, and in the other not, required equal quantities of pot-aſh to ſaturate them, it was a fair inference, that therefore ſulphuric acid is not decompoſed in diſſolving zinc; it is equally fair to infer from my experiment in which the muriatic acid, in which tin had been diſſolved, required ſo much leſs pot-aſh to ſaturate it than an equal quantity of the ſame acid in which no tin had been diſſolved, that part of the muriatic acid is decompoſed in diſſolving tin.

I am alſo ſenſible that the above experiment might be tried in a much better way: the quantity of water contained in the muriatic acid ought to be aſcertained previous to the ſolution of the tin, and after its precipitation by the alkali, and a calculation then made, whether the portion of water miſſing would account for all the hydrogen and oxygen produced: if it appeared that the quantities of theſe were greater than the water miſſing would furniſh, then I think it would be eſtabliſhed that they entered into the compoſition of muriatic acid. Another important experiment would be to expoſe tin to muriatic acid gas, perfectly free from water (a method of obtaining, which appears in Mr. Henry's experiments, and will be hereafter mentioned), and examine whether it becomes oxidated, and to expoſe not only tin but every other metal, and every other ſubſtance, likely to ſeparate the oxygen of this acid from the unknown radical. I am neither in poſſeſſion of an apparatus ſufficiently delicate, nor have I opportunity at preſent to try theſe and ſimilar experiments, but having ſeen in the Philoſophical Tranſactions for 1800, page 188, that Mr. William Henry has been engaged in experiments on the decompoſition of this acid, I have thought it right to ſuggeſt the above hints.

The chemical world has great obligations to Mr. Henry for the labour and patience he muſt have expended on his very intereſting experiments. I ſhould wiſh to be permitted to make a few obſervations which reſulted from an attentive peruſal of his paper.

The firſt ſeries of his experiments conſiſted in paſſing the electric ſhock repeatedly through muriatic acid gas confined over mercury. The effect was the diminution of this gas, the production of hydrogen gas, and the oxidation of the ſurface of the mercury: all this he attributes not to any decompoſition of the acid, but to that of the water, which in the dryeſt form, in which the gas is uſually produced, ſtill adheres to it. As theſe effects took place only for a limited time, after which continued electriſation produced them no longer, and as a portion of muriatic acid gas always remained entire, this inference is probably true, and the experiments prove that the electric matter at leaſt is incapable of decompoſing muriatic acid. But though no progreſs may have been made towards the decompoſition of the acid by theſe firſt experiments, they are yet far from being uſeleſs, ſince we are now ſupplied with the means of obtaining the muriatic acid perfectly free from water, namely, that portion which, after repeated electriſation, remains unaltered. I have no further remark to make on the firſt ſeries of experiments, except that there appears to be ſome obſcurity in the 5th, in which 143 meaſures of common air, and 116 of muriatic acid gas, were electriſed, and by 30 ſhocks reduced to 111. " The remainder " conſiſted of muriatic acid and azote gaſes, with a ſmall proportion of oxygenous gas." What, then, became of the hydrogen gas?

The ſecond ſeries of experiments was to aſcertain the effect produced by electriſying the muriatic acid gas with inflammable ſubſtances. Mr. Henry previouſly obſerves, that he has attempted the decompoſition of this acid by paſſing it over red hot charcoal. " An " immenſe production of hydrogenous gas took place, but it was not eaſy to determine " whether it had its origin from real acid or from water." I cannot help being ſurpriſed that Mr. Henry ſhould not have decided this point by paſſing muriatic acid gas entirely freed from water by electriſation, over red hot charcoal, as this was ſo obvious a method of aſcertaining whether charcoal be capable of decompoſing muriatic acid.

In the 12th experiment, Mr. Henry paſſed 200 ſhocks through a mixture of 83 meaſures of carbonated hydrogen gas, and 89 of muriatic acid gas. The permanent reſidue, after the admiſſion of water, was 101 meaſures; the addition, therefore, amounted to 18; of theſe 6 may be accounted for by the decompoſition of the water of the muriatic acid gas, and 10 by that of the carbonated hydrogen gas. There remain, therefore, only 2 meaſures that can be ſuppoſed to be produced from the muriatic acid gas; a quantity, Mr. Henry obſerves, too ſmall to afford grounds for ſuppoſing them to ariſe from decompoſed acid. Now, in the decompoſition of the water of the carbonated hydrogen gas by the carbon, the oxygen of the water unites with the carbon, and forms carbonic acid, which being ſoluble in water, would on the admiſſion of water, after the electriſation, wholly, or in part, diſappear; ſo that the increaſe is greater than 2, by all the carbonic acid diſſolved in the water. It is not, therefore, improbable that the muriatic acid gas was in part decompoſed in this experiment, and that the reaſon why more of it was not decompoſed is owing to there not having been more charcoal preſent, part of what there was being employed in decompoſing the water. Indeed, when it is conſidered that the carbon in carbomated hydrogen gas is minute in quantity, and that it has to decompoſe the water contained in the two gaſes before it can act on the muriatic acid gas, it appears that this is not the beſt method of aſcertaining whether carbon can decompoſe muriatic acid. There appears to me no better method than to paſs muriatic acid gas, perfectly freed from water, over red hot charcoal, and to examine whether carbonic acid and hydrogen are produced.

X.

Obſervations on the Action which the Sulphate of Iron exerciſes upon Nitrous Gas, and on the Formation of Muriatic Acid. By Citizen BERTHOLLET*.

M. HUMBOLDT (in the Bulletin des Sciences, No. 71 & 21) had aſſerted that the faculty which ſulphate of iron poſſeſſes of abſorbing nitrous gas without the azote which might be mixed with it, was a means of aſcertaining with preciſion the purity of the nitrous gas employed in eudiometrical experiments.

Citizen Berthollet, on the contrary, is of opinion, that in this caſe the nitrous gas is not only abſorbed, but that it is decompoſed; that the oxygen abandons a part of the azote, and forms, with the other part, nitrous acid; that this decompoſition equally takes place by means of water, mercury, ſolution of pot-aſh, or of hydrogenated ſulphuret of alkali; that it is more or leſs complete in proportion as the liquid, in contact with this gas, contains bodies which have a greater degree of affinity to nitrous acid, in the formation of which they aſſiſt, and nitrites are formed; that in the decompoſition by water, there is leſs nitrate of ammoniac and more nitrous acid formed than M. Humboldt imagined; which tends to prove that water did not furniſh the whole of the oxygen of the acid, ſince the quantity of acid thus formed, is far more conſiderable than may be preſumed from the exiſting quantity of ammoniac. It is to be remarked that the decompoſition becomes more difficult as it is more advanced, and the gas contains leſs oxygen. Citizen Berthollet attributes the differences which are obſervable in nitrous gas, to the different proportions in which the oxygen and azote are combined, and he does not think they are owing to azote in a ſtate of ſimple mixture.

It is a known fact, that oxigenated muriatic acid, which has no action upon azote, abſorbs nitrous gas very well. M. Humboldt obſerved in this abſorption a reſidue, which he attributed to azote mixed with the nitrous gas. Citizen Berthollet having repeated the experiment with nitrous gas, carefully prepared, found a reſidue ſo ſmall as not to deſerve notice.

In a word, Citizen Berthollet reſtores to hydrogenated ſulphuret of pot-aſh and to phoſphorus the property of taking up all the oxygen of the atmoſpheric air; a property which had been conteſted by M. Humboldt, who ſays, that nitrous gas always indicated the exiſtence of a reſidue of oxygen in air which had been ſubmitted to their action. Citizen Berthollet abſolutely maintains the contrary. Nitrous gas experienced only a ſlight diminution with the reſidue of atmoſpheric air decompoſed by phoſphorus acid; and he attributes this to the abſorption which takes place in the nitrous gas of that portion of phoſphorus which had been taken up by the azote.

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* Abridged from the Memoirs communicated to the French National Inſtitute (in the Bulletin de la Soc. Philom. No. 40.)

On the Nature of Muriatic Acid.

Two circumſtances induced Citizen Berthollet to entertain ſuſpicions, and to inveſtigate the nature of this acid.

The one is that which is announced in the Bulletin, No. 17, page 165, where M. Humboldt ſays, that muriate of iron is formed by the abſorption of nitrous gas, by means of ſulphate of iron.

The other, which had been obſerved by Cavendiſh, is the precipitation of nitrate of ſilver into muriate, by the nitrite of pot-aſh obtained from nitrate of pot-aſh in part decompoſed by fire. By adding to theſe obſervations the circumſtance of the almoſt invariable preſence of muriatic acid in every caſe where nitric acid is formed, and by means of ſeveral other experiments carefully made, Citizen Berthollet was led to the diſcovery of the nature of muriatic acid.

He aſcertained that nitrous gas does not precipitate the ſolution of ſilver. By repeating the experiment of Cavendiſh, with nitrate of pot-aſh, he found it accurate, and was moreover aſſured that the precipitation could not proceed from the nitrous gas of the nitrite; for,

1ſt, Nitrate of lime does not produce the ſame reſult.

2d, If a ſolution of iron be made in nitric acid, and it be ſo managed as to load it with this metal, little ammoniac is formed; the ſolution becomes turbid, and it does not precipitate the ſolution of ſilver. If a freſh quantity of iron be added to this ſolution, there is an effervеſcence, with a precipitation of almoſt all the oxide of iron, the liquid contains more ammoniac, together with muriatic acid, which is eaſily demonſtrable by the ſolution of ſilver. If this liquid be diſtilled, that which comes over contains nothing but ammoniac; the muriatic acid remaining in the retort with part of the ammoniac.

3d. Nitric ſolutions of tin, of zinc, of copper, made by heat, have ſometimes yielded muriatic acid. But it is to be remarked that this acid was more generally found in proportion as the production of ammoniac was conſiderable.

In theſe experiments there are anomalies, the cauſe of which citizen Berthollet has not yet been able to aſſign. But they already prove that muriatic acid is formed under theſe circumſtances, and that its formation cannot be attributed to the preſence of pot-aſh. The principles of this acid muſt therefore be ſought for in water and nitric acid.

Its incombuſtibility, its reſiſtance to decompoſition, prove, that if it contains hydrogen and oxigen, theſe two bodies are not predominant, for it is a principle of the theory of affinities, that the difficulty of deſtroying a compound is in proportion to the ſmallneſs of the quantity of one of its component parts. Almoſt all the proportions of the combinations of azote and oxigen are known. Citizen Berthollet, therefore, ſuppoſes himſelf to be juſtified in thinking that muriatic acid is a triple compoſition of oxygen, of hydrogen in a ſmall quantity, and azote in a greater proportion. By adopting this opinion, the preſence of muriatic acid, in a great number of chemical phenomena, is eaſily explained.

Thus it appears (experiment 2.) that it is at the moment when the iron has decompoſed almoſt all the nitric acid, and it becomes neceſſary alſo to decompoſe the water, in order to oxidate more metal; that the muriatic acid, and the greateſt part of the ammoniac, is formed by the decompoſition of the water.

It is to the preſence of oxygen, azote, and hydrogen, that citizen Berthollet attributes the formation of muriatic acid in artificial nitre beds, when the materials do not previouſly contain any muriate.

Although muriatic acid, from the proportions of its conſtituent principles, ought ſtrongly to reſiſt decompoſition, citizen Berthollet thinks that he has obſerved that this decompoſition takes place under ſome circumſtances.

He thinks that the reſidue which is left by oxygen gas diſengaged by heat, from oxigenated muriate of pot-aſh, is owing to the decompoſition of a ſmall portion of this acid. He had, at firſt, attributed this reſidue to a foreign cauſe; but having remarked that it was more abundant at the end than at the beginning of the operation, he conceived it could not reſult from ſuch a cauſe.

Citizen Berthollet cloſes his Memoir, by ſhewing, from more accurate experiments, that the black colour which muriate of ſilver aſſumes from light, heat, and even from a ſimple current of air, ought not to be attributed, as he himſelf had ſuppoſed, to a gaſeous diſengagement of oxygen, but to the ſeparation of a part of the muriatic acid, without decompoſition.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Philoſophical Tranſactions of the Royal Society of London, for the Year 1800. *Part the Second. Quarto,* 46 *Pages, with Eleven Plates.* London, ſold by Elmſley.

THIS part contains.—12. On double images, cauſed by atmoſpherical refraction. By William Hyde, Wollaſton, M. D. F. R. S. (ſee our preſent, vol. p. 298.) 13. Inveſtigation of the powers of the priſmatic colours to heat and illuminate objects; with remarks, that prove the different refrangibility of radiant heat. To which is added, an enquiry into the method of viewing the ſun advantageouſly, with teleſcopes of large apertures and high magnifying powers. By William Herſchel L. L. D. F. R. S. 14. Experiments on the refrangibility of the inviſible rays of the ſun. By William Herſchel, L. L. D. F. R. S. (abridgment of theſe two papers is given at p. 320) of our preſent volume. 15. Experiments on the ſolar, and on the terreſtrial rays that occaſion heat; with a comparative view of the laws to which light and heat, or rather the rays which occaſion them, are ſubject, in order to determine whether they are the ſame, or different. By William

Herſchel, L. L. D. F. R. S. (ſee p. 360 of this number). 16. Chemical experiments on zoophytes; with ſome obſervations on the component parts of membrane, by Charles Hatchett, Eſq. F. R. S. 17. On the electricity excited by the mere contract of conducting ſubſtances of different kinds. In a letter from Mr. Alexandar Volta, F. R. S. profeſſor of natural philoſophy in the univerſity of Pavia, to the Right Hon. Sir Joſeph Banks, Bart. K. B. F. R. S. (abridged in this Journal, IV. p. 179.)

Foſſil Wood, found at a very great Elevation.

Citizen Villars has communicated a Memoir to the national inſtitute of France, in which he relates his having ſeen, in the department of *Iſere*, foſſil wood, bedded in turf, at the height of 2320 metres above the (actual level of the ſea, and 850 metres above the moſt elevated ſituation, where wood grows, at preſent. The mountain where he has made this intereſting obſervation, is that of Lans, in the canton of Aiſans. The trees which are found are The roots, and parts of the trunk are obſervable. The latter of theſe trees no longer grows in the vicinity.

The author attributes the refrigeration which theſe mountains have experienced to two leading cauſes; the firſt is the excavation of the valleys, which has changed the elevation of the ſummits relative to their baſe, and the ſurrounding country; and the ſecond is the deſtruction of the ancient foreſts, which extended contiguouſly upwards to great heights, but cannot again grow at ſimilar elevations, when once deſtroyed, and the trees thus deprived of their mutual ſhelter and defence. Soc. Philom. No. 33.

Extract of a Letter from Mr. Humphry Davy, dated October 23. ſupplementary to his Paper on Galvaniſm, in the preſent Number.

When I exhibited to Dr. Beddoes the phænomenon of the renovation of the powers of the galvanic pile, after it had ceaſed to act in hydrogen, nitrogen, &c. by momentary immerſion in water, holding in ſolution atmoſpheric air, he requeſted me to try whether oxigenated muriatic acid gas, ſolution of nitre, &c. would not increaſe the effects. The fear of deſtroying the ſilver prevented me at the time from trying the experiment on oxigenated muriatic acid gas. I have ſince, however, in endeavouring to aſcertain whether the electrical phænomena of galvaniſm were producible when the plates of the pile were oxidating, (ſeparated from each other by a non-conductor, and in contact with an aeriform fluid only) had an opportunity of obſerving the very great power of this ſubſtance in producing the galvanic decompoſition of water.

Ten pairs of plates of ſilver and zinc were connected by cement, ſo as to prevent the alternate contact of the metal. The ſpaces filled by cloths in the common pile, were

uffered

ſuffered to remain open, ſo as to admit of a free circulation of air. A tube, with water, and ſilver wires, was adapted to the extreme plates.

This pile was introduced, without being moiſtened, into a veſſel, provided with a ſtopper, filled with oxigenated marine acid gas; but no perceptible galvanic action took place. After two hours, no gas had formed in the tube, nor had any oxidation of the zinc wire been produced *. The ſame pile was now moiſtened by immerſion in water. Before and after it had been wiped, it ſhewed no ſigns of action in the atmoſphere. It was introduced into a veſſel of oxigenated marine acid gas, opened as before in the atmoſphere. In a moment, the zinc wire, in the tube, began to oxidate with the greateſt rapidity, whilſt gas was given plentifully from the ſilver wire. The proceſs continued to go on till the green colour had diſappeared in the cylinder.

This experiment not only arranges with the facts of Fabroni and Colonel Haldane, and thoſe I have before ſtated; but likewiſe ſeems to prove that the chief uſe of the large ſurface of water required in the pile of Volta is to oxidate a larger quantity of zinc: for in this inſtance very minute quantities of water connected the plates, and conſequently very minute quantities were ſufficient to enable the electrical currents to form the circuit.

I am at preſent engaged in endeavouring to aſcertain by experiments whether any differences exiſt in the gaſes evolved from water by the galvanic current, when different oxidating ſubſtances form the medium of communication between the plates. When theſe experiments are compleated, or at ſome future time I ſhall probably offer ſome obſervations on the peculiar affinities which enable iron, zinc, &c. to decompoſe water only when it holds in ſolution atmoſpheric air, acids, or other bodies containing oxygen. On the principles before laid down, nothing is more eaſy than to explain the uſe of muriate of ſoda, muriate of ammoniac, &c. in increaſing the powers of the common pile.

As the quantity of power in a pile is probably in proportion to the quantity of the oxidation of the zinc, and the number of the ſeries of plates, the pile of Mr. Cruickſhank, or that I have deſcribed, will probably be moſt uſeful for proceſſes, in which much galvanic power is required. In thoſe proceſſes, muriatic acid, or very diluted nitrous acid, may be uſed as the oxidating ſubſtance: for they will enable the plates to act till all the oxidable metal is deſtroyed, without the common trouble of cleaning and rebuilding the pile.

Analyſis of various Foſſils.

Trommsdorff, who has lately been engaged in mineral analyſes, has found, that the lapis obſidianus of mount Heckla in Iceland is compoſed of 63 parts of ſiliceous earth, 20.5 parts of alumine, and 13.5 parts of oxide of iron.

That the heliotrope of Bohemia contains 68 parts of ſiliceous earth, 15 of alumine, 10 of oxide of iron.

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* The metals of the pile had been acted upon in this experiment, and were warm at the concluſion of it.

That the black felſpar which is found at Unkel in the Baſaltes, is compoſed of 66.5 parts of alumine, 15.0 of ſiliceous earth, 6.5 of oxide of iron, and 4.0 of oxide of manganeſe.

That the blue calcedony of Siberia contains only ſiliceous earth.

That rock cryſtal is the ſame as pure ſiliceous earth.

That the Holzſtein of Bareuth contains 80.0 parts of ſiliceous earth, 11.0 of alumine, 3.0 of oxide of iron, 1.0 of oxide of chrome; which proves that chrome is alſo found in Germany. He ſuſpects that it is likewiſe to be found in ſeveral other of the foſſils of this country.

Lime in the Nut-gall.

Prouſt did not obtain lime from the nut gall; Trommsdorff aſcertained the preſence of this earth with preciſion.

The method which Prouſt propoſes to procure pure gallic acid is not advantageous; at beſt it is good for ſeparating this acid from the tanning principle, but it does not diſengage it from the extractive matter, and when it is propoſed to ſeparate the muriatic acid by evaporation, the elevated temperature which is neceſſary to effect it, reſolves the gallic acid into its principles, or at leaſt carboniſes it.

Extraction of the Alkali of Sea Salt by Lime.

The decompoſition of ſulphate of ſoda by lime in order to obtain that alcali cannot be advantageous, becauſe this earth attacks the ſalt only in proportion as it is itſelf diſſolved in water.

Method of obtaining Phoſphorus by Phoſphate of Lead.

Trommsdorff has made new attempts to render Giobert's proceſs uſeful, by precipitating urine by nitrate and acetite of lead; but the phoſphorus which he obtained was in ſuch ſmall quantities, that it did not pay the expence of the fire. The chemiſt of Turin would be juſtified in affirming that his method is the beſt, if his precipitate had been the pure phoſphate of lead. But the reduced lead which remains in the retort is the phoſphuret of that metal.

On the Mineral Kermes, and the golden Sulphur of Antimony.

Several new experiments have alſo been made by the ſame chemiſt, on the red and orange coloured hydro-ſulphurets of antimony, and he is perfectly ſatisfied that the former conſiſts of oxide of antimony and ſulphurated hydrogen; and that the latter is compoſed of the ſame principles, and a portion of ſulphur not hydrogenated. By directing a current

rent of ſulphurated hydrogen gas into nitro-muriate of antimony, kermes immediately falls down, and afterwards when there is a ſufficient quantity of acid at liberty to diſhydrogenate the ſulphur, golden ſulphur precipitates. The ſame gas converts all the antimony contained in the acetite, and in the tartrite of this metal into kermes, which proceſs affords the means of procuring this preparation of a nature perfectly uniform.[*]

Elaſtic Reſin in Opium.

M. Bucholz, a friend of Trommsdorf, has lately addreſſed a very curious memoir to him upon opium, which he means to inſert in his next Journal. He found, in particular, that opium contains a great quantity of elaſtic gum.

Diſcovery of a New Earth in the Beryl of Georgen-Stadt.

During the laſt ten years a mineral has been found in the mines near Georgen-Stadt, to which, on account of its reſemblance to the beryl, the name of that foſſil had been given. Trommsdorff undertook to analyze this ſubſtance in the hopes of finding the glucine, but inſtead of that earth he found a new one, or one which was different from thoſe hitherto known.

1ſt. In its pure ſtate it reſembles alumine. 2. It is not more ſoluble either in the dry or humid way in the cauſtic alcalis, than in their carbonates. 3. Ammoniac, whether cauſtic or carbonated, exerciſes no action upon it. 4. It retains the carbonic acid but weakly. 5. It acquires hardneſs, but not ſapidity by fire. 6. It is not ſoluble in water. 7. It readily unites to acids with which it forms ſalts, which have little or no taſte. 8. The earth, hardened by fire, diſſolves in the acids with the ſame eaſe as that which has not been ſo treated. 9. It forms with ſulphuric acid a ſalt little ſoluble, perfectly inſipid, which when it is acidulated diſſolves without difficulty, and becomes cryſtallized in ſtars. 10. Superſaturated with phoſphoric acid it affords a very ſoluble ſalt. 11. Its acetite is of very ſparing ſolubility. Its other characteriſtics, as well as his proceſs of analyſis, will be detailed in the 1ſt part of the 8th Volume of his Journal. He has given the name of *Aguſtine* to this new earth, from its property of forming with acids ſalts without taſte[†].

On the Method of hearing by the Teeth.

Citizen Vidron, muſic maſter at Paris, has announced the diſcovery of a method of rendering muſic audible by perſons deaf and dumb from their birth.

Citizens Hauy, Lacépède, and Cuvier, who were appointed by the National Inſtitute to examine this diſcovery, made their report (the 21 Meſſidor) in the year 8 (July 21, 1800.)

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[*] See Annales de Chimie XXXII. 257, for an account of all the circumſtances which determine the formation of the different ſulphurets of antimony. French editor.

[†] The laſt ſeven articles are tranſlated from a letter of Trommsdorff in the Annales de Chimie XXXIV. 130.

Citizen Vidron uſes a rod of ſteel, one end of which he places upon the ſound-board of the muſical inſtrument, and the other between the teeth of the deaf perſon. He adds to it a branch terminated by a braſs button which reſts upon the cavity of the ſtomach, and ſometimes a third which he places upon the head.

The commiſſioners have found that many authors have announced the fact of the deaf being made to hear by putting their teeth in communication with the inſtrument by means of a ſtick, a goblet, or ſome other body. They have quoted, amongſt others, Fabricius D'Aquapendente, Schelhammer, Boerhaave, Winkla, and Joriſſen.

They have alſo found that in like circumſtances ſteel is better for the purpoſe than wood, which had been almoſt generally uſed before the time of citizen Vidron; but that his two branches are of no uſe with reſpect to what may properly be called hearing.

They particularly endeavoured to determine to what extent this method might be uſeful; as well with reſpect to the different kinds of deafneſs, as to the different kinds of ſounds which are propoſed to be rendered audible.

They produced an artificial deafneſs in themſelves by ſtopping their ears, and retiring to a diſtance. In both caſes they heard diſtinctly by means of the ſteel rod, and the ſounds appeared to them to proceed from within this rod, and not from their true place.

But the perſons, really deaf, whom they have examined preſented very different reſults. Some of them manifeſtly heard; but the greater number declared they only experienced a tremulous action more or leſs general.

The commiſſioners conclude, that this method may be ſerviceable in deafneſs which only proceeds from obſtructions in the external paſſage, but that it is uſeleſs in that which is cauſed by a paralyſis of the nerve, or an eſſential derangement in the interior part; which is the moſt common kind of deafneſs, eſpecially in thoſe who are deaf from their birth. They think it proper, nevertheleſs, to make the experiment on all young perſons who are deaf, ſince if only one out of an hundred ſhould derive benefit from it, to that one at leaſt it would be a ſource of enjoyment.

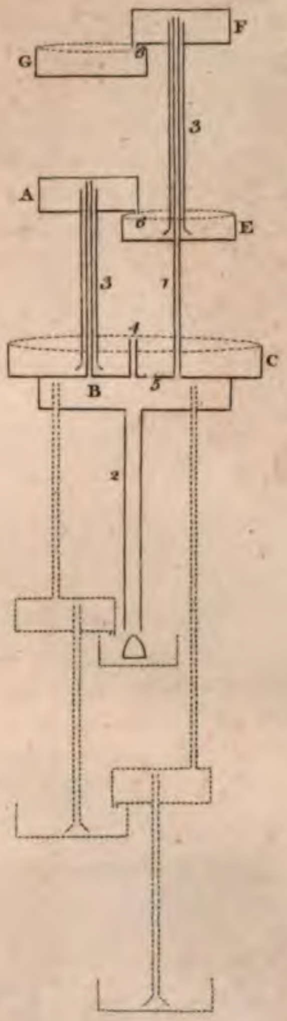
As to articulated ſounds or ſpeech the commiſſioners found that it was almoſt impoſſible to hope for an exact tranſmiſſion of them by this method, at leaſt in its preſent ſtate.

Societé Philomath. No. 41.

⁂ Mr. Leſlie has favoured me with a correction of his paper, where page 346, line 19, for *public opinion* the words *popular opinion* are to be ſubſtituted. And he fears that other verbal inaccuracies may have occurred, as the copy was written in haſte, and the unexpected abſence of the author, during the impreſſion, prevented any ſubſequent reviſion.

Philos. Journal. Vol. IV. Pl. XV. facing p. 384.

Fig. 1.



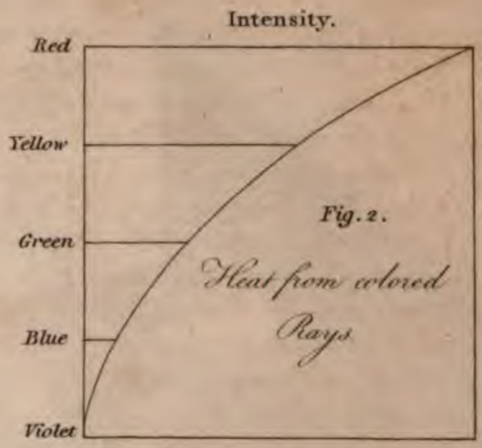
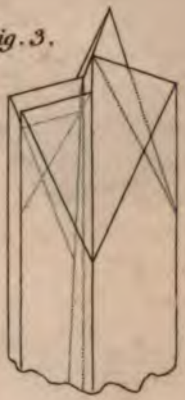


Fig. 2.

Heat from colored Rays

Fig. 3.



Arragonite

Fig. 4.

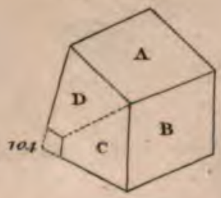
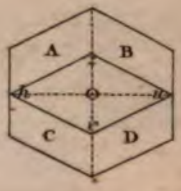


Fig. 5.



Mutlow Sculp.

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ASTOR, LENOX AND
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Radiation of heat.

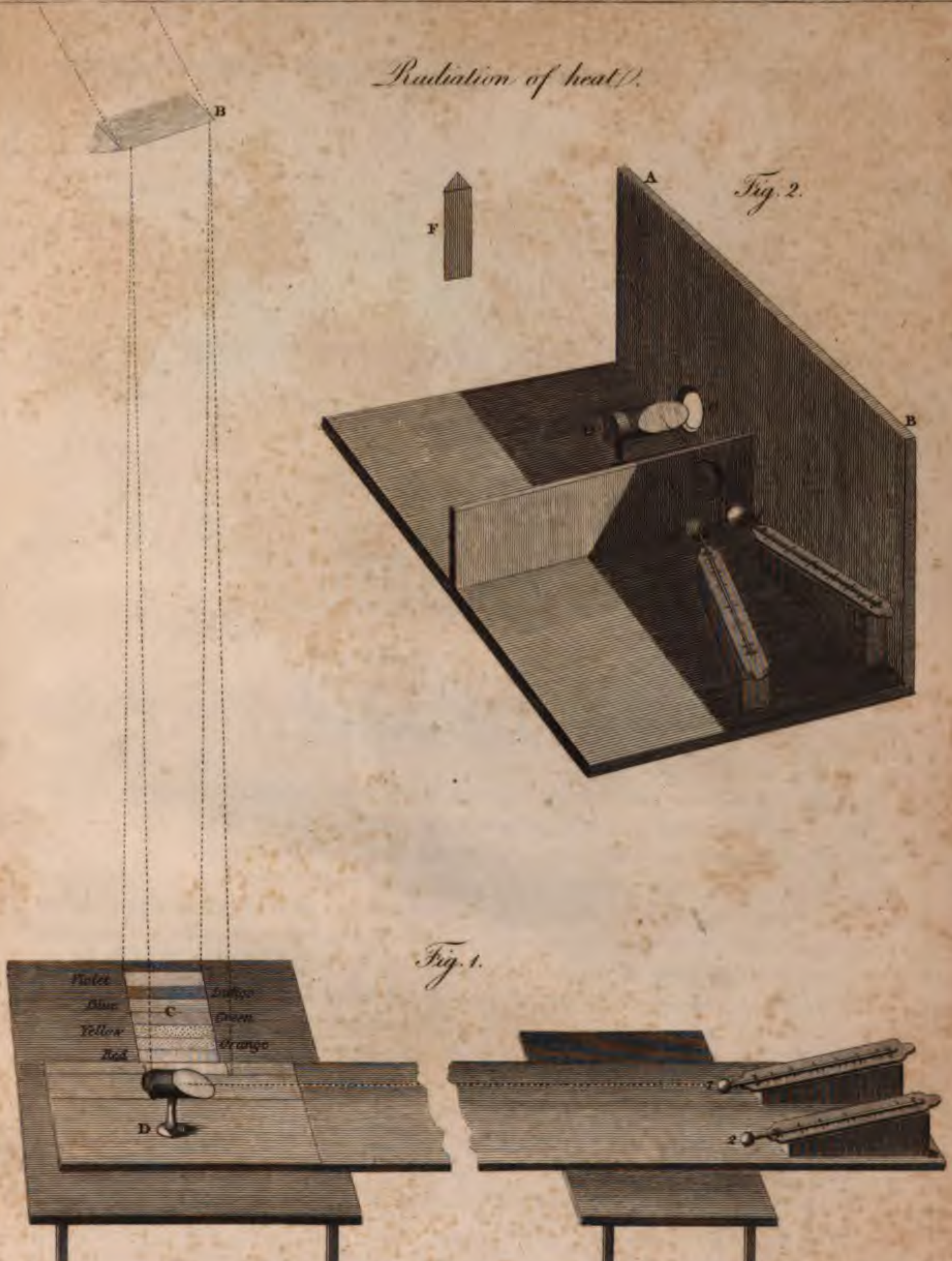

Fig. 2.

Fig. 1.

Mutlow Sculp.

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ASTOR, LENOX AND
TILDEN FOUNDATIONS

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER 1800.

ARTICLE I.

On the Number of the Primitive Colorific Rays in Solar Light. By the Rev. MATTHEW YOUNG, *D. D. S. F. T. C. D. & M. R. I. A.*

THE opinion that there are but three primitive colours has been maintained by M. du Fay, and after him by Father Caſtelli. See Montucla, Vol. I. p. 630.; but they and all others who hold the ſame doctrine, defend it merely on the principles of a painter, who ſhews how with theſe three colours on his pallet, he can compound all others; for with red and yellow he can form an orange colour; with blue and yellow he forms green; and with blue and red he forms indigo and violet; and thus having compounded the ſeven priſmatic colours, it is manifeſt that all other colours, with their different gradations, can be formed from them likewiſe. But this *pharmaceutical* argument is by no means ſufficient to ſatisfy us as to the real compoſition of ſolar light.

" Light, in refracting, is decompoſed into ſeven rays, red, orange, yellow, green, " blue, indigo and violet. It has been ſuppoſed," ſays Fourcroy, " that three of theſe " colours, the red, yellow, and blue, were ſimple; and that the other four were formed " each of its two neighbours; that is, the orange from the red and yellow, the green from " the yellow and blue, the indigo from the blue and violet, and the violet from the red " and indigo. But this ſuppoſition has never been proved." See his Philoſophy of Chem.

chap. 1. § 3. Beſides that this is a mere hypotheſis, unſupported by any fact, as Fourcroy obſerves, we remark, that it is in itſelf inadequate; 1ſt, becauſe in the ſolar ſpectrum, the red and indigo are not *neighbouring* colours, but are almoſt at the greateſt poſſible diſtance from each other. 2dly, According to this hypotheſis, indigo is compoſed of blue and violet; but violet is compoſed of red and indigo; indigo therefore is compoſed of red, blue and indigo, that is, indigo itſelf is one of its own eſſential ingredients, which is abſurd.

The experiments of the priſm ſeem to eſtabliſh, in a very clear manner, the exiſtence of ſeven original and uncompounded colours; and though green, for inſtance, may be compounded of blue and yellow, yet it does not directly follow from thence, that it always is ſo actually compounded. Accordingly Newton tells us, that green may be exhibited in two different ways, either by primitive, green-making rays, which are ſimple and not reſolvable by any reflection or refraction into different rays; or by a compoſition of blue and yellow rays, which are differently refrangible, and which therefore after their union, may again be ſeparated by refraction, and exhibit their proper colours of blue and yellow.

On this doctrine of the two-fold generation of green, we may in the firſt place remark, that the ancient, received axiom "*Deus nil agit fuſtra*" ought not to be too haſtily abandoned, as it muſt appear to be, if this doctrine be maintained: for if green may be produced by blue and yellow, then blue and yellow being already exiſtent, green is a conſequence; and therefore peculiar rays formed for the production of green are ſuperfluous. Though I acknowledge, that this maxim is not ſo cogent or ſelf-evident, as to preclude all objection, yet ſince the general obſervation of nature ſeems to ſhew, that this waſte of power or multiplicity of means is not adopted by the Supreme Artiſt, it certainly ſeems juſtly entitled to our attention, at leaſt ſo far as this, that we ſhould be careful in ſhewing, that we are led to theſe different cauſes of the ſame effect, by a legitimate and cautious analyſis.

In defence of the doctrine of three primitive colours only, F. Caſtelli contents himſelf with ſaying, that the colours of the priſm are immaterial, accidental, artificial, and therefore unworthy the regard of a philoſopher; whereas the colours of painters are ſubſtantial, natural, palpable. From them, of conſequence, the theory of chromatics ſhould be deduced; but they tell us, that there are but three parent colours, which give birth to all others.

In reply to this we need only obſerve, that Sir I. Newton has proved, that the colours of natural bodies depend on the colorific qualities of the rays of light; and therefore that our theory of colours muſt be derived from an enquiry into the conſtitution of ſolar light, for according to that conſtitution the colours of bodies will vary: and he farther ſhews, that if ſolar light conſiſted of but one ſort of rays, all bodies in the world would be of the ſame colour. However true therefore F. Caſtelli's theory may be, the manner in which he deduces it from phænomena is unqueſtionably falſe.

I ſhall therefore proceed to enquire ſcrupulouſly into the compoſition of the ſolar ſpectrum, from which, without doubt, the true doctrine of the origin of colours is to be derived.

If the ſolar light conſiſted of ſeven primitive, homogeneal coloured rays, and that theſe homogeneal rays were equally refrangible, the ſpectrum would conſiſt of ſeven circles of different colours, ſince the homogeneal rays of each colour would paint a circular image of the ſun. But it is manifeſt, that ſeven circles could not compoſe an oblong ſpectrum, with rectilineal ſides. Therefore the rays of the ſame denomination of colour muſt be differently refrangible. Which is alſo made ſtill farther evident by obſervation of the ſpectrum, ſince in it we perceive, that the priſmatic colours are diffuſed over ſpaces, which are, on the ſides, terminated by right lines, and therefore the centers of the circles of the ſame denomination of colour are diffuſed over lines equal to theſe ſegments of the rectilineal ſides of the ſpectrum. Newton has ſhewn, prop. 4. B. I. Optics, how to ſeparate from one another the heterogeneous rays of compound light, by diminiſhing the breadth of the ſpectrum, its length remaining unchanged; and when the length of the ſpectrum is to its breadth, as 72 to 1, the light of the image is ſeventy-one times leſs compound than the ſun's direct light. In the middle of a black paper he made a round hole, about a fifth or a ſixth part of an inch in diameter, upon which he cauſed this ſpectrum ſo to fall, that ſome part of the light might paſs through the whole of the paper; this tranſmitted part of the light he refracted with a priſm placed behind the paper, and letting the light fall perpendicularly upon a white paper, he found that the ſpectrum formed by it was perfectly circular. Hence, therefore, it follows, that the equally refrangible rays occupy a ſpace on the rectilineal ſides of the ſpectrum equal at leaſt to the fifth or ſixth part of an inch; that is, the rays of the ſame colour are differently refrangible.

The different quantity of the homogeneous rays of different colours will not account for the different ſpaces they occupy in the ſpectrum; for this difference in quantity would affect only the intenſity of the colour, not the magnitude of the ſpace which it would occupy. All the red light therefore is not homogeneous; but conſiſts of rays of innumerable, different degrees of refrangibility; and ſo of the other colours.

Now ſince the rays which are of the ſame denomination of colour are differently refrangible, they will either form oblong ſpectrums detached from each other; or they will in part lap over, and fall on each other. The former poſition is manifeſtly falſe: therefore the original priſmatic colours will partly lap over and fall on each other, and therefore neceſſarily generate the intermediate colours. And ſo Sir I. Newton obſerves, where he ſays, that the original, priſmatic colours will not be diſturbed by the intermixture of the conterminous rays, which are intermixed together. This overlapping however, which Newton ſpeaks of, ariſes only from the ſun's having a ſenſible diameter, and does not neceſſarily imply an equal refrangibility in any differently coloured rays. If there be but three original priſmatic colours, red, yellow, and blue, and that the red and yellow lap over, ſo as that there ſhall be a certain ſpace in the ſides of the ſpectrum equally occupied by yellow and red circles, then will theſe circles by their intermixture compound an orange colour; and this colour as to refrangibility will be homogeneous, becauſe the coincident rays of different colours are equally refrangible. In like manner green may be compounded by the mixture of blue and yellow circles, equally refrangible. Now this is ſimple, and conformable to the other phænomena of the ſpectrum; for if rays of the ſame denomination of colour be differently refrangible, it is not unreaſonable to ſuppoſe, that rays of a different denomination of colour may be equally refrangible; and therefore ſince the red rays are unequally refrangible, and likewiſe the yellow, there is nothing incongruous in ſuppoſing that ſome of the leſs refrangible of the yellow may be equally refrangible with ſome of the more refrangible of the red; and if ſo, they will conſequently be intermixed with them: and the ſame may be ſaid of the green. This hypotheſis likewiſe receives conſiderable ſtrength from this conſideration, that the orange, green, indigo, and violet occupy thoſe places which they ought to do, in caſe there were but three primitive colours, red, yellow, and blue: thus the orange lies between the red and yellow, becauſe it is formed by ſome of the extreme rays of red and yellow, which are equally refrangible; in like manner the green lies between the blue and yellow, becauſe it is formed by the mixture of blue and yellow. The indigo and violet muſt alſo occupy the extreme part of the ſpectrum, where the moſt refrangible red and blue rays are united, and gradually becoming more and more dilute, fade away, and at length entirely vaniſh. But if the orange, green, indigo and violet be primitive colours, there is no apparent reaſon why they ſhould have had ſuch degrees of refrangibility aſſigned them, as that they ſhould occupy the places they do, rather than any other.

Moreover, if theſe three colours red, yellow, and blue be the primitive colours, they cannot themſelves be generated; and accordingly we find, that yellow cannot be generated by the mixture of the adjacent priſmatic colours, orange and green; and the reaſon of this is evident, becauſe orange is compounded of red and yellow; and green is compounded of yellow and blue; but red and blue compoſe purple; which added to the yellow will generate a new compound colour, viz. a ſickly green, differing manifeſtly from yellow, the colour which ought to reſult according to the analogy of the other primitive colours, in which the extremes, by their mixture, generate that which is intermediate. In the ſame manner, blue cannot be generated by the mixture of green and indigo, becauſe green is compoſed of yellow and blue, and indigo of blue and violet; therefore the reſulting colour is compoſed of blue, yellow, and violet; but yellow and violet do not compoſe blue, therefore neither will blue, yellow, and violet compoſe a blue colour. Now if orange and green be primitive colours, in the ſame manner as red, yellow, and blue, we can aſſign no reaſon why blue ſhould not be generated by the mixture of the adjacent colours, as well as green and orange. But it is a received principle, that an hypotheſis ſhould ſolve all the phænomena; of the two hypotheſes therefore, namely, that there are ſeven primitive colours, differently refrangible; or that there are but three, ſome of which, of each ſpecies, are equally refrangible; the latter alone ſolves all the phænomena of the ſolar ſpectrum, and therefore is to be preferred.

If it be ſaid, that thoſe rays which are equally refrangible muſt excite the ſame ſenſation on the retina, becauſe they muſt have the ſame momentum; it is replied, 1ſt, That it has not yet been proved, that the ſenſation of different colours depends on the different momentum of the rays. 2dly, The rays may have different momentums, and yet be equally refrangible; for ſince refraction is ſuppoſed to depend on the attractive force of the denſer medium, we muſt ſuppoſe it analogous to the attractive force of gravity, which is proportional to the quantity of matter; and therefore the greater or leſs quantity of matter in a particle of light would produce no alteration in its refraction. Neither can the different refrangibility depend on the different velocity of the rays; becauſe the difference of refrangibility of the red and violet rays is much greater in flint glaſs than in crown glaſs; and this would require a proportionably greater difference in the original velocities, which cannot be. And this ſame argument holds equally againſt the former hypotheſis, that the difference of refrangibility depends on the different magnitude or denſity of the particles of light. 3dly, Refraction ſeems to ariſe from a ſpecies of elective attraction, ſince different mediums which act on the mean rays equally, act on the extreme rays unequally: hence rays of the ſame quantity of matter and velocity, and therefore of the ſame momentum, may be diverſely refracted; and rays of different momentums equally refracted.

Nor is it to be wondered at that the rays of light ſhould be differently refrangible, independent of any regard to their momentum, when we conſider, that the different coloured rays appear to be combined with combuſtible bodies, with different degrees of attractive force. For in combuſtion we find, that different bodies are diſpoſed to part with different rays with greater facility; but when the combuſtion is ſufficiently rapid, they part with all the different coloured rays together, and the flame is therefore white; and this is what is called a white heat. Dr. Fordyce in the Phil. Tranſ. for 1776, tells us, that when the heated ſubſtances are colourleſs, they firſt emit a red light; then a red mixed with yellow, and laſtly, with a great degree of heat, a pure white. All this is wonderfully conformable to the refraction of light by tranſparent ſubſtances, which refract, and therefore attract the red light leſs, and conſequently in combuſtion part with it more eaſily. On the other hand I know it is generally believed, that the light in combuſtion proceeds from the air; but this circumſtance of the different colour of the light in different caſes, ſeems to overturn this opinion; for if vital air were oxygen diſſolved in caloric and light, then the oxygen being abſorbed by the burning body, the light extricated would in all caſes be of the ſame nature; the greater or leſs rapidity of the combuſtion would only produce an extrication of a greater or leſs quantity of light, but could not produce any variation in its nature, it being neceſſarily the ſame in all caſes, to wit, that in which vital air is diſſolved. But the truth or falſhood of this reaſoning will not affect the validity of the poſition, that the refrangibility of the rays of light cannot depend on the different magnitude, denſity, or velocity of the particles.

But though ſpeculation ſeems thus to render it probable, that there are but three parent colours; our theory muſt ever remain unſatisfactory, unleſs it receives the ſanction of direct experiment. In this however there is no ſmall difficulty; for ſince the rays of light which compoſe any given individual point of the colours of orange, green, violet, and indigo are equally refrangible, they will be alſo equally reflexible; and therefore cannot be ſeparated either by refraction or reflection, ſo as to exhibit the different coloured rays of which they are compoſed. It ſeems therefore, that the only way remaining, by which we can experimentally aſcertain the compoſition of theſe colours, if they be indeed compound, is tranſmiſſion. For ſince tranſparent coloured bodies are ſuch merely by their letting paſs through them either ſolely, or more copiouſly, rays of a certain colour, and intercepting all others, ſuch tranſparent bodies, applied to compound colours, will aſcertain that compoſition, by extinguiſhing, in a great meaſure, all rays except ſuch as are ſo adapted to its conformation, as to paſs through it, and give it its peculiar denomination of colour.

In order to try the truth of the hypotheſis of ſeven colours by this teſt, I looked through a blue glaſs at the red end of the ſpectrum: now we are to conſider, that if that part of the ſpectrum was compoſed of red rays, and none other, the only effect of the blue glaſs would either be a total or partial ſuffocation of the red rays; and therefore that part of the ſpectrum, when looked at through the glaſs, would either totally diſappear, or become a faint and diluted red. But, on experiment it appeared of a purple colour. The purple in this caſe could not be a primitive and original colour, as is manifeſt, becauſe it did not proceed from the purple part of the ſpectrum; we muſt therefore conclude, that it was a compound colour. But purple, when compound, is made up of blue and red, therefore it follows, that ſome blue rays did actually exiſt in the red part of the ſpectrum: which combined with the few, ſtraggling red rays which penetrated the blue glaſs, compoſed that purple colour, which the red extremity of the ſpectrum aſſumed, when viewed by the light tranſmitted through the blue medium.

To try, on the other hand, whether any red rays lay hid amongſt the blue, I proceeded in the ſame manner, and looking at the blueſt part of the ſpectrum through a red glaſs, it appeared of a purple colour: ſome red rays therefore are equally refrangible with the blue; and if the red extends as far as the blue, there is no reaſon why we may not ſuppoſe that it extends ſomewhat farther, ſo as to compound, with a diluted blue, the extreme colours of the ſpectrum, indigo and violet.

But it may be ſaid, that if blue rays exiſted amongſt the red, that part of the ſpectrum could not appear ſo extremely brilliant as it really does; but would put on a purpliſh appearance in the ſpectrum itſelf, even to the naked eye. In anſwer to this objection we may obſerve, that the moſt intenſe and vivid, natural red bodies do, in fact, reflect a very great proportion of blue rays, becauſe they appear of a ſtrong blue colour when placed in the blue part of the ſpectrum; and therefore they reflect juſt as many when the direct, white ſolar light falls on them, in which all that blue is involved; though by the predominance of the red rays, they appear of that colour, without any viſible tincture of blue.

In order to determine whether the purple appearance of the red extremity of the ſpectrum, when viewed through a blue glaſs, was cauſed by any of the white ſolar light, which might perhaps be reflected from the air, or ſurrounding objects to the ſpectrum, and thus throw on that part ſuch a quantity of blue as might produce a ſenſible effect; I cauſed the middle and moſt intenſe part of the red to paſs through a hole in a blackened paper, and then fall on an optical ſcreen; by which I was ſure that I had as pure and uncompounded a red as could be deſired; which alſo underwent the uſual teſt of purity by ſubſequent refraction, without any change in the form of the ſpectrum; I then looked at the body which was illuminated with this red, through the ſame blue glaſs, and the effect was the ſame as before.

To try this doctrine of three parent colours ſtill farther, I conſidered, that if the orange were really compounded of the red and yellow rays, then by looking at the orange through a red glaſs, the orange would in a great meaſure vaniſh, and the red would appear to extend much farther than in the original ſpectrum; becauſe the yellow rays being conſiderably obſtructed, the red would become more predominant; and that part of the ſpectrum, which before appeared orange, in conſequence of a certain mixture of yellow and red, would now, by the failure of ſo conſiderable a part of the yellow, loſe its orange appearance, and put on that of red: and, on experiment, I found the caſe to be ſo really in fact; for while an aſſiſtant looked at the ſpectrum through the red glaſs, I moved an obſtacle from the red towards the other end of the ſpectrum, deſiring him to ſtop me, when the obſtacle ſhould arrive at the confines of red and orange; but when he did ſo, the obſtacle had attained the middle of the orange, or rather had paſſed beyond it. Now if the orange were really a primitive colour, I ſhould ſuppoſe, that when looked at through the red glaſs, it would either appear diluted, without any change of dimenſions; or that if the weak part of the orange, next the red, ſhould vaniſh, by the obſtruction of the glaſs, a dark interval would appear between the orange and the red; in neither caſe can we account for the apparent extenſion of the red into the region of the orange; nor by any other hypotheſis, as appears to me, than that ſome of the red rays are equally refrangible with ſome of the orange.

There is another argument derived from the ocular ſpectra of Dr. Darwin, which ſtill further corroborates the doctrine of three primogenial colours. Place a piece of coloured ſilk, about an inch in diameter, on a ſheet of white paper, about half a yard from your eyes; look ſteadily upon it for a minute; then remove your eyes upon another part of the white paper, and a ſpectrum will be ſeen of the form of the ſilk thus inſpected, but of a different colour, thus

Red ſilk produced a green ſpectrum,
Green - red,
Orange - blue,
Blue - orange,
Yellow - violet,
Violet - yellow.

The reaſon of theſe phænomena is very ingeniouſly aſſigned by Dr. Darwin; he ſays, that the retina being excited into a violent and long continued action by the red rays, in the firſt experiment, at length is ſo fatigued as to become inſenſible to them; but that it ſtill remains ſenſible, that is, liable to be excited into action by any other colours at the ſame time; and therefore the ſpectrum aſſumes a green appearance, becauſe if all the red rays be taken out of the ſolar light, the remaining rays will compoſe green. See Phil. Tranſ. Vol. LXXVI. Converſely, a green object produces a red ocular ſpectrum. Now we may obſerve, that if all the green rays be taken out of the ſolar ſpectrum of ſeven colours, the remaining colours will not compound red. If indeed green be not a primitive colour, but a compoſition of blue and yellow, then will the eye, in looking on a green object, be at once affected by blue and yellow rays; and therefore become inſenſible to them both; and conſequently the ſpectrum will appear red. But if green be a primitive, original colour, generated by its own peculiar green-making rays, the eye in contemplating a green object, will become inſenſible only to the green rays; and therefore the other ſix priſmatic colours, which are ſpecifically different from the green, ought to be ſenſible, and produce their proper compound effect; but this would not be the ſenſation of red. In like manner, if the object be yellow, the eye will at length become inſenſible to the yellow-making rays, and the ſpectrum will be violet. Now ſince on the hypotheſis of ſeven original colours, the orange and green are primitive, though the eye be rendered inſenſible to the yellow rays, it will not be ſo to the orange and green, which therefore, together with the red, blue, violet and indigo, will produce their compound effect; but the colour reſulting from this joint action is not violet, which nevertheleſs is the colour of the ocular ſpectrum. On the other hand, if there be but three primitive colours, red, yellow, and blue, when the eye is inſenſible to the yellow-making rays, the ſpectrum muſt neceſſarily be violet, which is the colour that reſults from the mixture of red and blue. If it be objected, that the eye is not only inſenſible to the unmixed yellow rays, but likewiſe to the yellow of the orange and the green, then it is admitted that orange and green are compound colours. Beſides, ſince the colour which would reſult from the mixture of red, orange, green, blue, indigo and violet is not yellow, the eye ought not to be inſenſible to this colour; and conſequently, ſince by the exemption of the yellow rays from the white ſolar light, that colour does not reſult, but a diſtinct purple, it follows, that the orange and green are not primitive colours inherent in ſolar light.

It remains now only for us to ſhew, that the three colours of red, yellow, and blue, are adequate to the ſolution of all the phænomena of chromatics. But in order to ſhew this, few words will be ſufficient, for having ſeen, that the ſeven priſmatic colours can be generated by theſe three, it follows that all others can be generated from them, as Sir I. Newton has proved at large. However, I think it will not be ſuperfluous to obſerve, that white may be directly produced by theſe three colours, without the previous generation of the other four priſmatic colours, in the ſame manner as it is uſually generated with ſeven. " I could never yet," ſays Newton, " by mixing only two primary colours, produce a per" fect white. Whether it may be compoſed of a mixture of three, taken at equal diſtances " in the circumference, I do not know." Now to ſhew that white may be thus generated, let an annulus of about four inches diameter be divided into three parts by lines tending towards the centre, and let theſe three diviſions be reſpectively painted red, yellow, and blue, in proportions to be aſcertained by trial; then if the annulus be turned ſwiftly round its centre, it will appear white. That white may be generated by the mixture of only the three colours red, yellow, and blue, might alſo appear from the rule which Newton himſelf has given us, for determining the colour of the compound which reſults from the mixture of any primary colours, the quantity and quality of each being given.

The rule is this, the circumference of a circle is diſtinguiſhed into ſeven arches proportional to the ſeven muſical intervals in an octave, that is, proportional to the numbers 45, 27, 48, 60, 60, 40, 80: the firſt part is to repreſent a red colour, the ſecond orange, the third yellow, the fourth green, the fifth blue, the ſixth indigo, and the ſeventh violet. Theſe are to be conſidered to be all the colours of uncompounded light gradually paſſing into one another, as they do when made by priſms, the circumference repreſenting the whole ſeries of colours from one end of the ſun's coloured image to the other. Round the centers of gravity of theſe arches let circles proportional to the number of rays of each colour in the given mixture be deſcribed. Find the common centre of gravity of all theſe circles, and if this common centre of gravity coincide with the centre of the circle, Newton ſays that the compound will be white. Join therefore the centers of gravity of the blue and yellow circles, and from the centre of the red circle draw a right line through the centre of the principal circle; from the conſtruction it will cut the line which joins the centers of the blue and yellow circles; if therefore the number of the blue and yellow rays be to each other inverſely as their diſtances from the point where the line which joins their centers is cut by the line drawn from the centre of the red circle; and if the number of red rays be to the ſum of the yellow and blue rays inverſely as the diſtances of the centre of the red circle, and the common centre of the yellow and blue from the centre of the principal circle, the common centre of gravity of the red, blue and yellow circles will coincide with the centre of the principal circle, and therefore the reſulting compound will be white.

But it is manifeſt that this conſtruction cannot be relied on, becauſe the quantities of the rays of any given colour in ſolar light, do not appear to be proportional to the ſpaces which they occupy in the rectilineal ſides of the ſpectrum. Thus it is known that the yellow making rays are predominant in ſolar light, yet the ſpace they occupy in the ſpectrum is to the ſpace occupied either by green or blue as four to five, and to the ſpace occupied by the violet only as three to five:

II.

An Account of ſome Additional Experiments and Obſervations on the Galvanic Phenomena. By Mr. DAVY, *Superintendant of the Pneumatic Inſtitution. Communicated by the Author.*

I.

Additional Experiments on the Cauſes of the Galvanic Phenomena.

1. SULPHURIC acid, when highly concentrated, is poſſeſſed of but little power of action upon zinc, though when diluted it diſſolves it with the greateſt rapidity. Aſſuming then the truth of the principles advanced in my laſt paper, namely, that the powers of the pile of Volta are primarily excited by the oxidation of the zinc, it follows, that diluted ſulphuric acid, when made the medium of connection between the pairs of plates, ought to produce much greater effects than concentrated ſulphuric acid.

This I have found is actually the caſe. When the cells of a ſeries of twenty pairs of plates of ſilver and zinc, conſtructed with waxen cement, in the mode deſcribed in 8 of my laſt paper, were filled with ſulphuric acid, nearly of ſpecific gravity 1.9, no galvanic action, except the production of a ſlight cauſtic taſte, was perceptible by the uſual methods of trial; though when diluted ſulphuric acid was uſed the ends of the ſeries gave ſhocks to the moiſtened fingers, and wires connected with them effected the uſual changes in water.

That concentrated ſulphuric acid is not of that order of more perfect galvanic conductors which, when interpoſed between the plates, deſtroy their electrical effects, is evident from the following experiment: the cells of ten pairs of plates of copper and zinc, conſtructed with waxen cement, were filled with concentrated ſulphuric acid; but not the ſlighteſt galvanic power was produced. A ſmall drop of water was then poured upon the acid in each of the cells. The action of the ſeries was immediately ſhewn by its producing the uſual appearances on wires in water.

2. The galvanic conducting powers of liquid ſulphurets are at leaſt equal to thoſe of water. I found that when the fingers were plunged into glaſſes, containing ſolution of ſulphuret of ſtrontian connected with the ends of a pile, the ſhock was full as ſenſible as if the communication had been made through water. When the galvanic current was made to act on ſolution of ſulphuret of ſtrontian by means of ſilver wires, the zinc wire became blackened, and gas was given out round the ſilver wire. But ſolutions of ſulphurets are incapable of giving oxygen to zinc; they, conſequently, ought to produce no galvanic effects when made the media of connexion of the double plates in the pile of Volta. Twenty-five pairs of ſilver and zinc, erected with cloths moiſtened in ſolution of ſul-

phuret of ſtrontian, produced no ſenſible galvanic action, though the moment the ſides of the pile were moiſtened with a little nitrous acid, the ends gave ſhocks as powerful as thoſe of a ſimilar common pile.

3. No phenomenon is more conſtant than the ceſſation of the action of the common galvanic pile in a vacuum * when the gage is below one-tenth. Suppoſing the expulſion of atmoſpheric air from the water preventing it from oxidating the zinc, the ſole cauſe of this ceſſation, it follows, that a pile ought to act in vacuo when nitrous acid, or diluted ſulphuric acid, is the medium of connexion between the plates. Into each of the cells of a ſeries of twelve pairs of ſilver and zinc plates, which had juſt been moiſtened with a little water, a large drop of nitric acid was introduced: when the wires connected with the ends immediately began to produce the uſual appearances in water. The ſeries was introduced under the receiver of an air pump, and the ſilver wire from its zinc end connected with a veſſel of water that had been long boiled. The wire from its ſilver end was ſo faſtened by reſinous cement, to a ſliding braſs wire paſſing through the top of the receiver, that it could be plunged at pleaſure into the water when the vacuum was made. The receiver was exhauſted till the gage ſtood at $\frac{6}{10}$ of an inch, when the communication was effected. The zinc wire immediately began to oxidate, and gas was given out round the ſilver wire. The proceſs went on for many minutes, and when it had ceaſed, was not ſenſibly revived by the admiſſion of the atmoſphere. In another experiment the ſame phenomena were obſerved. Gas appeared to be given out more rapidly from the ſilver wire than in the atmoſphere; but this was from the diminution of preſſure. The oxidation was certainly leſs: which may be eaſily accounted for, when we conſider, that no nitrous acid could be recompoſed in vacuo, as in the atmoſphere from the nitrous gas diſengaged between the plates, and that, in conſequence of the diminiſhed preſſure, ſome of the acid muſt probably have aſſumed the aëriform ſtate.

A drop of ſulphuric acid, poured into each of the moiſtened cells of twelve pairs of plates, enabled the wires from the ends to effect the uſual changes in pure water for rather more than half an hour in vacuo, the gage being at $\frac{6}{10}$. The oxidation went on nearly as vividly as in the atmoſphere, and what is rather remarkable, ſome gas was given out from the oxidating wire, though very little was produced from the ſilver wire.

4. The reſults of the laſt experiment are intereſting not only from their coincidence with the other facts, but likewiſe becauſe they afford proofs that the preſence of oxygen in that looſely combined or peculiar ſtate in which, when abſorbed by combuſtible bodies, it produces inflammation, and in which, in my infant chemical ſpeculations, I ſuppoſed it to be combined with the matter of light, is not eſſential to the galvanic effects. Whether water is abſolutely eſſential, we ſhall find ſome difficulty in determining, as it exiſts in larger or ſmaller quantities in all the non-metallic fluid galvanic conductors that have been yet experimented upon. The following fact is in favour of

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* See Colonel Haldane, Phil. Journal, No. 43, Vol. IV. and my laſt paper, No. 44.

its eſſentiality. The compound of concentrated ſulphuric acid and oxigenated muriatic acid * (which may be formed by introducing oxigenated muriate of pot-aſh into ſulphuric acid, or by paſſing oxigenated muriatic acid gas through it), ſlowly oxidates both zinc and ſilver, the oxigenated marine acid being decompoſed. I expected that it would produce ſtrong galvanic effects when made the communicating medium of the cells of a pile; but in this I was diſappointed; a ſeries of twenty pairs connected by it produced hardly any ſenſible action.

5. If any perſon wiſhes to repeat the experiments in vacuo juſt detailed, great caution muſt be obſerved with regard to the quantities of acids introduced into the cells. Two or three drops in each will be ſufficient, particularly if it has been previouſly a little moiſtened. When larger portions are employed, the effervescence highly increaſed by the removal of atmoſpheric preſſure will be often ſufficient to moiſten the edges of the cells, and to make a communication betweem them. In conſequence of the uſe of too much acid, I have made many unſucceſsful experiments.

The water uſed in vacuo for connecting the wires ſhould be always previouſly deprived of looſe air by long ebullition, or otherwiſe the diſengagement of that ſubſtance from it will much diſturb the reſults.

II.

Obſervations gained from minute Inſpection of Galvanic Proceſſes.

1. Whenever the galvanic circuit, paſſing through the pile with wires, is broken by means of water, oxygen is uniformly produced at the zinc metallic point, and hydrogen at the ſilver metallic point. This is ſhewn from many experiments in Mr. Nicholſon's Philoſophical Journal. Conſidering analogies, an intereſting queſtion occurs. Do not the ſame phenomena take place in every part of the ſeries? *i. e.* is not oxygen fixed on every plate of zinc, and hydrogen produced on every plate of ſilver, at the points of their contact with the water of the cloths? With the hopes of gaining a ſolution of this queſtion, I conſtructed a ſeries of twenty glaſſes with ſpring water, containing plates of ſilver and zinc connected by braſs wire, in the mode pointed out by Volta. This ſeries gave feeble ſhocks, and a ſilver wire connected with it produced the uſual appearances in water. Oxygen was fixed upon that part of the wire in the glaſs containing the laſt ſilver plate, and hydrogen was liberated from that part of it in the glaſs containing the laſt zinc plate.

The ſeries was made analogous in all its parts, the end glaſſes being connected by a pair of plates, ſo that every glaſs contained a ſilver plate and a zinc plate. On minutely inſpecting the glaſſes immediately after, no particular phenomena could be perceived. But after occaſional attention to the proceſs for many hours, I obſerved the zinc plates beginning to oxidate in many of their points, though no gas was produced upon them. No gas had formed upon the ſilver plates, but the ſurface of the water in contact with them became covered with an opaque white pellicle.

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* I accidentally diſcovered this combination in July, 1799. Some of its properties are very peculiar. More intereſting enquiries have hitherto prevented me from minutely examining them.

2. The ſilver plates uſed in this experiment were not perfectly poliſhed. This might have influenced the reſults: and ſome gas might have eſcaped my obſervation. That the operation might go on in cloſed veſſels, I cut off the bottoms of ſome bottles with a file, ſo that they could be eaſily joined again by cement. Into one of theſe bottles I introduced a plate of poliſhed zinc *, and into another a plate of poliſhed ſilver. The plates were connected by a wire attached to their upper angles, which protruded into the atmoſphere through orifices made at the places of junction of the bottoms of the phials with their ſides; theſe places of junction being rendered perfectly water-tight by reſinous cement. Four apparatuſſes of this kind were conſtructed. They were filled with pump water, inverted in the galvanic order in glaſſes containing that fluid, and made part of a connected ſeries of twenty glaſſes.

After more than twelve hours, the zinc plates had become tarniſhed, but had given out no gas. In two of the bottles with the ſilver plates, globules of gas, too ſmall to be analyſed, had collected: theſe plates examined in the atmoſphere, as well as all the other ſilver plates, were covered in ſome points with a film of white ſubſtance, which was ſoluble with ſlight effervеſcence, and without producing cloudineſs in muriatic acid.

3. Unable to account for the non-appearance of hydrogen during the oxidation of the zinc, I could not but conclude that it was condenſed or abſorbed in ſome new compound on the ſurface of the ſilver or the zinc. Gueſſing that the quantity of ſurface might be connected with the phenomenon of its non-appearance, I ſubſtituted in three of the phials for the ſquare ſilver plates oblong ones of the ſame length, and about ,3 inches wide. Theſe had not been long connected with the ſeries before gas began to form upon them; and in five hours ſufficient was collected to be examined: from the coarſe teſt of inflammability it appeared to be hydrogen.

Thirteen pairs of a connected ſeries of twenty-five glaſſes were now compoſed of ſquare zinc plates, and oblong ſilver plates of different ſizes; ſome of them being about ,3, and others not more than ,1 inches wide. Gas was almoſt immediately given out from the greater number of the oblong plates, and in largeſt quantities from the ſmalleſt: from the ſlips of ,1 wide indeed a conſtant ſtream of globules aſcended through the water.

Small oval, circular, and ſquare plates of nearly equal ſurfaces, with the ſlips connected in the ſeries in the places of ſome of them, produced preciſely the ſame effects. In ſhort, whenever the ſurfaces of the ſilver plates did not exceed one-fourth of the quan-

* Of 1.2 inches ſquare, the ſize uſed in all the former experiments.

tity of the ſurfaces of the zinc plates, whatever were their forms, gas was always produced upon them: and both large and ſmall ſurfaces in common water in a great length of time became covered at ſome of their points of contact with that fluid with a whitiſh film.

When ſmall oblong zinc plates were introduced into any part of the ſeries inſtead of the larger plates, they appeared to oxidate rapidly, without giving out any gas.

4. The ſubſtitution of oblong ſilver ſlips for many of the plates, did not apparently much diminiſh the power of the ſeries: I therefore conſtructed a ſeries of twenty-ſeven glaſſes, wholly compoſed of zinc plates attached to thin ſilver wires. This combination with pump water gave feeble ſhocks, which were leſs vivid than thoſe of the common ſeries of eighteen. When, however, it was made analogous in all its parts, all the wires not deeply inſerted in the water gave out gas, and the zinc plates ſlowly oxidated. In another experiment, in which a ſeries of thirty glaſſes, containing wires and zinc plates, were uſed, moſt of the wires not only gave out gas, but after ſome time became covered at their points of contact with the ſurface of the water with a white film; a few of them, not deeply inſerted, produced a ſlight white precipitation.

5. Theſe facts ſeemed to ſhew that the quantity of hydrogen produced in a ſeries was in ſome meaſure, and to a certain point, in the inverſe ratio of the quantity of the ſurface of the ſilver plates. Speculating upon them, and comparing them with the experiments of Mr. Cruickſhank, and thoſe which I noticed in my laſt paper on the ſigns of ammoniac perceived during the action of a pile in common air, I could not but conjecture that whilſt oxygen was condenſed on all the zinc excitors in the ſeries, hydrogen was produced on all the ſilver ones; and in ſmall ſurfaces chiefly liberated, whilſt on larger ones it was almoſt wholly condenſed by the nitrogen of atmoſpheric air diſſolved in the water, and this conjecture was rendered more probable, when I conſidered the white matter chiefly formed round the ſilver at the ſurface of the water, and its ſolubility, without cloudineſs in acids, as it might eaſily have been produced by the decompoſition of magneſian ſalts exiſting in the pump water.

To determine whether ammoniac was produced, I made many experiments on different ſeries, conſiſting of from ſeventeen to thirty glaſſes. In ſome of theſe glaſſes wires were uſed, and in ſome of them plates. Sometimes diſtilled water was employed, and ſometimes pump water, both of which were occaſionally tinged with red cabbage juice.

Without being minute in the detail of theſe experiments, I ſhall give their general reſults. In the veſſels containing red cabbage juice, that fluid, after many hours, became tinged with green where it was in contact with the ſilver, though at its points of contact with the zinc no change of colour could be obſerved in it. In the pump water a white film always formed on the ſurface of the water near its points of contact with the ſilver: whilſt in diſtilled water ſuch an appearance was hardly perceptible. The anomaly of its being now and then perceptible I am inclined to refer to accidental impurities in the veſſels.

The ſilver ſlips in pump water almoſt always became incruſted with a white matter, which was never notable in diſtilled water, and which was ſoluble, without cloudineſs, in nitrous acid.

In one experiment, when a ſilver ſlip, forming part of a powerful ſeries, was introduced into a ſmall veſſel, containing ſolution of muriate of magneſia * (connected with the next zinc glaſs by means of muſcular fibre, to prevent the interference of the oxydating metal with the reſults), in the courſe of a night much gas was given out from it, and it became incruſted with a white matter, which diſſolved with ſlight efferveſcence in marine acid. A precipitation had taken place in the fluid.

Theſe reſults afford ſtrong probabilities in favour of the production of ammoniac on all the ſilver excitors of the ſeries formed with common water: and compared with the facts before mentioned, they amount almoſt to demonſtrations. Whether the nitrogen of atmoſperic air diſſolved in water is the agent which forms with the hydrogen ammoniac, future experiments made in vacuo muſt determine †.

6. The power of the ſeries with ſilver wires, was much leſs than that of the ſeries with plates. Suppoſing the formation of ammoniac, it was probable, that the larger quantity produced upon the plates, might be in ſome meaſure the cauſe of their greater powers; and if ſo, it was likely that the condenſation of naſcent hydrogen upon the wires would be connected with increaſe of power. From the following facts it appears that this is the caſe. A ſeries of thirteen plates of zinc, with their ſilver wires conſtructed in glaſſes, containing weak ſolutions of red ſulphate of iron, mingled with a little ſolution of common ſulphate of iron and nitrous gas, acted full as powerfully as the common ſeries of twenty plates. The wires gave out no hydrogen, but occaſioned a brown precipitation in the fluid, and the zinc plates ſoon became covered with green oxide of iron. The phænomenon was the ſame with other mixed metallic ſolutions, capable at the ſame time of abſorbing hydrogen and oxydating zinc.

7. The ſtrongeſt analogies would induce us to believe, that all the galvanic ſeries compoſed of eaſily oxydable metals and difficultly oxydable metals muſt follow the ſame laws in producing changes in their connecting fluid as zinc and ſilver. But as from the intereſting facts of Colonel Haldane, it appeared that iron and zinc as a combination were poſſeſſed of very conſiderable powers, and as iron is but little inferior to zinc in its affinity for oxygen,

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* Muriate of magneſia was uſed in preference to nitrate or ſulphate, becauſe there was a poſſibility of the decompoſition of the acids in theſe ſalts by the naſcent hydrogen, which alone would occaſion a precipitation of earth.

† Mr. Cruickſhank, who firſt noticed the probable formation of ammoniac on the ſilver wire of the pile, has offered ſome ingenious arguments to prove that nitrous acid is formed at the zinc wire. Analogy would induce us to conjecture, that if it was formed on one of the oxydating ſurfaces, it ought to be formed on all. That no change of colour takes place in cabbage juice, in contact with the oxydating zinc plates, may be owing to the great extenſion of their ſurfaces. It is worth obſerving, that the ſilver oxydates as rapidly in water in vacuo as in the atmoſphere, when the pile is in the atmoſphere; or when nitrous or ſulphuric acid is uſed in the cells in vacuo.

it became a curious queſtion what would be the habitudes of thoſe metals in effecting changes in the water conſtituting them a ſeries? Sixteen zinc plates, and ſixteen pieces of thin poliſhed iron wire, were connected in galvanic order. One half of the ſeries of glaſſes being filled with ſolution of red ſulphate of iron, and the other half with pump water, the end glaſſes gave faint ſhocks when the tongue was introduced into one, and the fingers into the other. When they was connected ſo as to make the ſeries analogous in all its parts, all the iron wires in the common water gave out gas without oxydating, and theſe in the ſolution exhibited the ſame appearances as the ſilver wires in II. 6.

8. A ſeries of glaſſes compoſed of zinc plates and ſilver wires in galvanic order with pump water, was ſuffered to remain for ſome hours without being connected at the extremities, ſo as to compleat the circuit. At the end of this time ſome globules of gas appeared on ſome of the zinc plates, which were a little tarniſhed; no gas was produced on the ſilver wires, and they had undergone no apparent change.

Having ſet up a powerful ſeries of 27 glaſſes, (ſome with red ſulphate of iron, and ſome water) in which zinc and ſilver wire were the excitors, I found that whenever I ſupplied the place of a pair of plates, either by a ſingle metallic wire, or a chain compoſed of different metals, whatever were their habitudes of oxydation, hydrogen was always produced at the place of the ſilver, and oxygen always fixed or extricated at the place of the zinc. When many ſilver wires were introduced into the ſeries in new glaſſes, ſo as to preſerve the original number of exciting plates, the powers of it ſeemed to be very little diminiſhed, and gas was given out, and oxygen fixed in every new glaſs. When the points of contact of ſome of the plates and wires above the water were covered with cement, the phænomena were the ſame as if they had been expoſed to the atmoſphere. When one pair of a ſeries was in vacuo, the gage being at $\frac{6}{10}$, the powers of the whole were not ſenſibly diminiſhed, and gas was extricated from the ſilver wire.

9. On theſe facts I ſhall not preſume to ſpeculate. There is every reaſon to believe that a number of new experiments muſt be made, before we ſhall be able to diſcover the laws in conſequence of which one quantity of chemical action generates in the galvanic ſeries of Volta, an influence capable of increaſing all analogous actions, and of generating new ſimilar actions. Many new obſervations muſt be collected, probably before we ſhall be able to aſcertain whether water is decompoſed in galvanic proceſſes. Suppoſing its decompoſition, we muſt aſſume, that at leaſt one of its elements is capable of rapidly paſſing in an inviſible form through metallic ſubſtances, or through water and many connected organic bodies; and ſuch an aſſumption is incommenſurable with all known facts. But a ſhort period is elapſed ſince philoſophers beheld with wonder, ſolid and fluid ſubſtances aſſuming new modes of exiſtence in different gaſes. Do not the new phænomena of galvaniſm authoriſe us to hope that at no very diſtant time they will behold even thoſe gaſes undergoing novel changes, and exiſting in new and now unknown forms?

III. *Remarks*

III.

Remarks on the Powers of different Galvanic Combinations.

1. I have found by many experiments, that when muriatic acid is introduced (in quantities ſufficient to produce viſible changes) into water contained in the glaſſes of an effective galvanic ſeries compoſed of zinc plates and ſilver wires, the zinc plates are acted upon, and gas is given out from every part of their ſurfaces; whilſt the quantities of gas produced on the ſilver wires are increaſed only in the apparent ratio of the increaſed power of the ſeries to give the ſhock. When muriatic acid is introduced into a certain number only of the glaſſes of a ſeries, ſimilar changes are produced in thoſe glaſſes; but there is no apparent alteration in the nature of the phænomena taking place in the other glaſſes; theſe phænomena are only rendered more vivid. Effects analogous in *appearance* take place when ſulphuric acid, and even when nitric acid is employed. So that it appears that the power of a ſeries to fix oxygen upon its zinc plates, and to evolve hydrogen from its ſilver plates, is limited, and cannot be increaſed by oxydating bodies beyond a certain extent. We muſt conſequently conclude, that only a certain quantity of galvanic influence can be made to circulate through a ſeries in a given time, and that the increaſe of oxidation beyond a certain term is connected with no new increaſe of power.

2. The ſubſtances which are capable of rapidly oxydating the imperfect metals, and of condenſing naſcent hydrogen at the ſame time, are thoſe which produce the moſt powerful effects, when made the media of connection between the metals in the galvanic ſeries. The nitric and oxygenated marine acids appear to be the moſt powerful of the known fluid excitors of the pile. The ſolutions of metallic ſalts, compoſed of acids and oxydes at their maximum of oxidation, ſtand next in order: then follow the muriatic and ſulphuric acids, and the neutral ſalts containing thoſe acids, or nitric acid.

I have lately endeavoured to make ſome compariſons between the powers of piles conſtructed with nitric acid, and thoſe of common piles; but without much ſucceſs. The rapid action of the acid, the evolution of gaſes connecting the plates by moiſture, and the production of heat, all tended to diſturb the reſults. The ſmalleſt ſeries of plates from which I was able to obtain ſhocks by nitric acid, was compoſed of three pairs. Six pairs, with moderately ſtrong nitric acid, gave a ſhock more acute than that produced by a common pile of twenty-ſeven pairs, but apparently much more limited in extent: it was felt no further than the upper joints of the fingers. With twelve pairs, or ſtill more numerous combinations, the ſhock was always more acute and painful, than from a common pile compoſed of four or five times the number of plates; but apparently more local, and felt over a ſmaller ſurface. As fuſion of the cement always took place when the trough was uſed in theſe experiments, cloths wetted with the acid were generally employed; but in no caſe was it poſſible to prevent the edges of the plates from being moiſtened, ſo that a certain degree of communication between the ends always exiſted, and from this communication the effects muſt have been much diminiſhed. A pile with nitric acid had its power very little leſſened by momentary immerſion in water. Hence the increaſe of effect cannot much depend on the increaſed temperature of the plates.

3. Copper and zinc act very powerfully with nitric acid. Iron and zinc ſeem to act nearly as intenſely with muriatic acid as with nitric acid.

The galvanic combinations that I have been lately moſt in the habits of uſing, are series of glaſſes conſtructed with zinc plates, ſilver or iron wire, and ſolutions of red ſulphate or muriate of iron. Theſe combinations act for a long time intenſely; ſix or eight glaſſes being capable of ſlowly decompoſing water; and if their action is at any time diminiſhed from the depoſition of oxyde of iron on the zinc plates, it may be eaſily reſtored by the addition of a little acid to the ſolution.

4. I noticed in a former paper the conducting powers of charcoal, when made part of the galvanic circuit*, I lately ſet up a ſeries of eight glaſſes, with ſmall pieces of well burnt charcoal, zinc, and ſolution of red ſulphate of iron; the charcoal and zinc being connected by ſilver wire. This ſeries gave ſenſible ſhocks, and rapidly evolved gaſes from water; whilſt an equal ſeries with ſilver and zinc produced much weaker effects. Hence it would appear that charcoal and zinc are equal, if not ſuperior, to any metallic combinations.

⁂ In my laſt paper two errors occur, which require to be corrected. Page 340, line 18, for $\frac{6}{40}$ read $\frac{6}{10}$; and page 341, line 11, for *proportional* read *in great meaſure proportional.*

III.

On the Quotients ariſing from the Diviſion of an Unit by prime Numbers. By H. Goodwyn, Eſq.

To Mr. NICHOLSON.

SIR,

THE following account of the quotients ariſing from the diviſion of an unit, &c. by prime numbers, being, I believe, perfectly new, and promiſing to be very uſeful, is very much at your ſervice; and if you think it worthy a place in your Journal, it may induce the publication of a ſmall table prepared for a farther elucidation of the ſubject.

I am, SIR,

Reſpectfully your's,

Eaſt Smithfield, Oct. 1800. H. GOODWYN.

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* Dr. Wells, in an excellent paper on galvaniſm, in the Phil. Tranſ. has mentioned the great powers of an arc compoſed of charcoal and zinc, in exciting the limbs of frogs.

The quotient of an unit, divided by the prime number 17·, will conſiſt of 16 places of figures, forming a complete circulating decimal. If the numbers, 2, 3, 4, &c. to 16, be divided by the ſame prime number, each reſpective quotient will ſtill conſiſt of 16 places of circulating decimals. Thus far the property of like diviſions has been aſcertained by various writers on decimal arithmetic, &c.

But at leaſt one very curious, conciſe, and uſeful property attached to ſimilar diviſions in general, yet remains to be unfolded. It is this—that the quotient ariſing from the firſt diviſion, virtually repreſents the quotient of every other diviſion above-mentioned.

FIRST QUOTIENT.

	Dividends.		Diviſors.																	
For	1·	÷	17·	=	·$\dot{0}$	5	8	8	2	3	5	2	9	4	1	1	7	6	4	$\dot{7}$
	2·	÷	17	=	-	-	-	-	-	-	-	-	-	-	$\dot{1}$	1	7	6	4	7
					0	5	8	8	2	3	5	2	9	$\dot{4}$						
	3	÷	17	=	-	-	-	-	-	-	-	-	-	-	-	$\dot{1}$	7	6	4	7
					0	5	8	8	2	3	5	2	9	4	$\dot{1}$					
	4	÷	17	=	-	-	-	-	$\dot{2}$	3	5	2	9	4	1	1	7	6	4	7
					0	5	8	$\dot{8}$												

And in like manner the quotient ariſing from the diviſion of an unit by every other whole number, leſs than the diviſor, will commence with a different figure in the firſt quotient, and will circulate to that figure again.

And thus the complete quotient ariſing from the diviſion of each whole number, leſs than the diviſor, in that diviſion may be expreſſed on the firſt quotient, by placing the reſpective dividends over their firſt quotient figure.

Thus	1	10	15	14	4	6	9	5	16	7	2	3	13	11	8	12	dividends.
	·$\dot{0}$	5	8	8	2	3	5	2	9	4	1	1	7	6	4	$\dot{7}$	quotients.

This diſpoſition of the dividend and firſt quotient enables us to find, by inſpection, the complete decimal quotient or expreſſion for a vulgar fraction, whoſe numerator or dividend is any given whole number, between 1 and 17, and whoſe denominator or diviſor is 17. The quotient of $\frac{2}{17}$, $\frac{3}{17}$, and $\frac{4}{7}$, is ſeen in the firſt elucidating arrangement above, and perfectly coincides with this laſt. By this, if I want the complete decimal quotient of $\frac{16}{17}$, I have only to ſearch for the number 16 in the line of dividends, and under it is the firſt figure of the circulating decimal that will comprize complete quotient of $\frac{16}{17}$, viz. ·$\dot{9}41176470588235\dot{2}$, and the ſame of the other dividends.

But what authoriſes the above property to be termed curious, conciſe, and uſeful, is, that it does not attach to the prime number 17· only, but under certain laws is equally applicable to all prime and multiples of prime numbers whatever.

IV.

Conſtruction of a Wheel adapted to expreſs by its Rotation the unequal angular Motion of the Planets. By M. Roemer.

To Mr. NICHOLSON.

SIR,

AS you have profeſſed it to be part of your plan to inſert all diſcoveries in your Journal without regard to date, provided they poſſeſs ſufficient merit, and are not ſufficiently known in this country, I take the liberty of recommending to your notice an invention of ſingular ingenuity, upwards of a century old, but ſo little known, that the (in my opinion inferior, though happy) contrivance of Deſaguliers was offered to the public forty years afterwards, and ſtill continues in uſe.

I am, SIR,
Your's, &c.
R. B.

If it be deſired * to move a wheel of 24 teeth by a pinion of ſix, in ſuch a manner that in certain parts of its revolutions it ſhall move ſo ſwiftly as if it had but 12 teeth, and in other parts as ſlowly as if it had 48 teeth, the method of accompliſhing this is as follows:

1. Deſcribe the right angled parallelogram L M N O, Fig. 1, Plate XVII. having its ſide N O equal to the diameters of the great wheel and the pinion taken together, and its breadth L N equal to their thickneſs, which laſt muſt be greater, the more conſiderable the inequality of the intended movement.

Divide N O in Q, in ſuch a manner that Q O may be to Q N as 6 to 48, that is to ſay, reciprocally as the velocity of the pinion to the greateſt velocity of the wheel.

Divide alſo L M in P, in the proportion of 6 to 12, or reciprocally as the velocity of the pinion to the ſmalleſt velocity of the wheel. Then draw P Q, and as many lines S R, parallel to L M, as there are teeth intended in the great wheel, upon which write the degree of velocity they expreſs, which are in the inverted ratio of their lengths.

2. Let two truncated cones be made in the lathe; one equal to what might be formed by the revolution of the trapezium L P Q M round L N as an axis, and the other equal to what might be formed by the revolution of the trapezium P Q M O round the axis M O.

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* In what follows I tranſlate and take the figure from the *Machines et Inventions approuvées par l'Academie Royale des Sciences* for 1699, page 89.---B.

On the largeſt of theſe two cones let the circles generated by the revolution of the points P, T, Q, be marked and diſtinguiſhed by the ſame numerical figures as the correſponding parallels of the parallelogram L O.

Deſcribe upon the two baſes of the conical fruſtum radial lines, which ſhall make angles at the centre C, Fig. 3, in the ſame proportion to each other as the intended velocities of the wheel, as expreſſed in Fig. 2, and let teeth be cut in the ſurface of the cone according with theſe lines; after which look on the circles which expreſs the different velocities, and have been traced on the ſame ſurface, to find what part of each tooth ought to remain oppoſite its correſponding radius, and cut or file away the reſt. The teeth will thus lie in an elliptical or inclined curve on the conical face, which in the figure is marked by a darker ſhade.

The pinion muſt be made of a regular conical form, as is ſhewn at M O, in Fig. 3.

By this contrivance the largeſt or wideſt teeth will always meet the largeſt part of the pinion, and the narroweſt will correſpond with the ſmalleſt part; by which means, though the pinion has an uniform motion, the wheel will be carried unequably, according to the required law *.

V.

On the Solutions and Precipitates of Mercury. By Cit. BERTHOLLET †.

WHITE ſulphate of mercury, ſlightly oxided, as deſcribed by Citizen Fourcroy in the laſt volume of the Academy of Sciences, is obtained with more eaſe, and in a purer ſtate, by boiling very diluted ſulphuric acid upon mercury. When the liquid undergoes a ſtrong heat, it becomes oxigenated ſulphate of mercury. When there is not too great an exceſs of acid, it is in part decompoſed by the water, which beomes acid by ſeizing a portion of the acid. It, however, contains leſs acid than the mild ſulphate. The proportions between the precipitated oxided ſulphate, and the acid ſulphate ſuſpended in the water, are different, according to the temperatures, the quantity of water, &c.

When the ſulphate is decompoſed by an alkali, the precipitated oxide always retains a ſmall quantity of acid.

Nitric acid has the ſame habitudes with the oxides of mercury. Citizen Gay has remarked, that when mercury is diſſolved by heat in nitric acid, there is firſt a diſengagement of nitrous gas; after which ſolution then takes place quietly, the oxigenated nitrate, which was firſt formed, being decompoſed by the portion of mercury which remains. The nitric ſolution of mercury made by heat, cannot keep all the oxide of mercury in ſolution, except by means of an exceſs of acid.

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* The contrivance of *Deſaguliers,* deſcribed in his *Courſe of Experimental Philoſophy,* I. 464, conſiſts of two elliptical wheels, Fig. 4 and 5, connected by tooth or by catgut (which laſt is a very bad way.) They revolve on their foci, and while the driving ellipſis moves uniformly, the radius vector of the other has the required motion.---R. B.

† Societé Philomath. No. 41.

Muriate of ſoda precipitates the nitric ſolutions of mercury, and affords muriates which differ according to the degrees of oxigenation of the mercury in its ſolutions.

Though the oxides of mercury, when too much oxigenated, cannot remain combined with the ſulphuric and nitric acids, it is not the caſe with reſpect to the muriatic acid which, not being ſaturated with oxygen like the two former diſſolves mercury, and the other metals at every degree of oxidation. Thus mercury combined with that acid in the oxigenated muriate, is much more oxided than in the moſt highly oxided of its nitric combinations.

Bayen has proved, that moſt of the precipitates of mercury contain a ſmall quantity of acid. Citizen Berthollet has added new experiments to thoſe already made by this chemiſt.

When a muriatic ſolution of mercury is precipitated by carbonate of ſoda, an examination of the precipitate, and of the ſupernatant fluid, proves that the latter contains the alkali with a great part of the carbonic acid, muriatic acid, and a ſmall quantity of oxide of mercury. The precipitate is formed of oxide of mercury, muriate of mercury, and carbonate of mercury. When carbonate of pot-aſh is uſed, the precipitate ſeizes all the carbonic acid, and a greater portion of the muriatic acid; ſo that it ſublimes almoſt totally in mercurial muriate. The precipitate contains leſs muriatic acid, if the carbonate of pot-aſh which is uſed contains pure pot-aſh.

Bayen had alſo remarked, that certain precipitates of mercury poſſeſſed the property of detonating when mixed with ſulphur, but he has not explained this phænomenon, and the circumſtances under which it preſented itſelf. Citizen Berthollet has ſhewn, that ſulphur in contact with the oxides of mercury, ſuddenly takes from them the ſlightly adhering oxygen, when theſe precipitates contain only a ſmall quantity of muriatic acid; but this effect cannot take place when the oxide of mercury is defended from the action of the ſulphur by too large a quantity of muriatic acid.

VI.

A Chemical Analyſis of Three Species of Whinſtone, and Two of Lava. By Robert Kennedy, *M. D. F. R. S. & F. A. S. Edin.*[*]

ON the 5th of Auguſt (1798) I announced to the Society that I had diſcovered ſoda in ſeveral varieties of the whinſtone † of Scotland, and alſo in lava from Mount Ætna; but did not deſcribe the various experiments to which theſe ſubſtances had been ſubjected in my examination of them. In the following paper, therefore, I have the honour of laying an account of theſe experiments before the Society.

ANALYSIS I.

Baſalt of Staffa.

The ſpecimen of this baſalt, ſubmitted to the following analyſis, was given me by a gentleman, who brought it himſelf from the celebrated baſaltic columns in Staffa. A deſcription of its external mineralogical characters may be found in Sir James Hall's paper, (p. 8. and 56 of our preſent Volume) to which I beg leave to refer.

This baſalt, though reduced to fine powder, does not effervesce with acids. The colour of the powder is greyiſh, and when wet greeniſh. By being expoſed to a low red heat, the colour of the ſtone is changed to brown. It is not attracted by the magnet, either in its natural ſtate, or after being heated red hot.

Its ſpecific gravity, taken in diſtilled water at the temperature of 60° of Fahrenheit, I found to be 2.872.

Some ſmall pieces being expoſed to a low red heat for half an hour, loſt 5 *per cent.* in weight; and when the ſtone was reduced to powder, and heated red hot, the loſs was the ſame. I alſo examined the effects of high heat on it, in the following manner: having made ſome ſmall crucibles of the porcelain clay of Cornwall, which I uſed on account of its great purity and infuſibility, I baked them in pretty ſtrong fires, generally above 100 of Wedgwood. As ſoon as they were cold, they were each exactly weighed. A portion of the baſalt in fragments, alſo weighed, being put into one of theſe ſmall crucibles, and a pyrometer into another of the ſame ſize, both were placed in a Heſſian crucible. A ſmall flat cover, alſo made of the porcelain clay, was laid upon each; and then a lid was carefully luted on the Heſſian crucible with clay and ſand. The apparatus thus prepared was next ſet into a furnace; and the fire being raiſed gradually till it appeared to have attained the pitch deſired, it was kept as equal as poſſible for about an hour. The ſmall crucible, and the melted baſalt it contained, being weighed as ſoon as cold, it was eaſy to determine how much weight was loſt.

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[*] From the Edinburgh Tranſactions, 1799.

† The name whinſtone is uſed throughout this paper in a generic ſenſe, comprehending baſalt, trap, certain kinds of porphyry, wacken, and ſome other ſtones of the argillaceous claſs.

In this manner ſome of the baſalt was expoſed to a heat of 72 of Wedgwood, at which it was vitrified, and loſt exactly the ſame weight as in a low red heat. At 160 the effects were in every reſpect the ſame; the loſs not being greater in that intenſe fire. The ſmall crucibles, in which the pyrometers had been placed, did not in theſe experiments loſe the ſmalleſt weight.

The volatile matter thus driven off by heat is partly water, as the following experiment ſhows: I put half a pound troy of the baſalt in fragments, into a ſmall Wedgwood retort, and luted to it a receiver, into an aperture of which was fitted one end of a glaſs tube, the other end being adapted to a pneumatic apparatus. The retort was then heated ſlowly to redneſs, and kept moderately red hot for two hours. In the receiver ſome water was condenſed. Some gas alſo paſſed over; but I could not aſcertain with preciſion either its quantity or its nature, as it was mixed with the air of the receiver. I have not made farther experiments on the volatile matter contained in whins; but it deſerves to be examined with attention.

This baſalt being expoſed to heat in a muffle, was found to ſoften at 38 of Wedgwood[*]. Some of it being reduced to fine powder, was boiled in thirty times its weight of water for half an hour. After filtration the water was examined with different chemical teſts, but gave no precipitate with any, except a ſlight cloud with nitrate of ſilver; and a portion being evaporated to dryneſs, left only ſome thin ſtreaks on the bottom of the glaſs.

Having premiſed theſe particulars, I proceed to deſcribe the analyſis:

1. One hundred grains of the baſalt, reduced to fine powder in a Wedgwood mortar[†], were mixed, in a ſmall retort, with about 1200 grains of muriatic acid; and a receiver being adapted, the mixture was gradually heated till it boiled. It was at firſt of a browniſh-yellow colour, but afterwards became browniſh. Part of the powder was diſſolved. To diſtill off the uncombined acid, the heat was continued till the mixture began to grow thick. It was then diluted with boiling diſtilled water, and poured on a filter; and the undiſſolved part, after proper edulcoration, being dried and heated red hot a few minutes, weighed 67 grains, and was greyiſh-white.

2. The filtered ſolution was of a faint yellowiſh-brown colour. Being ſaturated with cauſtic ammonia, a bulky precipitate was thrown down, which was carefully ſeparated by filtration. It had at firſt a dirty greeniſh-colour, which was afterwards changed to brown by the action of the air.

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[*] The fuſibility of this, and the other ſubſtances to be afterwards mentioned, I examined with Sir James Hall. For this purpoſe, a ſmall piece of each was placed, with a pyrometer as near to it as poſſible, in an open muffle previouſly heated to redneſs. It could thus be ſeen perfectly during the operation, and the fire being raiſed, as ſoon as it was found to be ſoft, when preſſed ſlightly by an iron rod, the degree of heat was aſcertained by meaſuring the pyrometer.

[†] The mortar I uſed was not ſcratched by any of the whins or lavas mentioned in this paper.

3. The ſolution, after being freed from this precipitate, was perfectly colourleſs and tranſparent. I dropt into it a ſmall quantity of ſulphuric acid, which produced no cloud; conſequently the ſolution contained neither barytes nor ſtrontian. It was then evaporated to a ſmall quantity, and treated with carbonate of ammonia. Some white earth was thrown down, apparently carbonate of lime, which after being waſhed, dried, and heated red hot a few minutes, weighed 6¼ grains.

4. The inſoluble reſiduum, No. 1. which weighed 67½ grains, I mixed in a ſilver crucible with a ſolution of cauſtic pot-aſh, containing as much alkali as was equal to twice the weight of the reſiduum. This mixture, being evaporated to dryneſs, was expoſed for one hour to a red heat, in which it melted. When cold, the maſs was green. After being ſoftened, and waſhed out of the crucible with boiling diſtilled water, it was superſaturated with muriatic acid, by which the greater part was diſſolved. This mixture, being then evaporated to a ſmall quantity, became gelatinous. It was next diluted with water, digeſted, and filtered. Some ſilex remained on the filter, which, after proper waſhing being dried, and heated red hot a quarter of an hour, weighed 43 grains, and was perfectly white. To learn whether this ſilex was free from every other earth, I mixed a part of it with four parts of carbonate of ſoda, and melted the mixture in a ſilver crucible. Water, being poured on the melted maſs, diſſolved it entirely into a liquor ſilicum, which was diluted largely, and ſaturated exactly with an acid. No precipitate appeared, even after ſix or eight days; therefore theſe 43 grains were pure ſilex.

5. The ſolution, No. 4. (from which the ſilex had been ſeparated), was of a light greeniſh colour. Cauſtic ammonia, when poured into it, threw down a browniſh precipitate. Having carefully ſeparated this precipitate, and waſhed it on a filter, I dropt into the remaining ſolution, which was now colourleſs and tranſparent, a ſmall quantity of ſulphuric acid, in order to detect barytes or ſtrontian. No precipitate was formed. The ſolution was then evaporated to a ſmall quantity, and treated with carbonate of ammonia, by which a ſecond portion of carbonate of lime was obtained. Its weight, after being heated red hot, was 9½ grains.

6. The browniſh precipitates, thrown down from the ſolutions No. 2. and 5. by cauſtic ammonia, had the appearance of argil mixed with iron. To ſeparate the argil, theſe precipitates were mixed together, and boiled, while ſtill moiſt, in a ſolution of cauſtic pot-aſh, in a ſilver crucible. A part was diſſolved; but a ſpongy matter remained, of a darker brown colour than at firſt, which was collected on a filter.

7. Into the cauſtic-alkaline ſolution I poured ſulphuric acid, till ſlightly in exceſs, and neutraliſed it again by carbonate of ſoda. The argil was precipitated; which being ſufficiently waſhed, was re-diſſolved in diluted ſulphuric acid. This ſolution was then mixed with ſome acetite of pot-aſh, and gave, by ſucceſſive evaporations, ſmall regular cryſtals of alum. At laſt it became gelatinous; and being evaporated to dryneſs, and diluted again with water, one grain of ſilex was left. The remaining ſolution produced, to the laſt drop,

cryſtals of alum[*]. I diſſolved theſe cryſtals in water, and precipitated the argil by carbonate of ammonia. After being carefully waſhed, dried, and heated red hot a quarter of an hour, it weighed 12 grains.

8. The browniſh matter, No. 6. inſoluble in cauſtic pot-aſh, ſeemed to be oxyde of iron; and after having been heated red hot, weighed 24¼ grains. I powdered this maſs, and poured on it ſome acetous acid, in order to detect magneſia; but nothing was diſſolved. It was next treated with nitric acid, which diſſolved the iron, but left 4 grains of ſilex. The iron being precipitated, dried, and heated red hot, weighed 20 grains, and was magnetic. Suſpecting that ſome argil might ſtill be mixed with it, from having eſcaped the action of the cauſtic pot-aſh, I diſſolved 5 grains in muriatic acid, and precipitated the iron by Pruſſian alkali. Having ſeparated the blue precipitate, I boiled the ſolution with carbonate of ſoda, and obtained 1 grain of argil. Theſe 20 grains conſiſted, therefore, of 16 grains of iron, and 4 grains of argil.

The remaining part of the iron was melted for an hour, with ten times its weight of nitre, in order to detect manganeſe. The mixture, however, when cold, was not greeniſh; and water made a colourleſs ſolution of the ſaline matter, which did not become turbid, when expoſed ſome days to the action of the air[†].

9. The two portions of white earth above mentioned, which ſeemed to be carbonate of lime, weighed together 16 grains. To ſeparate magneſia, if any were mixed with this earth, I put it into a little water, and added ſulphuric acid till ſlightly in exceſs. Sulphate of lime was produced. Having poured ſome alcohol into this mixture, I filtered it, and waſhed the ſulphate of lime with more alcohol diluted with water. The filtered liquor was then boiled with carbonate of ſoda, but no magneſia was precipitated.

Theſe 16 grains were therefore carbonate of lime, of which, according to Mr. Klaproth's calculation, about 9 grains were pure lime.

One hundred parts of the baſalt of Staffa contain, according to the above analyſis:

Silex, - - -	(No. 4. 7. and 8.). - - -	48
Argil, - -	(No. 7. and 8.). - -	16
Oxyde of iron, -	(No. 8.). - - -	16
Lime, - - - -	(No. 9.). - - -	9
Moiſture, and other vol. matter,	- - - -	5
		94

The ſum is 94 parts; conſequently there is a loſs of 6 *per cent.*

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[*] It therefore contained none of the earth which Vauquelin lately diſcovered, and to which he has given the name of glucine.

[†] I think, however, it is probable, that this baſalt contained a ſmall quantity of manganeſe, both from the browniſh colour of the ſolution, No. 1. and from the green colour which the undiſſolved reſiduum gave, by fuſion, with cauſtic pot-aſh.

About a year ago I analyzed ſpecimens of ſome of the whins in the neighbourhood of Edinburgh, and found, that the ſum of the earths and iron, ſeparated by the analyſes, never amounted to more than 93 or 94 *per cent.*; ſo that the loſs was always equal to that juſt mentioned. It was this circumſtance which firſt led me to ſuſpect that ſome ſaline ſubſtance exiſted in theſe ſtones; and their conſiderable fuſibility favoured the ſuſpicion.

Soon after theſe analyſes were made, I obſerved another circumſtance, which amounted to an abſolute proof of the whins containing ſomething of a ſaline nature, in combination with their earthy baſes. Moſt of the artificial cryſtallites, made by Sir James Hall, which I had always an opportunity of examining, threw out on their ſurfaces, two or three weeks after their formation, a white efflореſcence, which had a very ſalt taſte. It was in too ſmall a quantity to be collected and examined; but when waſhed off, it was often formed a ſecond time.

I was thus convinced of the exiſtence of ſome ſaline ſubſtance in theſe bodies, and made different experiments with ſeveral of them, in order to ſeparate it, and aſcertain its nature; and ſoon found that it was ſoda.

I ſhall next deſcribe ſome of the methods by which this alkali was moſt eaſily ſeparated from the earthy parts of the whins.

Experiments to obtain the Soda, and determine its Quantity.

Having broken ſome of the baſalt of Staffa to ſmall fragments, I weighed 400 grains, and ground the whole with water to an extremely fine powder, in a Wedgwood mortar. The powder, and the water with which it had been ground, were then put into a ſmall retort, and mixed with about 1200 grains of ſulphuric acid, which I had carefully diſtilled for this operation. I placed the retort in the ſand bath of a ſmall furnace which I uſe for analyſes, adapted a receiver, raiſed the fire till the acid began to diſtil ſlowly, and carried on the diſtillation to dryneſs. Water was then poured into the retort, and boiled, the mixture thrown on a filter, and the undiſſolved reſiduum ſufficiently waſhed. This reſiduum was next treated a ſecond time with a freſh portion of ſulphuric acid; and afterwards boiled with water, filtered and waſhed, exactly in the ſame manner as before. The undiſſolved part of the ſtone was now almoſt white.

The filtered ſolutions being mixed together, were evaporated to dryneſs; and the ſaline maſs which remained was heated red hot for one hour in a clean and new Heſſian crucible. When cold the maſs was of a brick red colour. Having powdered it well, I boiled it in water, poured the whole on a filter, and waſhed the reddiſh matter carefully. This filtered liquor, in which all the ſoda, ſeparated from the baſalt by the ſulphuric acid, was diſſolved in the ſtate of ſulphate of ſoda, could contain only a ſmall quantity of earthy matter; for the greater part of the ſulphate of argil, and of iron, formed by the firſt part of the proceſs, muſt have been decompoſed by the red heat, to which the maſs was afterwards expoſed. Accordingly, the ſolution being treated with carbonate of ammonia, only a ſmall quantity of a precipitate was thrown down, which was carefully ſeparated. The ſolution was then evaporated to dryneſs. A ſaline maſs remained, conſiſting, in part, of ſulphate of ammonia; to ſeparate which the whole was expoſed to heat in a ſmall crucible, and when it ceaſed to emit fumes, the heat was increaſed to redneſs. A fixed white ſalt was left, which weighed 25 grains.

This ſalt I re-diſſolved in water, added ſome carbonate of ammonia, and heated the mixture till it boiled. A ſmall quantity of an earthy precipitate was again thrown down, which being ſeparated by filtration, the ſolution was evaporated to dryneſs, and the ſalt which remained heated red hot a ſecond time. By theſe ſucceſſive operations, all the earthy matter, at firſt diſſolved, was ſeparated. The ſalt now weighed 23 grains, and had all the properties of ſulphate of ſoda. Theſe properties were the following:

1. It was not volatile in a moderate red heat.

2. After being thus dried, it diſſolved readily in about ſix times its weight of water, at the temperature of 60 of Fahrenheit.

3. This ſolution gave, by evaporation, cryſtals exactly the ſame in form as artificial ſulphate of ſoda; and theſe cryſtals effloreſced in dry air.

4. A part of the ſolution of this ſalt being boiled with carbonate of ſoda, gave no precipitate; a proof that it contained no earthy matter.

5. Some of the ſolution being mixed with a very ſtrong ſolution of acid of tartar, remained unaffected; the ſalt therefore contained no pot-aſh.

6. Some of the ſalt being diſſolved in water, was decompoſed by nitrate of barytes; and the ſulphate of barytes produced was ſeparated by filtration. The nitric acid, thus united to the alkaline baſis, formed a ſaline compound, which, in the next place, was mixed and deflagrated with charcoal. By waſhing the coaly reſiduum, and evaporating the water, I obtained pure carbonate of ſoda, which effloreſced readily in the air.

There can be no ſuſpicion of the retort which was uſed furniſhing any part of the alkali; for I weighed it previouſly in a balance of great accuracy; and after the operation was finiſhed, found its weight exactly the ſame as at firſt, and the luſtre of the glaſs altogether unimpaired.

The whin which I next ſubmitted to examination, for the purpoſe of ſeparating the ſoda, was taken from a quarry near the Water of Leith*. I uſed a conſiderable quantity, 800 grains; which were diſtilled twice with ſulphuric acid, and then treated in every reſpect exactly as the preceding. The ſulphate of ſoda obtained, amounted to 43 grains.

I afterwards ſubjected ſome other whinſtones to the ſame kind of proceſſes, and in each ſpecies found ſoda. The nitric and muriatic acids alſo diſſolve a certain quantity of the alkali contained in theſe ſubſtances; but their action is weaker than that of the ſulphuric acid.

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* This ſpecies is the firſt mentioned in Sir James Hall's paper. When powdered, it efferveſced ſlightly with acids. I did not analyze it; but, in the courſe of the proceſs for detecting ſoda, one of the earthy precipitates proved, upon examination, to be magneſia. It is the only whin in which I have found this earth.

By the experiments now deſcribed, there were ſeparated from 100 parts of each of the whins, between five and ſix parts of ſulphate of ſoda, which may be equal to two or three parts of pure ſoda. But as theſe two or three parts, when added to the ſum of the earths and iron, did not account for the loſs of 6 or 7 *per cent.* always obſerved in my analyſes, I was ſatisfied that the whole of the alkali was not obtained by the proceſſes which were followed; even although, in that with the whin from the Water of Leith, it had been expoſed, in very fine powder, to the action of the ſulphuric acid, at a boiling heat, for more than eighteen hours. It appeared neceſſary, therefore, to try other methods; and after ſome conſideration it occurred to me, that if the powdered whins could be expoſed, while red hot, to the vapours of the ſulphuric acid, alſo in a red hot ſtate, its power in ſeparating the whole of the alkali from the earthy baſes of theſe ſubſtances, would probably be greatly increaſed in ſo high a a temperature. I ſucceeded in applying a red heat both to the powdered ſtone, and to the acid at the ſame time, by the following means.

Some of the baſalt of Staffa being mixed, in very fine powder, with three parts of ſulphuric acid, the mixture was evaporated ſlowly to dryneſs in a ſand bath. The dry maſs was then heated gradually to redneſs, and kept in the fire for one hour. It was next powdered, and boiled in water; and the water being filtered, was treated with carbonate of ammonia, which threw down a ſmall quantity of a browniſh precipitate. After ſeparating this precipitate by filtration, the liquor was evaporated to dryneſs, and the ſulphate of ſoda, which was left, was purified in the manner already deſcribed, and heated red hot. It amounted to 9 parts for every 100 parts of the baſalt employed.

In this experiment, the ſulphuric acid was firſt united to a part of the lime, of the argil, and of the iron, contained in the ſtone; and afterwards, when the maſs was expoſed to a red heat, the acid was driven off partly or wholly from theſe, and applied in red hot vapours to every part of the powder; by which its action appears to have been rendered much more powerful, as 9 *per cent.* of ſulphate of ſoda was produced: and by the ſame proceſs, ſo ſimple and eaſy to execute, I got from the reſt of the ſubſtances, to be mentioned in this paper, from 8 to 11 *per cent.* of ſulphate of ſoda, although, when they were merely boiled in the acid, the quantity of this ſalt never exceeded 5 or 6 *per cent.*

As the proportion of acid and alkali in neutral ſalts has not been hitherto determined with certainty, the quantity of ſoda in theſe whins cannot be exactly known. But it is probable that 9 parts of ſulphate of ſoda, dried by a red heat, do not contain leſs than 3½ or 4 parts of pure alkali*; which muſt therefore be conſidered as the weight in 100 parts of the baſalt of Staffa; and as 3½ or 4 parts of ſoda, when added to the ſum of the earths and iron, amount nearly to the 100 parts of the ſtone employed in the analyſis, this calculation may be reckoned very near the truth. For the ſame reaſon I think it likely, that the greater part, or the whole of the ſoda, was obtained from the baſalt by the proceſs which has been laſt deſcribed.

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* This is nearly the proportion given by Mr. Kirwan.

It is well known among the friends of the late Dr. Hutton, that he made ſome experiments on zeolite; by which he concluded, that ſoda entered into the compoſition of that ſubſtance *. He has not mentioned the circumſtance in any of his works; but Dr. Black has been accuſtomed, as he informed me himſelf, to take notice of it in his lectures on chemiſtry, for many years. It is my intention to analyze ſome ſpecies of zeolite; and if the reſults ſeem of any importance, they ſhall be laid before the Society.

Among the experiments on the baſalt of Staffa, already deſcribed, it has been obſerved, that, when the powder was boiled in water, a ſlight precipitate was produced in the water by nitrate of ſilver, thus indicating ſome traces of muriatic acid. As it appeared of importance to determine how much of this acid the baſalt contained, I ſubjected ſome of it to examination for that purpoſe.

Experiments to aſcertain the Quantity of Muriatic Acid in the Baſalt of Staffa.

One hundred grains of the ſtone, in fine powder, were mixed in a ſmall retort with ſome nitric acid; and a receiver being adapted, the mixture was boiled gently, till the greater part of the acid had diſtilled over. The liquor in the receiver being examined with nitrate of barytes, remained unaffected; but gave a ſlight cloud with nitrate of ſilver, which ſhewed that it contained ſome muriatic acid.

The maſs in the retort being diluted with water, the whole was filtered; and this filtered liquor produced no cloud with nitrate of barytes, but gave, like the former, a ſlight precipitate with nitrate of ſilver.

In the next place, the undiſſolved reſiduum was mixed with twice its weight of very pure cauſtic pot-aſh, and expoſed to a low red heat, for an hour, in a ſilver crucible. The maſs was then diluted with water, ſuperſaturated ſlightly with nitric acid, and filtered. With this ſolution nitrate of barytes produced no effect; conſequently, theſe experiments ſhow, that the ſtone in queſtion does not contain any traces of ſulphuric acid. With nitrate of ſilver, however, the ſolution gave a white precipitate, more abundant than the two preceding. The different portions of muriate of ſilver, being collected, and dried on a ſand bath, weighed only 4 grains.

As a fourth part of muriate of ſilver conſiſts of acid, according to the moſt correct experiments hitherto publiſhed, theſe 4 grains conſequently indicate, in 100 parts of this baſalt, only about one of muriatic acid. All the whins and lavas, to be mentioned in the remaining part of this paper, were found, by a ſimilar proceſs, to contain about the ſame quantity.

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* In the 26th volume of the *Annales de Chimie*, *p.* 119. M. Scherer, in a letter to Van Mons, ſays, that he was informed by Dr. Black, that Dr. Hutton had, long ago, found *pot-aſh* in zeolite. In this ſtatement M. Scherer is incorrect; becauſe it was ſoda, as above-mentioned, which Dr. Hutton obtained from that ſubſtance.

According to the reſults of theſe different proceſſes, there are, in 100 parts of the baſalt of Staffa,

Silex,	48
Argil,	16
Oxyde of iron,	16
Lime,	9
Moiſture, and other vol. matter,	5
Soda, about	4
Muriatic acid, about	1
	99

I have thus detailed exactly the various experiments performed in analyzing this ſpecies. As all the other whins, and the lavas, which follow, were analyzed in the ſame manner, and exhibited nearly the ſame chemical properties, the reſults only, in each example, ſhall be mentioned.

ANALYSIS II.

Whin of Saliſbury Rock.

The ſpecimen employed was choſen from the ſouth ſide of the cragg, and was perfectly hard and free from decompoſition, as the particular ſpot from which I broke it had been quarried a ſhort time before.

A deſcription of its external characters may be found in p. 54 of the 5th vol. of the Tranſactions of the Royal Society of Edinburgh, (or p. 15 of our preſent vol.) Its powder is light greeniſh grey; but after being wet, acquires a dirty green colour. When heated to redneſs, it becomes light brown. Though not attracted by the magnet in its natural ſtate, it becomes magnetic after being heated red hot. It does not effervesce with acids. When expoſed to a low red heat for half an hour, whether in fragments or in powder, it loſes 4 *per cent.* in weight. It ſoftens at 55 of Wedgwood. Its ſpecific gravity is 2.802. After being boiled, in the ſtate of fine powder, in ten or twelve parts of muriatic acid, the inſoluble reſiduum amounted to 65 *per cent.*

I analyzed 200 grains of this whin, and found that it contained, in 100 parts,

Silex,	46
Argil,	19
Oxyde of iron,	17
Lime,	8
Moiſture, and other vol. matter,	4
Soda, about	3.5
Muriatic acid, about	1
	98.5

(To be concluded in our next.)

VII.

Further Remarks on the Enquiries of Dr. Herſchel reſpecting Light and Heat. In a Letter from Mr. John Leslie.

To Mr. NICHOLSON.

SIR,

London, Nov. 17, 1800.

MY laſt Letter was written under the perſuaſion that your Journal for October contained, at leaſt in ſubſtance, the *whole* of Dr. Herſchel's late experiments and conjectures. Had I foreſeen that the ſubject would be reſumed in a future number, I ſhould certainly have deferred my remarks until the recital was cloſed. I now, therefore, feel myſelf reluctantly compelled to reconſider the queſtion, and to ſtate ſtill farther difficulties, ſhall I ſay fallacies, which preſs the ſequel. In moſt caſes, the toil of criticiſm is miſerable waſte of time; in every caſe, it is equally painful and inglorious; and in coming forward to attack the ſolidity of facts and concluſions ſanctioned by high authority, I ſhall probably, with men of a certain claſs, incur the charge of temerity and preſumption. But ſuch prudential conſiderations I utterly diſregard, being convinced that, on occaſions like the preſent, I may promote the cauſe of genuine ſcience as effectually by detecting errors as by announcing poſitive diſcoveries. Fortunately, I need not at this time engage in much elaborate diſcuſſion. The objections which I formerly urged ſubſiſt in their full force, nor do I find on recollection any material aſſertion which I ſhould deſire to correct.

The additional experiments which claim examination contain little indeed that can be ſtrictly called original. They are employed for the moſt in aſcertaining facts which have been long eſtabliſhed, or which are familiar to every perſon who has the ſmalleſt tincture of ſcience. The paper opens with a formality and apparent caution that might lead us to expect a chain of proofs ſcarcely inferior to mathematical evidence. We are ſoon forcibly reminded however, that *ſuch* is not always the ſureſt road to truth; and that the minute ſpirit of ſubdiviſion has prevailed moſt in the dark ages, and in the barbarous departments of literature, when ſenſe and reaſon were alike buried in the *verbiage* of ſcholaſtic ſyllogiſms, definitions, and diſtinctions. To the myſtic number *ſeven*, the child of judicial aſtrology, the Doctor bows with reverence. Light not only conſiſts in ſeven primitive rays, but each ray has ſeven *properties*; and ſo likewiſe correſponding have the "rays of heat." To muſter up preciſely thoſe ſeven analogous properties, however, required ſome degree of management, ſince one of them is to inform us, that the rays of light and thoſe of heat "are liable to be ſcattered *on* rough ſurfaces;" an expreſſion which, if it has any meaning at all, muſt denote irregular reflection, and therefore, to common apprehenſion at leaſt, ſeems comprized under a former head. The parallel ſo nicely drawn between the viſible and inviſible rays, changes, in the laſt article, into a curious contraſt, which aſſerts, in deſpite of vulgar prejudices, that Light *may not* give Heat, and yet that Heat *may* compoſe Light.

Of twenty experiments which are related in detail not fewer than ſixteen have no direct connection with the queſtion agitated. They refer merely to the heat occaſioned by the rays of light from the ſun, a candle, a fire, or a red-hot poker, when condenſed in the focus of a lens or ſpeculum; or to the heat collected by convergent reflexion from a heated maſs or from the vicinity of a luminated ſubſtance. In all this, I can diſcern nothing either new or ſtriking. But the Doctor thinks fit or convenient to ſubſtitute the word heat for light; and that eaſy change operates like a magical charm. Whatever property has been attributed to the rays of light, belongs henceforth excluſively to the “inviſible rays of heat,” and thus comes directly in ſupport of his darling hypotheſis. Such a mode of reaſoning, or rather aſſumption, hardly deſerves any ſerious refutation. But the Doctor ſeems quite tranſported with the diſcovery. In the eleventh experiment, entitled, “On the Refraction of Solar *Heat*,” a large Newtonian teleſcope, with a compound eye-piece, was directed to the ſun, and by the concentration of the broad beam of light in the focus, a very conſiderable heat, as every body knows, was produced. The philoſopher ſtops, as uſual, to wonder. “How *artfully*,” he exclaims, “in our preſent inſtance, was heat ſent from one place to “another!” And, borrowing the language of Newmarket, he continues:—“Heat croſſing “heat, through many interſecting courſes without joſtling together, and each parcel arriving “at laſt ſafely to its deſtined place.” The grand concluſion is, that “it cannot be doubted “that the rays of heat are ſubject to the laws of refraction.” In the whole of this ſtrange paſſage, perhaps the only thing which ſhould excite ſurpriſe in a ſober mind, is the glaring confuſion of ideas.

The 7th and 8th experiments, which undertake to “reflect and condenſe the inviſible “ſolar rays,” have nothing remarkable but their title. I formerly ſhowed, I hope in a convincing manner, that thoſe imaginary inviſible ſolar rays were merely the warm portions of air which environ an illuminated body. Of courſe, this heat may, as in other caſes, be collected by reflection. It is not my preſent deſign to diſcuſs the nature and propagation of heat. But I cannot forbear mentioning a ſingle argument, which, if I miſtake not, is deciſive againſt the adherents of Radiant Heat. No fact is better known than that the rays of light, in traverſing an uniform medium, are not in the ſmalleſt degree affected by its moſt violent agitation, but purſue unvaried their rectilineal courſe. The caſe is very different with what is termed radiant heat. The experiments on which it reſts ſucceed only in a cloſe room, and at very moderate diſtances from the ſource of heat. It is clear, therefore, that the heating matter muſt flow with ſuch feeble impulſe as to experience obſtruction in its paſſage through the air, and ſuffer much derangement from the accidental motions of that fluid. If this ſuppoſed radiant heat darted with a celerity in any degree comparable to that of light, nay even to the velocity of thoſe projectiles with which we are acquainted,—it would be exactly directed and concentrated in open air, and that at diſtances limited only by the unavoidable imperfection in the figure of the reflecting ſurface. Hence, the heating matter, whatever it really is, muſt have a groſſneſs of conſtitution, and a ſlowneſs of progreſs, commenſurate with the denſeneſs and ordinary mobility of our atmoſphere.

The 17th, 18th, 19th, and 20th, are the only experiments which require any particular notice, as ſeeming to countenance Dr. Herſchel's paradoxical opinions. In the firſt, a ſemicircular piece of paſteboard, covering the half of a large lens, received the coloured priſmatic ſpeculum, but permitted the "inviſible rays" beyond the margin of the red to paſs through the glaſs. A heat of 45 degrees was cauſed in the focus. But with what care and nicety the experiment was performed may be judged from the circumſtance, that the bulb of the thermometer appeared illumed by a reddiſh tint. This perplexing occurrence, however, only inflames the love of the marvellous. The Doctor very gravely proceeds to enquire whether *inviſible* rays can, by condenſation or accumulation, become *viſible*. He muſt entertain indeed a lofty idea of the nature and value of experiment, thus to ſet it in oppoſition to what are accounted the dictates of common ſenſe. Had the reſult proved ſucceſsful, what a triumph gained over the frailty of human reaſon! It would have taught us to liſten with humility to the dreams of a Platoniſt or viſionary of the preſent day, who announces the new and ſublime diſcovery, that the addition of *nothings* makes *ſomething*. But Dr. Herſchel's ſurmiſe was not confirmed; and on repeating his experiments with ſome little more attention, the effect was only 21 degrees, being not the half of what was before produced. This, among other inſtances, may ſerve as a ſample of the author's accuracy and circumſpection. But how did he diſcover that paſteboard would intercept the *whole* of the incident light? He confidently regards this cover, applied too on the very ſurface of the burning glaſs, as a perfect diaphragm. Yet when an experiment, and the only one in any degree concluſive, is adduced, tending to ſupport an opinion moſt repugnant to our general ideas, we might reaſonably expect that every precaution would be uſed, and the previous ſteps at leaſt ſcrupulouſly determined. Common writing-paper, I have found, tranſmits about one-half of the whole incident beam. What was the thickneſs or the texture of the Doctor's paſteboard, we are not informed. But even granting his experiments to be performed with accuracy, if the paſteboard permitted only the ſixth part of the light to paſs, this would have been ſufficient to produce the alledged effect. And after all, what reliance ought to be placed on obſervations which are at variance with every known fact, and every eſtabliſhed principle?

The 19th experiment, whoſe object is "the refraction of inviſible culinary heat," abſolutely proves nothing. A hot cylinder of iron was placed near 3 inches from a lens above an inch in diameter, and a thermometer at an equal diſtance behind it in a poſition correſponding to the ſecondary focus. The thermometer roſe a degree or two as the iron cooled, and diffuſed its heat among the neighbouring bodies. Nor did it require any vaſt ſtretch of ingenuity, ſtill leſs an actual appeal to experiment, to perceive that, each time a ſmall ſcreen was interpoſed, the bulb would ſuffer a certain depreſſion of temperature.

The 20th, and laſt experiment is intended to confirm the preceding; with what ſucceſs will be preſently ſeen. Another thermometer was placed near the former, but conſtantly expoſed to the ſtream of heat: In 4 or 5 minutes it acquired its maximum riſe, amounting to about a degree and a half. The other thermomer which was ſcreened and expoſed alternately, gained an equal increaſe of heat; but, for an obvious reaſon, required to that effect double the time. The obſervation was next repeated by placing both thermometers in a ſituation to be equally affected by the ſcreen, the one in the focus of the lens and the other beſide it. The thermometers kept pace together in their progreſs, and their fluctuations, as often as the ſcreen was interpoſed or withdrawn, correſponded with tolerable preciſion. The irregular difference of perhaps a quarter of a degree is ſurely a quantity too ſmall to form the ground of any legitimate inference. Yet mark with what confidence Dr. Herſchel pronounces his precipitate concluſion: “ All which ſo clearly confirm the effect of the refraction of the lens, that it muſt now be evident that there are rays iſſuing from hot iron, “ which, though in a ſtate of total inviſibility, have a power of occaſioning heat, and obey “ certain laws of refraction, very nearly the ſame with thoſe that affect light.” It is truely aſtoniſhing to obſerve how the mind, when once occupied by ſome favourite idea, recalls it at every ſtep, and tortures every ſlight appearance into an argument for its ſupport.

In another paper ſince read at the Royal Society, Dr Herſchel purſues his ſubject with the ſame monotonous prolixity. It chiefly conſiſts of experiments directed to aſcertain the quantities of light tranſmitted through different coloured glaſſes. But ſuch reſearches are of no real utility in a philoſophical view, unleſs many other points had been fixed which the Doctor entirely overlooks. The thickneſs of the glaſs, its compoſition, and the intenſity of its ſhade, were of the moſt eſſential conſequence. To determine that depth of tranſmitted colour, would have required a compariſon with the triangular cells of Mayer, as improved by Lambert, Achard, and Burja. The variable force of the ſun's rays ought alſo to enter into the account, whether as affected by his different altitude above the horizon, or by the condition of our atmoſphere, and principally in reſpect to humidity. I have obſerved a very ſenſible difference in the power of ſolar light in conſecutive days at noon, even when the ſky was apparently clear. If colours offend the eye by exceſs or defect, ſome groſs eſtimate may be made of their reſpective degrees of illumination; but to determine thoſe in general and with preciſion by ocular contraſt, ſeems altogether an impracticable, if not an abſurd, attempt. In fact, what juſt compariſon can obtain, for example, between green and red, which are ſtrictly things as heterogeneous as taſte and ſmell. To increaſe the embarraſſment, we cannot always judge of the colour of the tranſmitted light by that of the ſubſtance through which the rays are ſent. Thus a ſun-beam, in emerging from paſteboard, ivory, or white enamel, will, according to their thickneſs, exhibit all the tints from yellow to deep red. Yet admitting, in its fulleſt extent, the acaccuracy of Dr. Herſchel's reſults, they furniſh no evidence whatever in ſupport of his hypotheſis, or indeed of any other hypotheſis.

There is only a ſingle paſſage in the concluding paper that requires particular notice. Dr. Herſchel had anticipated one conſequence which I urged as an inſurmountable objection to his ſyſtem; namely, that a burning glaſs would act moſt fiercely at ſome diſtance behind the bright focus. The Doctor tried this, and, as he aſſerts, found it to ſucceed; though he acknowledges at the ſame time, and with great truth, that the experiment was but a *coarſe* one. After ſuch a confeſſion, your readers will judge what degree of credit it really deſerves. Is it at all likely, may poſſible, that ſo many able philoſophers, who for above a century back, have employed their talents on the improvement and application of the burning glaſs, ſhould have totally overlooked a fact ſo obvious and ſo palpable. The Doctor has a remarkable faculty, not peculiar to him however, of ſlurring over thoſe points which immediately affect his opinions, and of dwelling with minuteneſs on what all the world may be preſumed to know. Had he uniformly exerciſed the ſame laudable ſcepticiſm which he profeſſes at the outſet, he might perchance have doubted whether his precurſors were not ſometimes right, and himſelf miſtaken. It would require more than ordinary docility to believe Dr. Herſchel's "coarſe" experiment, in oppoſition to all the former concurrent teſtimonies.

I now take leave, I hope for ever, of this controverſy. Without queſtioning Dr. Herſchel's fidelity, I have ſhown that his aſſertions are not only inconſiſtent with all our general and beſt founded notions, but ſtand directly contradicted by actual obſervations made with peculiar advantages; that his experiments were injudiciouſly contrived, executed without circumſpection, and liable to a multiplicity of inaccuracies; that his reaſonings, how boldly ſoever advanced, were ſtill more defective; that his later experiments are the more vulnerable in proportion to the confident tone which he aſſumes; that by far the major part of them is totally unconnected with the ſubject in diſpute, and only twiſted to ſerve his purpoſe by the ſophiſtical tranſpoſition of terms; and that the few which actually apply are of ſuch obſcure and ambiguous character as to afford no certain evidence. In ſhort, thoſe experiments and conjectures, taking their combined impreſſion, may for a while gratify vulgar curioſity, but muſt ſoon haſten to final oblivion. I ſhould be ſorry if my ſtrictures gave offence to Dr. Herſchel. If I have ſpoken with freedom, I truſt it is in the language which conviction and the love of truth naturally inſpire. Undue authority, always depreſſing, is capable of producing moſt fatal effects, when ſuffered to gain poſſeſſion of the ſciences. How long did the aſcendancy of Ariſtotle, of Des Cartes, and ſhall I add in ſome few points that of the venerable name of Newton, retard the advancement of real knowledge. I reſpect Dr. Herſchel's talents, I admire his aſtronomical diſcoveries, and I am perſuaded that England, in the decline of her ſcience and philoſophy, needs the importatio of genius from abroad, and is honoured by becoming his adopted country. If I cannot equally approve of ſeveral of his late ſpeculations, I reflect that men ſeldom eſtimate aright their own powers, ſeldom know where their real ſtrength lies. Adventuring on new ſubjects they are not likely all at once to acquire the ſkill, preciſion, and caution which are generally the fruit of experience and patient application.

But I cannot finiſh the letter without addreſſing a few words to you, Sir, as editor of a reſpectable Journal. I advert to the note with which you have honoured me at page 348. I certainly preſumed that you gave unchanged the ideas, if not the words of your author. In this it appears I was miſtaken; and I chearfully retract the expreſſion to which it gave riſe, and which were merely the ſpontaneous effuſion of the moment. Dr. Herſchel only *ſuppoſes* "that the cooling cauſes *muſt* muſt have a ſtronger effect on the mercury in a ſmall bulb;" but *why* they ſhould have ſuch effect, is left for others to diſcover. Nor do I think, Sir, that you were happy in the attempt to extricate him from the dilemma. I readily grant that currents of air may aſcend, which, though warmer than the encircling maſs of atmoſphere, are colder than the body itſelf. But I maintain that thoſe currents *have already* received their heat, and aſcend only *in conſequence* of that communication.—The air which touches the hot body receives its full ſhare of heat, and, at the ſame inſtant, its force of aſcenſion: after it has begun to mount, the effect has ceaſed; nor can it exert its cooling energy unleſs it again deſcends, to renew the proceſs*. To argue otherwiſe would require the aſſiſtance of a ſort of aitiology like that with which ſome chemical writers have amuſed us of late years concerning pre-diſpoſing or pre-exiſtent affinities. But I will not dwell on a diſpute of trivial moment. The author of a periodical work, who is obliged, frequently perhaps without premeditation, to ſatiſfy the urgent calls of his compoſitor, is entitled to much indulgence. I moſt readily excuſe the paſſage which was criticiſed, and which might inadvertently ſlip from your pen; and by printing the above, you will give to the public one proof more of your impartiality and candour.

I am, SIR,

Your moſt obedient Servant,

JOHN LESLIE.

[*Erratum*—in laſt Number, page 346, line 9, before *viſual* read *ſame*.]

VIII.

Experiments and Obſervations on the Light which is ſpontaneouſly emitted, with ſome Degree of Permanency, from various Bodies. By NATHANIEL HULME, *M. D. F. R. S. and A. S.*†

INTRODUCTION.

THE diſcoveries which have been made with reſpect to light, as it proceeds immediately from the ſun, are many and important; but the obſervations on that ſpecies of light which is ſpontaneouſly emitted from various bodies, are not only few in number, but in general very imperfect. The author is therefore deſirous of drawing the future attention of the philoſopher more particularly to this ſubject, and of communicating his own experiments and obſervations upon it, to this learned Society.

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* The air, which is firſt heated, aſcends upon the ſame principle as other floating bodies; namely, becauſe its ſpecific gravity is diminiſhed, and the upward preſſure of the fluid beneath, is, therefore, greater than that of the ſuper-incumbent column added to that of the heated maſs. The lower part of the whole aſcending current (which Mr. L. ſeems to overlook) is as cold as the reſt of the atmoſphere; and it is obviouſly this portion which maintains the cooling proceſs, by ſtriking the inferior ſurface of the thermometer in its aſcent.---N.

† Philoſ. Tranſ. 1790, p. 161.

By the ſpontaneous emiſſion of this light, the author wiſhes to diſtinguiſh it from all kinds of artificial phoſphorus; which, as he apprehends, differ eſſentially, in ſome of their properties, from that light of which he means to treat. And, by its adheſion to bodies with ſome degree of permanency, he diſtinguiſhes it from that tranſient ſort of light which is obſervable in electricity, in meteors, and in other lucid emanations. The light which is the ſubject of this paper, he ſhall therefore beg leave to diſcriminate by the name of *ſpontaneous light.*

The ſubſtances from which ſuch light is emitted, are principally the following.

Marine animals, both in a living ſtate, and when deprived of life. As inſtances of the firſt may be mentioned, the ſhell-fiſh called *pholas*, the *meduſa phoſphorea*, and various other *molluſca.*

When deprived of life, marine fiſhes in general ſeem to abound with this kind of light. The honourable Mr. Boyle commonly obtained light, for his uſe, from the whiting, as appears from many parts of his works: the author of theſe experiments and obſervations procured his fiſh light chiefly from the herring and the mackerel.

The fleſh of quadrupeds has alſo been obſerved to emit light. Inſtances of this are mentioned by Fabricius *ab Aquapendente;* by T. Bartholin; by Mr. Boyle; and by Dr. Beale; for which, ſee T. Bartholin, *de Luce Animalium*, p. 183; Boyle's Works, Vol. III. p. 304; Phil. Tranſ. Vol. XI. p. 599.

In the claſs of inſects are many which emit light very copiouſly, particularly ſeveral ſpecies of *fulgora* or lantern-fly, and of *lampyris* or glow-worm; alſo the *ſcolopendra electrica;* and a ſpecies of crab, called *cancer fulgens.*

Rotten wood is well known to emit light ſpontaneouſly. Peat earth alſo has the ſame property. Of the effects of the latter, a remarkable inſtance is related in Plot's Natural Hiſtory of Staffordſhire, p. 115.

The place where the following experiments were made, was a dark wine-vault, which, for diſtinction's ſake, the author calls the *laboratory.* The heat of this laboratory varied, throughout the year, from about 40 degrees of temperature to 64°. The thermometer made uſe of was that of Fahrenheit.

The weight is always to be ſuppoſed that called troy weight. The liquid meaſure employed, was that uſed for wine in this country: the ounce containing 8 drams avoirdupois; and the pint, 16 ounces.

The water uſed in general for the experiments, was pure ſpring water, drawn up from under ground by means of a pump; and it was always employed cold, unleſs otherwiſe expreſſed.

SECTION

SECTION I.

The Quantity of Light emitted by putreſcent Animal Subſtances, is not in Proportion to the Degree of Putrefaction in ſuch Subſtances, as is commonly ſuppoſed; but, on the contrary, the greater the Putreſcence, the leſs is the Quantity of Light emitted.

EXPERIMENTS.

Exper. 1. Two very freſh herrings were bought in the morning, and hung up in the laboratory; on examining them in the evening, they were beginning to be luminous.

Exper. 2. Three herrings, which were quite freſh, after being ſcaled and gutted, were hung up by a ſtring in the laboratory. The next evening they were become exceedingly luminous in every part, and much lucid matter had exuded, as it were, upon their ſurface, which was eaſily ſcraped off by the blunt edge of a knife; it alſo adhered to the fingers, or other parts of the body, when touched; but, as they grew more putreſcent, the quantity of light diminiſhed, and at laſt was extinguiſhed.

Exper. 3. A ſingle herring, that was perfectly ſweet, was hung up in the laboratory. On the ſecond night, it was covered with light; on the third, not ſo lucid; on the fourth, leſs ſo; and ſo on, in proportion to the degree of putreſcence.

Exper. 4. Two herrings, ſomewhat ſtale, were hung up in the morning, and at 8 P. M. one of them was pretty luminous, but the other leſs ſo. On the next evening, the former was but ſlightly luminous, and the latter was dark; on the ſucceeding evening, they were both dark.

Exper. 5. Two mackerels were brought from the market at 1 P. M. which, to the ſight and ſmell, were perfectly ſweet and good. Being then carried into the dark laboratory, and examined, the one was found to be a little luminous, and the other pretty much ſo eſpecially about its belly.

Exper. 6. A fine freſh mackerel, with a bright eye, was purchaſed about noon, and placed as uſual in the laboratory, the temperature of which, at that time, was about 54°. At 11 P. M. this beautiful fiſh was luminous about the head and upper parts; and the inſide of the mouth, which was wide open, ſhone with moſt brilliant light. The next evening, the whole body of the fiſh was very luminous: on the third night, it was leſs ſo; and on the fourth the light was nearly extinguiſhed.

Exper. 7. In the forenoon, about ten o'oclock, a couple of fine-looking mackerels were hung up in the laboratory, at the temperature of 56°, and at 10 P. M. they began to ſhine in various parts, the light ſeeming to proceed from within outwards. On the ſecond night, they put on a luminous appearance all over their ſurface: on the third, the light was not ſo vivid; and on the fifth it was almoſt extinct.

N. B. In experiments of this kind, for the production of light, the fiſhes ſhould always be gutted, the roes taken out, and the ſcales, if any, carefully removed. As the roes are likewiſe very productive of light, they ſhould be preſerved.

OBSER-

OBSERVATIONS.

Obs. 1. Theſe experiments clearly prove, that light begins to be emitted by marine fiſhes, before any ſigns of putrefaction appear: they likewiſe demonſtrate, that as ſoon as a great degree of putreſcence has taken place, the luminous property of the fiſhes is deſtroyed, and the light extinguiſhed.

Obs. 2. In the inſtance of light proceeding ſpontaneouſly from animal fleſh, recorded by Aquapendente, the fleſh emitted light before any ſenſible putreſcence had taken place, the meat being hung up in the larder for uſe. In that alſo mentioned by Bartholin, in 1641, the fleſh muſt have been freſh and ſweet, for it was not intended to be dreſſed until the next day. Mr. Boyle, in his report of light iſſuing from fleſh, expreſsly ſays, that neither he, nor any of thoſe who were about him, could perceive in it any offenſive ſmell, whence to infer any putrefaction; the meat being judged very freſh, and well conditioned, and fit to be dreſſed. And, laſtly, Dr. Beale, in his account of a luminous neck of veal, ſays, that when it was dreſſed, on February the 27th, ſome of the neighbours, who ſaw it ſhining, were invited to eat of it, and all eſteemed it as good as they had ever taſted; that a part of it was kept for February 28th and 29th, in which time it loſt nothing of its ſweetneſs.

Obs. 3. Whenever I wiſh to obtain a plentiful ſupply of light from fiſhes, for the purpoſe of experiments, I always endeavour to procure the freſheſt that can be had: long experience and frequent diſappointments have taught me to adopt ſuch a precaution.

SECTION II.

The Light here treated of is a conſtituent Principle of ſome Bodies, particularly of Marine Fiſhes, and may be ſeparated from them, by a peculiar Proceſs; may be retained, and rendered permanent for ſome Time. It ſeems to be incorporated with their whole Subſtance, and to make a Part thereof, in the ſame Manner as any other conſtituent Principles.

EXPERIMENTS.

The Fleſh of Herring[*].

Exper. 1. A freſh herring was ſplit, or divided longitudinally, by a knife, into two parts. Then, about four drams of it, being cut acroſs, were put into a ſolution, compoſed of two drams of Epſom ſalt or vitriolated magneſia, and two ounces of cold ſpring water drawn up by the pump. The liquid was contained in a wide-mouthed three-ounce phial, which was placed in the laboratory. Upon carefully examining the liquid, on the ſecond evening after the proceſs was begun, I could plainly perceive a lucid ring (for the phial was round) floating at the top of the liquid, the part below it being dark; but, on ſhaking the phial, the whole at once became beautifully luminous, and continued in that ſtate. On the third evening, the light had again riſen to the top; but the lucid ring appeared leſs vivid, and, on ſhaking the phial as before, the liquid was not ſo luminous as on the preceding night.

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[*] The quantity uſed in each experiment was about four drams.

Exper. 2. The ſame experiment was repeated. On the ſecond night, the liquid, being agitated, was very luminous; on the third, not ſo lucid; and on the fourth, the light was extinguiſhed.

Exper. 3. With ſea ſalt or muriated natron half a dram, and two ounces of water. On the ſecond night, the liquid, when agitated, was dark; on the the third, lucid; on the fourth, very luminous; on the fifth, it began to loſe light; on the ſixth, it continued to decreaſe; and on the ſeventh it was quite gone. Neither the liquid, nor the herring, had contracted any putrid ſmell.

Exper. 4. With ſea water two ounces. On the ſecond night, dark; on the third, fourth, and fifth, luminous; on the ſixth, nearly extinct; and on the ſeventh, totally. The piece of herring, when taken out and examined, was remarkably ſweet.

Roe of Herring *.

Exper. 5. With Epſom ſalt two drams, and water two ounces. On the ſecond night, the liquid was pretty luminous; on the third and fourth, ſtill luminous; and on the fifth its light was extinct.

Exper. 6. With Glauber's ſalt or vitriolated natron two drams, to two ounces of water. On the ſecond night, when the phial was ſhaken, as uſual in all theſe experiments, the liquid was pretty luminous; on the third, leſs ſo; and on the fourth the light was ſcarcely viſible.

Exper. 7. With ſea water two ounces. On the ſecond night, dark; on the third, the liquid was moderately luminous; on the fourth and fifth, it had extracted much light; and on the ſeventh it was ſtill ſhining. After this proceſs, both the roe and the ſea water remained perfectly ſweet.

The Fleſh of Mackerel.

Exper. 8. With Epſom ſalt two drams, and water two ounces. On the ſecond night, the liquid was finely illuminated; on the third, a ſimilar appearance; on the fourth, a diminution of light; on the fifth, it continued lucid in a ſmall degree; annd on the ſixth the light was extinguiſhed.

Roe of Mackerel.

Exper. 9. With Epſom ſalt two drams, and water two ounces. On the ſecond night, the liquid, when agitated, was exceedingly bright; on the third, the ſame; and on the fourth and fifth, ſtill lucid.

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* The quantity uſed in each experiment was about four drams.

The Tadpole.

Exper. 10. It occurred to my mind, in the year 1797, to try what effect a ſaline menſtruum would have upon the tadpole. Accordingly, I procured ſome tadpoles on the 10th of June, and put ſix of them into a ſolution of two drams of Glauber's ſalt in two ounces of water. On the 11th, in the evening, the menſtruum was dark; on the 12th, after ſhaking the phial, I was agreeably ſurpriſed to find it impregnated with light; on the 13th, the light was ſo abundant as to float on the top of the menſtruum; on the 14th, the ſame phænomenon appeared; on the 15th and 16th, it was ſtill preſent; on the 17th, the lucidneſs began to diminiſh; on the 18th, it was faint; and on the 19th it had vaniſhed.

Exper. 11. On the 11th of June, ſix other tadpoles were dropped into a ſolution of one dram of common ſalt in three ounces of water. On the 12th and 13th, the menſtruum was dark; on the 14th, it had extracted from the tadpoles a very beautiful bright light; on the 15th, the menſtruum was exceedingly luminous; on the 16th and 17th, nearly the ſame: the light then gradually faded, ſo that on the 21ſt it was merely viſible; and on the 22d it diſappeared.

Exper. 12. On the 21ſt of June, the above two experiments were repeated; when the tadpoles remained in the menſtruums till the 27th, but no light was emitted. What was the cauſe of this failure in theſe two laſt experiments? Was it the ten days' increaſed growth of the animal, which was taken from the ſame pond, that made the difference?

Exper. 13. The above experiments were repeated, when the tadpole had juſt put on the ſtate of a frog, but without producing any lucid appearance.

The Light is incorporated with the whole Subſtance of Marine Fiſhes.

Exper. 14. A fine freſh herring, being gutted, was divided longitudinally into two parts, both of which were hung up, by pieces of ſtring, in the laboratory. On the 2d night, they were very lucid on the ſkinny ſide, but not on the fleſhy or inward part; on the 3d, the fleſhy or central parts of the fiſh, were thickly covered with a rich azure light; on the 4th, they continued exceedingly luminous; and on the 5th and 6th they were ſtill lucid. It is ſurpriſing to think what a profuſion of light was emitted from the interior ſubſtance of this ſingle fiſh.

Exper. 15. A ſimilar experiment was made with a mackerel, and with ſimilar effects. Theſe two experiments were frequently repeated.

Exper. 16. But the ſoft-roe, of both the herring and the mackerel, abounds more with light than even the fleſh. When it is in its moſt luminous ſtate, which generally happens about the 3d or 4th night, it will ſometimes ſhine ſo very ſplendidly, as to appear like a complete body of light. It is remarkable that the hard-roe, in general, does not emit ſo much light as the ſoft-roe. When the roes were uſed, they were laid upon plates, and depoſited in the laboratory.

OBSER-

OBSERVATIONS.

Obs. 1. The above experiments clearly prove, as I apprehend, that this light is a conſtituent principle of marine fiſhes: and that it is ſeparated, by the menſtruum employed on this occaſion, in the ſame way that the principles of any other body are ſeparated, by the menſtruum fitted to decompoſe it. They likewiſe ſhow, that it is not partially but wholly incorporated with every part of their ſubſtance, and makes a part thereof, in the ſame manner as any other conſtituent principle.

Obs. 2. Light is probably the firſt conſtituent principle that eſcapes, after the death of marine fiſhes. The experiments of the firſt Section teach us that it appears ſoon after death, even in fiſhes which, to the eye, ſeem quite freſh and ſweet; or, at leaſt, long before any ſenſible putreſcence takes place. And we have ſeen that the fleſh and roes, infuſed in the ſaline menſtruums, continued to emit light for ſeveral days, without undergoing any apparent putrefactive change.

Obs. 3. The experiments likewiſe render it probable, that no offenſive putrefaction takes place in the ſea, after the death of ſuch myriads of animals as muſt needs daily periſh in the vaſt ocean, (quite contrary to what happens on land;) and that the fleſh of marine fiſhes remains pretty ſweet for ſome time, and may become wholeſome food for many kinds of thoſe which ſtill remain alive. An eminent inſtance this, of the wiſdom of the Creator, in the conſtruction of the aqueous part of the world, which comprehends, by far, the greateſt portion of the terraqueous globe, and is the moſt replete with animal life!

SECTION III.

Some Bodies or Subſtances have a Power of extinguiſhing ſpontaneous Light when it is applied to them.

EXPERIMENTS.

The luminous matter proceeding from the herring and the mackerel, was quickly extinguiſhed when mixed with the following ſubſtances: 1. Water alone. 2. Water impregnated with quick-lime. 3. Water impregnated with carbonic acid gas. 4. Water impregnated with hepatic gas. 5. Fermented liquors. 6. Ardent ſpirits. 7. Mineral acids, both in a concentrated and diluted ſtate. 8. Vegetable acids. 9. Fixed and volatile alkalis, when diſſolved in water. 10. Neutral ſalts: *viz. ſaturated* ſolutions of Epſom ſalt, of common ſalt, and of ſal ammoniac. 11. Infuſions or chamomile flowers, of long pepper, and of camphor, made with boiling-hot water, but not uſed till quite cool. 12. Pure honey, if uſed alone.

(To be concluded hereafter.)

IX.

On Areometry; more particularly as it relates to Alcohol of different Strengths and Temperatures. By Cit. HASSENFRATZ*.

THE Author calls the inſtruments made uſe of for determining the proportion of mixtures of alcohol and water, alcogrades; and after a ſhort preface reſpecting the uſes of ſpirituous and vinous fluids, he proceeds to enumerate the following ſix methods of meaſuring the ſtrength of brandies, &c. 1. By the bubble or bead which appears when the fluid is ſhaken. 2. The ſwimming or ſinking of oil poured therein. 3. Diſtillation, to ſhow the quantity of ſpirit. 4. Burning the fluid in a ſilver veſſel, and noting the reſidue. 5. Wetting a known quantity of dry gunpowder with a little of the ſpirit, and obſerving the facility or difficulty with which it inflames. Theſe five methods are, as he remarks, not only too uncertain to ſhew the difference of ſpirit with the deſired preciſion, but will even give very different reſults, according to the management, with ſamples of the very ſame ſpirit. 6. The fixth method conſiſts in determining the ſpecific gravity with the Areometer, which as it has an appearance of preciſion, deſerves to be more minutely examined.

The Areometer or inſtruments for determining this ſpecific gravity of fluids are of two kinds, namely, 1. a floating ball, with a ſtem above, either graduated or ſupporting a diſh to receive weights, and a counterpoiſe below to preſerve the erect poſition; and 2. a bottle terminating in two ſmall apertures, and capable of holding the ſame invariable bulk of fluid.

The firſt, namely, the hydrometer with the graduated ſtem is moſt in uſe. Inſtruments of this kind have been graduated in France by obſerving certain points to which the ſubſidence was made in water and in alcohol, or in a ſaline ſolution. Nine of thoſe more generally uſed in that country are exhibited in the following table:

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* Abſtracted from the fourth Memoir on this ſubject, in the Annales de Chimie, XXXIII. 3.

Table of Compariſon for the Areometers of

Names of the Liquors.	Names of the Inventors or Companies.										
	Lante-nay	Cartier	Baumé	Buffat	Machi		Mac Daniel	Juges d'Aunis	Marchands de Paris	Struve	Specific gravity
Rectified Alcohol	80	36	40	100	66	0	33	16	130	37.5	0.8276
Melaſſes Spirit	78	35	38	93	64	1	30.75	15.5	127	35.5	0.8372
Common Alcohol	74	33	35.3	87	62	2	27.8	13	121	33	0.8508
Cognac 6—11	65	31	32.75	79	52	7	25	12	106	30.75	0.8636
Brandy 4— 7	60	30	32	75	49	9	24	11.75	100	30	0.8675
Brandy of Barcelona	61	31	32.75	79	51	7.75	25	11.5	102	30.7	0.8636
— of Montpellier	59	29.75	31	75	47	9.75	23	11.3	95	29	0.8727
— potable ſimp. 4 years	30	20	20.5	40	23.75	22	11.3	3.5	48	19.6	0.9320
of 20 years	28	30	20	40	23	22.75	10.5	3.5	46	19.1	0.9351
— artificielle 6—11	25	19	18.75	34	20	23.75	9.3	2.5	40	17.	0.9427
Red Champagne wine	5	12	11	10	2	33	0.5	0	13	11.5	0.9931
White Burgundy	4	11.75	11	9	2	33	0.5	0	14	11.	0.9931
White Orleans Vinegar	0	10	9	2	0			0	0	9.	1.0070
Diſtilled Water	0	10	10	5	0	34	0.5	0	0	10	1.0000

From the imperfection of theſe inſtruments, particularly with regard to the indication of the component parts of ſpirituous mixtures, it has been an object of importance to philoſophers, and the projectors of legiſlative impoſts, to examine the ſubject with great preciſion. Among the philoſophers who have made reſearches on this head, our author confines his notice to Baumé, Briſſon, Gouvenain, Struve, Jacob Fagot, of Stockholm, and Blagden. Of the reſults obtained by theſe philoſophers, he gives conciſe tables, by means of which he has traced curves to exhibit the ſame with their comparative degrees of regularity and reſpective differences. See Plate XVIII.

In examining theſe curves, he found that thoſe of Fagot and Struve abound with ſinuoſities, which prove that their experiments were not made with due care, and ought to be rejected out of the compariſon: beſides which, the fluids they uſed were too remote in their denſities, from thoſe which were ſubjected to experiment by the four other philoſophers.

On examining the curves conſtructed from the experiments of the laſt, it was found that the curve of Baumé contains alſo a great number of ſinuoſities; that of Blagden leſs, and thoſe of Briſſon and Gouvenain were the moſt regular. Theſe curves alſo appear to indicate, that the alcohol uſed by Briſſon had a ſtronger attraction for water than that of Gouvenain; that this laſt was ſtronger in this reſpect than that of Blagden: and laſtly, that the ſpirit uſed by Baumé had a weaker attraction than either of the other three. This important obſervation ſhews, that the practical uſe of any ſet of tables in determining the ſpecific gravities of different mixtures of ſpirit and water, muſt, to a certain degree, be confined to that ſpecies of liquor from which the tables, or equivalent inſtrument was formed.

Notwithſtanding the regularity of the tables of Briſſon and Gouvenain, Cit. Haſſenfratz was obliged to reject them in his farther proceedings, becauſe thoſe of Briſſon were made at one temperature only; and from the very uniform courſe of the differences in thoſe of Gouvenain, he ſaw reaſon to conclude, that they muſt have been made by interpolation between few and diſtant experiments. He gives, nevertheleſs. tabulated abſtracts of their reſults. The Royal Society's experiments publiſhed by Sir Charles Blagden, are conſequently the foundation of his conſtruction of an alcograde.

If from theſe experiments curves be traced, expreſſing the denſities of the ſeveral combinations of water and alcohol taken at different temperatures, he obſerves, that with regard to alcohol and its ſeveral combinations, as low as nine parts of alcohol, and one of water, the denſity follows the inverſe proportion of the temperature, and the curve becomes a ſtrait line; but that every leſs proportion of alcohol follows a different inverſe proportion; that is to ſay, the line which paſſes through the extremities of the ordinates, is a curve of which the radius at curvature is longer the greater the proportion of water.

After ſome diſcuſſion of the method of forming the combinations by weight and by meaſure, the firſt of which has the great advantages of accuracy and facility of experiment, with regard to thoſe operations which require change of temperature; and the latter poſſeſſes that of being more accommodated to the commercial habits of ſociety; Citizen H. makes ſome obſervations on the difference between brandies, and the mixtures of alcohol and water. Brandy, ſays he, is the product obtained by diſtillation from a vinous liquor, in which the action of fire changes the order of the component parts, and carries over this fluid: alcohol is the product of a ſecond operation of the ſame nature, in which ſimilar effects are produced upon the brandy. In proof of the force of this method of conſidering the ſubject, he remarks, that the brandy will not again form wine by mixing it with the reſidue left in the ſtill: neither will the alcohol form brandy by mixture with the reſidue, from which it was produced and driven over.

The differences of the vinous fluids in denſity above or below that of water, in flavor, and other obvious characters, are extreme; and the brandies from wine properly ſo called, from beer, perry, cyder, &c. are no leſs remarkable for their peculiar qualities. All theſe afford alcohol, which has been ſuppoſed to be then the ſame fluid; but our author conſiders theſe alſo as differing in their immediate and intrinſic qualities. The alcograde is inſufficient for the ſtrict exhibition of the variations of theſe with water, and moſt obviouſly with reſpect to brandies, of which the price is governed ſo much by qualities and circumſtances, not at all commenſurate with their ſpecific gravities. For wines, beer, and other immediate products of fermentation, this method of examination does not appear to be of any uſe.

He admits, nevertheleſs, that the alcograde is beſt adapted of any inſtrument for giving that approximate indication of value which the tranſactions of life demand. He propoſes the conſtruction of a floating inſtrument, which ſhall ſhew the proportion of alcohol by inſpection at any known temperature by graduation. The following table, is derived from the Experiments of Gilpin, in the Philoſophical Tranſactions 1794; but I muſt, for the ſake of brevity, leave the conſideration of the method of its fabrication to the ſcientific reader.

Table of the lengths of the Tubes of Alcogrades, which ſhall ſhew the Proportions by meaſure of Alcohol in 1000 *parts of any Mixture, at different Temperatures.*

Proportion of alcohol in 1000 parts of the whole mixture.	Length of the Tube for the Temperatures Centigrade.						
	0°	5°	10°	15°	20°	25°	30°
0	—9	—10	—7	0	+9	+21	+36
100	+102	+102	+113	118	130	141	152
200	180	182	197	205	220	243	257
300	240	260	279	300	328	344	362
400	327	356	424	427	442	468	493
500	446	482	515	552	588	620	660
520	484	516	559	587	624	658	698
540	508	551	585	623	661	697	737
560	550	587	622	660	699	737	777
580	589	624	660	698	739	778	819
600	630	662	700	738	780	820	862
620	670	702	742	779	823	863	906
640	711	743	786	822	868	908	952
660	753	785	831	866	914	954	1001
680	796	828	877	912	963	1001	1052
700	839	878	925	961	1012	1050	1104
720	885	921	976	1006	1062	1101	1157
740	933	970	1028	1061	1113	1154	1211
760	983	1021	1081	1127	1165	1209	1266
780	1036	1075	1135	1178	1217	1267	1322
800	1091	1130	1190	1231	1270	1327	1379
820	1147	1188	1247	1288	1327	1388	1437
840	1205	1248	1305	1348	1388	1450	1496
860	1266	1310	1364	1411	1451	1513	1556
880	1327	1375	1424	1477	1518	1577	1618
900	1390	1441	1485	1545	1588	1641	1682
920	1456	1507	1550	1616	1660	1709	1751
940	1524	1578	1620	1690	1734	1781	1826
960	1595	1652	1696	1767	1809	1858	1907
980	1669	1729	1778	1847	1885	1941	1994
1000	1741	1810	1864	1930	1961	2025	2086

In applying this table to the purpoſe of graduating the (cylindrical or priſmical) ſtem of an alcograde or hydrometer, the author ſuppoſes the inſtrument to be plunged in a liquid of the ſpecific gravity 90,000, and afterwards in another liquid* of the ſpecific gravity 100,000

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* In this proceſs, ſince theſe diviſions indicate ten thouſandth parts of the portion moſt deeply immerſed, and there are 2096 diviſions on the whole ſtem, it will conſtitute rather more than one-fifth part of the bulk of

100,000, and that the difference or interval upon the ſtem between the interſection by the ſurface of the fluid in the firſt and in the laſt ſituation, ſhall be taken as a ſcale of 1000 parts; and of theſe parts the requiſite portions being ſet off from the table upon the ſtem, as marked Fig. 1. Pl. XVIII. the inſtrument is then ready for uſe.

It ſeems almoſt unneceſſary to remark, that the ſcale upon the ſtem muſt be read upon that vertical line which is marked with the temperature which a thermometer ſhews the fluid to poſſeſs. The inclined croſs line, which interſects at the ſame place that vertical line and the ſurface of the fluid, is marked with the number denoting the proportion of alcohol contained in 1000 parts of the fluid. The reduction of this reſult to any deſired ſtrength or proof, of which the proportions of alcohol and water are known, will be eaſy to moſt arithmeticians, but would extend our limits too far if examples were to be given here.

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of the inſtrument. Hence by a rough calculation, if the length of the ſtem be about five times the diameter of the ball, the diameter of the ſtem will prove about one-ſixth part of the diameter of the ball.

It is not neceſſary to prepare two fluids of the preciſe denſities above-mentioned in order to find the interval, as it may eaſily be had from any two fluids whatever. Thus, if water at 1000, and alcohol at 840, were uſed, the interval on the ſtem would be denoted by $\frac{1000-840}{1000}$ or $\frac{160}{1000}$ or $\frac{1600}{10000}$ of the portion moſt deeply immerſed, which are nearly the diviſions of the table. Or, if a ſingle fluid be prefered, then--- 1. Weigh the whole inſtrument ſuſpended to the diſh of a pair of ſcales. 2. Find the apparent loſs the inſtrument ſuffers by immerſion in water, with a load or ballaſt attached to it, of which the reſidual weight is known, and can be allowed for. This immerſion muſt be adjuſted to the upper diviſion of the ſtem. 3. Place a weight in the oppoſite ſcale, equal to one-fifth of that apparent loſs. The inſtrument will riſe, and the ſurface of the water will interſect a point at the diſtance of 2000 diviſions below the firſt. 4. Or if any ſubdiviſion of that fifth part be put into the ſcale, the riſe of the ſtem will be a proportionate part of 2000.---W. N.

Specific Gravity & Strength of ardent Spirit

Fig. 2.

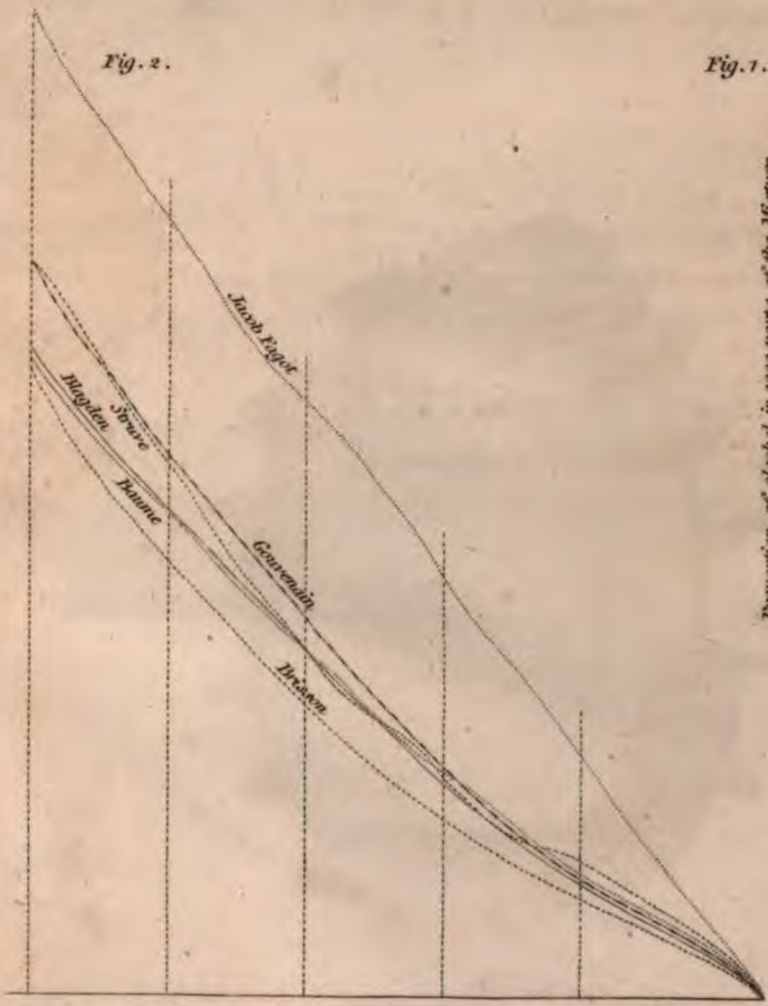
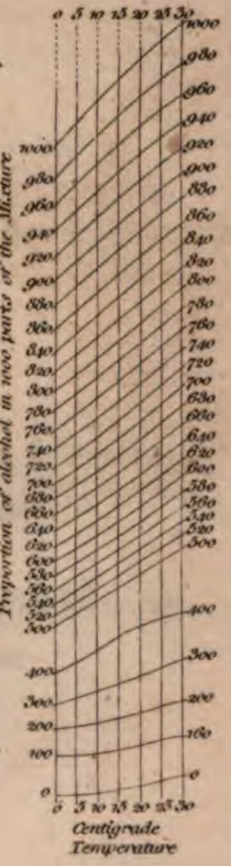


Fig. 1.



Mutlow Sculp.

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Wheel for shewing the Planetary motions.

Fig. 2.

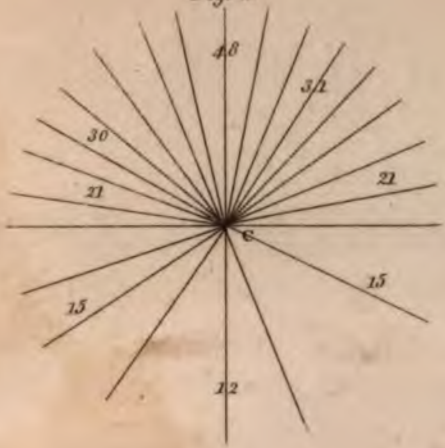


Fig. 1.

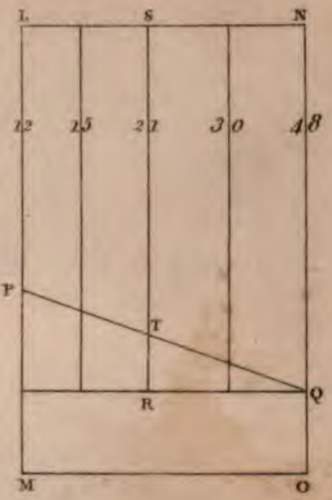


Fig. 3.

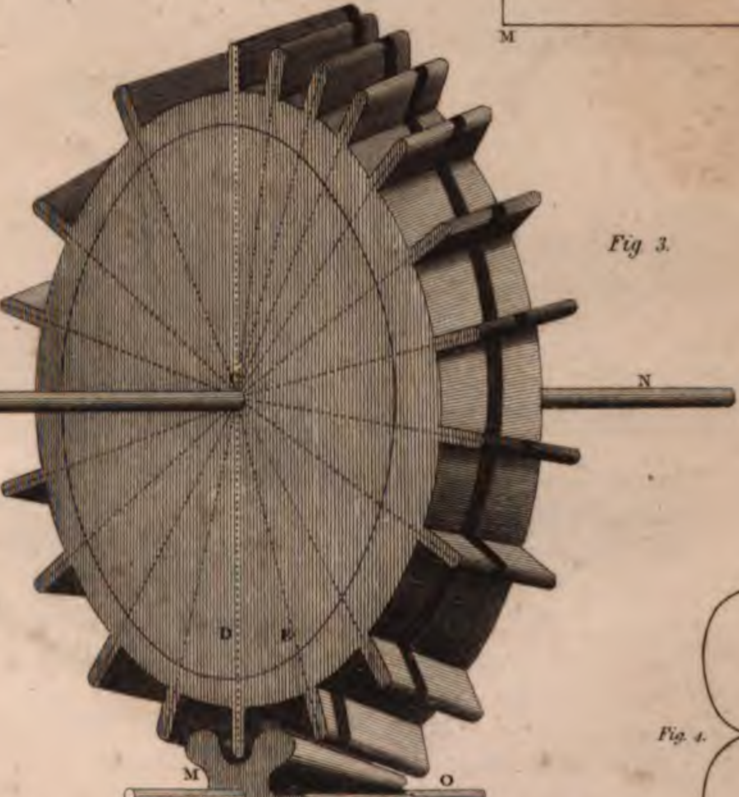


Fig. 5.

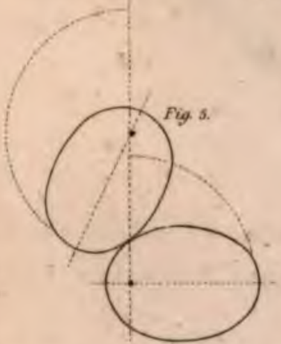
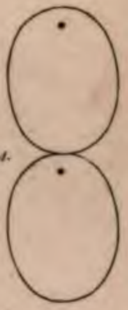


Fig. 4.



Mutlow sculp.

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A
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AND
THE ARTS.

JANUARY 1801.

ARTICLE I.

Conſtruction and Uſe of an univerſal Table of Intereſt. *By* H. GOODWYN, *Esq.*

To MR. NICHOLSON.

SIR,

AS you have inſerted other tables in your valuable periodical work, perhaps the encloſed may be thought worthy your notice.

It is not only applicable to the calculation of intereſt in this, but alſo in all other nations, and probably is as complete and conciſe as the nature of ſuch a performance will admit. It is derived from that principle of prime numbers you have already honoured with publication.

I am,
SIR,
Your moſt humble ſervant,
H. GOODWYN.

Eaſt Smithfield, Dec. 15, 1800.

A Deſcription of the Table.

This table conſiſts of ten principal diviſions.

In the left hand column of the firſt nine diviſions are two cyphers, which in uſe have always the precedence.

The ſecond column from the left of each of the firſt nine diviſions contains at the top a cypher, followed by the nine digits, both arithmetically and perpendicularly arranged. And, either the cypher, or one of the digits, is always to be placed next to and on the left hand of the two cyphers firſt above-mentioned.

At the top of each of the firſt nine principal diviſions is an horizontal line, containing eight circulating figures, which are to be annexed to the two firſt mentioned cyphers, and the other cypher, or digit, next mentioned above, according to ſuch an arrangement as will be hereafter deſcribed.

In the body of each of the firſt nine diviſions, and oppoſite either to the cypher or to one of the nine digits under the arithmetical arrangement before-mentioned, are ten broken horizontal rows of days.

The 1ſt,	or uppermoſt rows, againſt the	cyphers,	contain all the days from	1 to 36
2d,	do.	1	do.	37 to 72
3d,	do.	2	do.	74 to 109
4th,	do.	3	do.	110 to 145
5th,	do.	4	do.	147 to 182
6th,	do.	5	do.	183 to 218
7th,	do.	6	do.	220 to 255
8th,	do.	7	do.	256 to 291
9th,	do.	8	do.	293 to 328
10th,	do.	9	do.	329 to 364

The tenth principal diviſions contains five ſets of days, with the intereſt of 1*l. per cent. per annum*, placed againſt each of them reſpectively.

Rules for applying the Table.

Seek the number of days, for which the intereſt is required, in one of the firſt nine principal diviſions, and theſe will be eaſily found by the arrangement above ſtated.

In the left hand column of the diviſion adjoining the given number of days are two cyphers, with a decimal point on their left.

To theſe two cyphers add, from the next column, the cypher or ſingle digit, which is placed in a line with the days firſt found.

Next to theſe cyphers or figures, and on their right, place the eight circulating figures, which are at the top of the diviſion, beginning with that immediately over the given number of days (and which will be the firſt of the new circulating part of the complete intereſt) and take out the whole eight in the order they ſtand, ending with that immediately on the left hand (which will be the laſt figure of the circulating part.) You will then have obtained the complete intereſt of 1*l.* at 1 *per cent.* for the given number of days.

To find the intereſt at 1 *per cent.* of the decuples of 1*l.* viz. 10*l.* 100*l.* 1000*l.* &c. move the decimal point one, two, three, &c. places more to the right hand, and the complete intereſt of the decuples will be obtained.

The intereſt of 1*l.* to its decuples at 1*l.* *per cent.* for any given number of days having been found, the intereſt of any other ſum, at the ſame rate, may eaſily be derived from it by the rule of practice.

And this amount being obtained, let it be multiplied by any given rate of intereſt, and the product will be the complete intereſt of any given ſum, for any given number of days at the given rate of intereſt.

EXAMPLE I.

Required the intereſt of 1*l.* for 1 day, at the rate of 1 *per cent. per annum.*

In the firſt line of the firſt principal diviſion is one day, and in the left hand column are two cyphers, viz. - -	·00
In the ſecond column from the left, and againſt the one day, is a cypher, viz. - - - -	0
In the line of circulates, immediately over the one day, is a cypher, viz. - - - - -	$\dot{0}$
And this is the firſt circulate of the complete intereſt.	
The other ſeven circulates follow in rotation, thus - -	$273972\dot{6}$
This ſum is the anſwer	£. ·$000\dot{0}273272\dot{6}$

EXAMPLE II.

Required the complete intereſt of 1*l.* at 1 *per cent. per annum* for 364 days.

By the foregoing part or deſcription of the table 364 days muſt be in the tenth, or laſt, column of one of the firſt nine principal diviſions; and accordingly it is found at the bottom of the firſt diviſion.

As before, in the left hand column are two cyphers -	·00
In the ſecond column from the left, and againſt the 364 days, is fig.	9
In the line of circulates immediately over 364 days, is fig.	$\dot{9}$
The remaining circulates are - - -	$726027\dot{3}$
Anſwer	£. ·$009\dot{9}726027\dot{3}$

EXAMPLE III.

Required the intereſt of 10*l.* 100*l.* and 1000*l.* for 1 day, at 1 *per cent.*

The intereſt of 1*l.* at 1 *per cent.* for 1 day, *per* example 1, is £. $\cdot 000\dot{0}273972\dot{6}$

do.	10*l.*	at	do.	$\cdot 000\dot{2}73972\dot{6}$
do.	100*l.*	at	do.	$\cdot 00\dot{2}73972\dot{6}$
do.	1000*l.*	at	do.	$\cdot 0\dot{2}73972\dot{6}$

EXAMPLE IV.

Required the intereſt of 1000000·*l.* for 364 days at the rate of 1*l.* *per cent. per annum.* By examples 2 and 3, applied properly, the anſwer will be $9\dot{9}72\cdot 6027\dot{3}$ = £. 9972·12·0½.

EXAMPLE V.

Required the intereſt of 1000000·*l.* for 247 days at 5*l.* *per cent.*

The intereſt of 1000000·*l.* for 247 days, at 1*l.* *per. cent.* is - - - - £$6\dot{7}67\cdot 1232\dot{8}$

5

Anſwer £. 33835·61643 = £. 33835 12 3

EXAMPLE VI.

Required the intereſt of 1220 at 4*l.* *per cent. per annum* for 263 days.

The intereſt of 1*l.* at 1*l.* *per cent. per annum* for 263 days is $\cdot 007\dot{2}054794\dot{5}$, and of £. 1000· is £. $7\cdot\dot{2}0547$

200 or $\frac{1}{5}$ 1·44109

20 or $\frac{1}{10}$ ·14416

£. 8·79066

4

Anſwer £. 35·1624 = £. 35 3 3

A TABLE

Shewing, by Inſpection, the complete Intereſt of One Pound at the Rate of £. 1 per cent. per annum ; *and by Induction the complete Intereſt of any Sum, at any Rate, and for any given Number of Days, from* 1 *to* 365, *in Decimals of a Pound Sterling.*

	$\dot{0}$	2	7	3	9	7	2	$\dot{0}$
				Days.				
0	1	10	27					22
1				51	72	63	46	
2	74	83	100					95
3				124	145	136	119	
4	147	156	173					168
·00 5				197	218	209	192	
6	220	229	246					241
7				270	291	282	265	
8	293	302	319					314
9				343	364	355	338	

	$\dot{1}$	0	9	5	8	9	0	$\dot{4}$
				Days.				
0	4		35			33		15
1		40		58	69		38	
2	77		108			106		88
3		113		131	142		111	
4	150		181			179		161
·00 5		186		204	215		184	
6	223		254			252		234
7		259		277	288		257	
8	296		327			325		307
9		332		350	361		330	

	$\dot{2}$	4	6	5	7	5	3	$\dot{4}$
				Days.				
0	9	17	24	21				
1					64	56	49	52
2	82	90	97	94				
3					137	129	122	125
4	155	163	170	167				
·00 5					210	202	195	198
6	228	236	243	240				
7					283	275	268	271
8	301	309	316	313				
9					356	341	348	344

	$\dot{0}$	5	4	7	9	4	5	$\dot{2}$
				Days.				
0	2	20		29			19	
1			54		71	53		44
2	75	93		102			92	
3			127		144	126		117
4	148	166		175			165	
·00 5			200		217	199		190
6	221	239		248			238	
7			273		290	272		263
8	294	312		321			311	
9			346		363	345		336

	$\dot{1}$	3	6	9	8	6	3	$\dot{0}$
				Days.				
0	5			36		23	11	
1		50	62		68			37
2	78			109		96	84	
3		123	135		141			110
4	151			182		169	157	
·00 5		196	208		214			183
6	224			255		242	230	
7		269	281		287			256
8	297			328		315	303	
9		342	354		360			329

	$\dot{3}$	2	8	7	6	7	1	$\dot{2}$
				Days.				
0	12		32	28		26		
1		47			61		41	45
2	85		105	101		99		
3		120			134		114	118
4	158		178	174		172		
·00 5		193			207		187	191
6	231		251	247		245		
7		266			280		260	264
8	304		324	320		318		
9		339			353		333	337

	$\dot{0}$	8	2	1	9	1	7	$\dot{8}$
				Days.				
0	3	30	8	7				
1					70	43	65	66
2	76	103	81	80				
3					143	116	138	139
4	149	176	154	153				
·00 5					216	189	211	212
6	222	249	227	226				
7					289	262	284	285
8	295	322	300	299				
9					362	335	357	358

	$\dot{1}$	6	4	3	8	3	5	$\dot{6}$
				Days.				
0	6		16	14		13		
1		60			67		57	59
2	79		89	87		86		
3		133			140		130	132
4	152		162	160		159		
·00 5		206			213		203	205
6	225		235	233		232		
7		279			286		276	278
8	298		308	306		305		
9		352			359		349	351

	$\dot{4}$	9	3	1	5	0	6	$\dot{8}$
				Days.				
0	18	34					25	31
1			48	42	55	39		
2	91	107					98	104
3			121	115	128	112		
4	164	180					171	177
·00 5			194	188	201	185		
6	237	253					244	250
7			267	261	274	251		
8	310	326					317	323
9			340	334	347	331		

Days.	Intereſt.	Days.	Intereſt.	Days.	Intereſt.	Days.	Intereſt.	Days.	Intereſt.
73	£. ·002	146	£. ·004	219	£. ·006	292	£. ·008	365	£. ·01

II.

A Chemical Analyſis of Three Species of Whinſtone, and Two of Lava. By ROBERT KENNEDY, *M. D. F. R. S. & F. A. S. Edin.*

(Concluded from page 415.*)*

ANALYSIS III.

Whin of the Calton Hill, near Edinburgh.

THE rock of this hill varies much in different parts; but its general character is that of porphyry. The piece I choſe for analyſis was taken from the ſouth ſide, about ten or twelve feet below its higheſt point; and was free from calcareous ſpar.

The external characters of this particular piece are as follow: it conſiſts of a greyiſh baſis, containing rhomboidal cryſtals of felſpar of a light reddiſh-brown colour; and ſmall ſpherical maſſes of green earth. The baſis, in its fracture, is uneven, and earthy, and has no luſtre. It can be ſcratched eaſily with a knife, and gives an earthy ſmell when breathed on. The green earth is ſoft, and is affected in ſome degree by water; and being decompoſed by the weather, as well as the veins and nodules of calcareous ſpar, which are very common in this hill, the rock in many places is extremely porous.

The ſpecimen I have deſcribed may be called argillaceous porphyry. It effervefces ſlightly with acids; ſo that the lime which it contains muſt be united to carbonic acid. Its powder is light grey, with a certain ſhade of purple. When heated to redneſs, it becomes of a brown colour. It is not attracted by the magnet, either in its natural ſtate, or after ignition. By being expoſed to a low red heat for half an hour, it loſes 5 *per cent.* of its weight. It ſoftens at 44 of Wedgwood. Its ſpecific gravity is 2.663, as nearly as I could aſcertain, from the effect which the water had in making it crumble down.

One hundred parts contain,

Silex, - - - - - -	50
Argil, - - - - - -	18.50
Oxyde of iron, - - - - -	16.75
Carbonate of Lime, - - - - -	3
Moiſture, and other vol. matter, - - -	5
Soda, about - - - - -	4
Muriatic acid, about - - - -	1
	98.25

ANALYSIS

ANALYSIS IV.

Lava of Catania. Ætna.

This lava, and the ſpecies to be next mentioned, were brought from Mount Ætna by Sir James Hall and Dr. James Home. At their requeſt I analyzed ſpecimens of each. Mineralogiſts are well acquainted with theſe lavas, from the deſcriptions which have been given of them by M. Dolomieu; therefore it is unneceſſary for me to mention their external characters.

The lava of Catania gives a powder of a light grey colour, which is very little changed in appearance by being heated red hot. After it is wet, it becomes dark grey. When this lava is reduced to ſmall fragments, ſome parts of it are attracted by the magnet, and others are not. In fine powder it is but feebly attracted; and after ignition its qualities in this reſpect do not ſeem to be altered. It ſoftens at 33 of Wedgwood. The ſpecific gravity of the pieces moſt free from air bubbles, is 2.795.

I have expoſed this lava, in the manner already deſcribed, to various degrees of heat, from redneſs to 158 of Wedgwood, and conſtantly obſerved that it never loſt the ſmalleſt weight.

When boiled like the whins in muriatic acid, the part remaining undiſſolved weighed 68 *per cent.*

There are in 100 parts,

Silex, - - - - -	51
Argil, - - - - - -	19
Oxyde of iron, - - - - -	14.50
Lime, - - - - - -	9.50
Soda, about - - - - -	4
Muriatic acid, about - - - -	1
	99.

ANALYSIS V.

Lava S^{ta} Venere, Piedimonte, Ætna *.

This ſpecies gives, like the preceding, a greyiſh powder, which becomes dark grey when wet, and its colour is ſcarcely affected by being expoſed to a low red heat. It is but feebly attracted by the magnet, whether in ſmall fragments or in powder; and its qualities in this reſpect are not changed by low ignition.

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* Dolomieu, in deſcribing this lava, ſays, that its fracture is conchoidal, like that of ſilex. The ſpecimen which I analyzed had an uneven fracture; and its colour was blackiſh-blue: in other reſpects, however, it anſwered to Dolomieu's deſcription.

It ſoftens at 32 of Wedgwood, and does not loſe any weight in fires between 150 and 160. After being boiled in muriatic acid, it left 68 parts *per cent.* undiſſolved. Its ſpecific gravity is 2.823.

In 100 parts I found,

Silex, - - - - - - -	50.75
Argil, - - - - - -	17.50
Oxyde of iron, - - - - - -	14.25
Lime, - - - - - - -	10
Soda, about - - - - - -	4
Muriatic acid, about - - - - - -	1
	97.50

In analyzing theſe two lavas, I examined the different ſolutions, with particular attention, for magneſia, and ſulphuric acid; but could not detect any traces of either of theſe ſubſtances.

The reſults of theſe analyſes ſhow, that whins, and a certain claſs of lavas, taken from remote quarters of the globe, conſiſt of the ſame component elements, united in each, nearly in the ſame proportion. The only circumſtance in which they materially differ, is the loſs of ſome volatile matter in the fire, which is peculiar to the whins alone.

We need not be now ſurpriſed at the facts mentioned by Dolomieu, and others, of ſoda being found about volcanos, or upon the ſurface of lavas; as it has thus been ſhown to exiſt in theſe ſubſtances in combination with their earthy baſes.

The facts and experiments I am next to mention will prove, that whins and lavas are not the only ſtones which contain ſoda; and will even render it probable, that this alkali is widely diffuſed through mineral bodies.

Soon after I firſt diſcovered it in whins, and had communicated the circumſtance to Sir James Hall, he ſent me, from a high ſandſtone rock in his eſtate, a quantity of the ſtone decompoſed; and informed me that there was a ſaline eﬄoreſcence mixed with it, which was collected along with the looſe matter, and which ſeemed, by the taſte, to be ſea-ſalt. The place has been long called by the common people the *Salt Heugh*. By ſimply boiling ſome of the decompoſed ſandy part in water, and afterwards filtering and evaporating the water, I obtained regular cryſtals of ſea-ſalt, mixed with a ſmall quantity of ſulphate of ſoda.

Hence it appeared likely, that common ſalt would be found to be one of the component parts of ordinary ſandſtone ſtrata. To verify this important obſervation, I next examined two hard and ſolid ſpecimens, taken from ſome depth below the ſurface, and perfectly free from decompoſition. The firſt was broken from a quarry about two miles to the weſtward of Edinburgh. A portion of it being reduced to minute grains, of ſuch a ſize as the particles of the ſtone ſeemed to have conſiſted of originally, was mixed with ſome diluted nitric acid, and boiled with it gently for three hours. The acid, after being filtered, was examined with nitrate of barytes, with which it produced only a ſlight cloud. But nitrate of ſilver, when poured into it, threw down a copious white precipitate of muriate of ſilver, indicating the preſence of muriatic acid.

After this precipitate was ſeparated by filtration, the liquor which paſſed through was evaporated to dryneſs. A ſaline matter remained, which being mixed with ſome charcoal, and heated, deflagrated like nitrous ſalts. Having waſhed the coaly reſiduum, and evaporated the water, I got ſome perfectly pure carbonate of ſoda. There had been, therefore, in the ſandſtone, ſome common ſalt, which, by this proceſs, was decompoſed, and its acid and alkali collected ſeparately; but whether the whole of the ſalt was obtained, and what proportion it bore to the earthy parts, I cannot determine, as the ſtone itſelf was not analyzed.

The next ſpecimen was taken from a ſtratum of ſandſtone, which lies below the hill of Saliſbury Craig; and I choſe this ſpecies, becauſe the whin to which it is contiguous has already been ſhown to contain ſoda. Some of it being reduced to the ſtate of fine ſand, and treated in every reſpect as the preceding, gave a portion of muriate of ſilver, and of carbonate of ſoda. The exiſtence, therefore, of ſea-ſalt in theſe varieties of ſandſtone, is thus fully eſtabliſhed *.

The celebrated Mr. Klaproth of Berlin has already ſhown, that pot-aſh enters into the compoſition of ſeveral ſtony ſubſtances; and by the experiments deſcribed in this paper, the other fixed alkali, ſoda, has alſo been proved to exiſt in mineral bodies, as it has been ſeparated from nine different varieties; all of which alſo contain a certain quantity of muriatic acid.

As cauſtic fixed alkali was much uſed in theſe analyſes, I ſhall conclude this paper by deſcribing, in a few words, the method by which I prepare it; both becauſe its purity is of the greateſt importance, and becauſe the proceſs I employ differs, in ſome circumſtances, from that of moſt chemiſts. Having obtained an alkali free from all earthy matter, either by burning white tartar, or by repeated ſolutions and cryſtallizations of carbonate of ſoda, I diſſolve it in a conſiderable quantity of water. The requiſite proportion of lime being ſlacked, and allowed to cool, it is diluted with water, and then mixed with the ſolution of the alkali. This mixture is frequently ſtirred during two or three days; for when no heat is applied, the lime requires a certain time to attract the whole of the carbonic acid. In the next place, the mixture is filtered through a piece of linen placed in a funnel, and water poured on it till the whole of the alkali is waſhed out. As it paſſes through, it is poured, at intervals, from the bottle which receives it firſt, into a ſecond that is cloſely ſtopped.

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* Since theſe experiments were performed, I have ſeen ſeveral decompoſed ſandſtones, on the ſurfaces of which there was an efflorescence of common ſalt.

In making the fixed alkalis cauſtic, it is uſual to boil them with the lime; but as moſt kinds of limeſtone contain a ſmall quantity of ſilex or argil, and as theſe earths, when in a ſtate of diviſion, are ſoluble in boiling cauſtic alkali, there is a probability, when heat is applied, of its being thus rendered impure. This is my reaſon for carefully avoiding heat in the firſt part of the proceſs; and I have not found that lime made from chalk, or from the purer kinds of limeſtone *, give any impurity whatever to the alkali, when mixed with it cold.

The ſolution, in its weak ſtate, is firſt evaporated in a baſon of hammered iron, poliſhed; but when it is ſomewhat concentrated, I carry on the evaporation in a diſh made of the pureſt ſilver, reduced from *luna cornea*. After being boiled to a ſmall quantity, it is allowed to cool, and then put into a well-ſtopped bottle for ſome days; during which, if the evaporation has been continued long enough, the neutral ſalts cryſtallize, as Mr. Lowitz has pointed out. Afterwards, the ſolution is carefully decanted from theſe ſalts, and again evaporated in the ſilver diſh, till it acquires the conſiſtence of thin oil †. In this ſtate, ſo little water is preſent, that any part of the alkali, which may be united to carbonic acid, cryſtallizes, and alſo any neutral ſalt that may remain; and the ſolution being decanted a ſecond time, is obtained perfectly pure, colourleſs, and tranſparent.

In the laſt boiling it is ſomewhat difficult to obſerve the exact degree of concentration at which all the alkaline carbonate will cryſtallize; and if the evaporation is carried too far, the cauſtic alkali, if pot-aſh, cryſtallizes itſelf: ſo that ſeveral evaporations are ſometimes requiſite.

When no more water remains in the ſolution than is juſt ſufficient to hold the cauſtic alkali diſſolved, it contains nearly half its weight of alkali; but the exact quantity is eaſily known, by evaporating a portion to dryneſs in a ſilver crucible. Before uſing ſuch a ſolution for analyſes, I aſcertain its purity in the following manner: ſome of it being ſuperſaturated with perfectly pure nitric acid, is examined with nitrate of barytes and of ſilver; with neither of which, if properly made, it will give the ſmalleſt cloud; conſequently it can contain no ſulphuric or muriatic acid.

Another portion being ſaturated exactly with a pure acid, the whole is evaporated to dryneſs, and the ſalt left is rediſſolved in a little water. If any earth were contained in the cauſtic alkali, it would remain thus undiſſolved; but when made as above deſcribed, I have never, in this examination, obſerved the ſmalleſt ſediment.

After the alkali is purified from neutral ſalts, and from the part united to carbonic acid, it may itſelf be cryſtallized by farther evaporation, as Mr. Lowitz has ſhown. But this proceſs ſeems of no uſe in chemical analyſis, as the alkali is previouſly obtained altogether pure.

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* Mr. Klaproth uſes lime made of Carrara marble, which he boils with the alkali. (*Beitrage*, vol. i. preface.)

† Mr. Lowitz, in deſcribing his proceſs for cryſtallizing cauſtic pot-aſh, directs the laſt evaporation to be performed in a glaſs retort. This method is very erroneous; as the alkali, when heated and concentrated, will diſſolve large quantities of the glaſs.

III.

Deſcription and Uſe of a cheap and ſimple Apparatus for teaching the firſt Principles of Mechanics. By RICHARD LOVELL EDGEWORTH, *Eſq. F. R. S. and M. R. I. A.*[*]

WE do not mean to undervalue either the application of ſtrict demonſtration to problems in mechanics, or the exhibition of the moſt accurate machinery in philoſophical lectures; but we wiſh to point out a method of giving a general notion of the mechanical organs to our pupils, which ſhall be immediately obvious to their comprehenſion, and which may ſerve as a ſure foundation for future improvement. When a perſon perceives the effect of his own bodily exertions with different engines, and when he can compare in a rough manner their relative advantages, he is not diſpoſed to reject their aſſiſtance, or to expect more than is reaſonable from their application. The young theoriſt in mechanics thinks he can produce a perpetual motion! When he has been accuſtomed to refer to the plain dictates of common ſenſe and experience, on this, as well as on every other ſubject, he will not eaſily be led aſtray by viſionary theories.

To bring the ſenſe of feeling to our aſſiſtance in teaching the uſes of the mechanic powers, the following apparatus was conſtructed, to which we have given the name Panorganon.

It is compoſed of two principal parts; a frame to contain the moving machinery; and a *capſtan* or *windlaſs*, which is erected on a *ſill* or plank, that is ſunk a few inches into the ground; the frame is by this means and by ſix braces or props rendered ſteady. The croſs-rail, or *tranſom*, is ſtrengthened by braces and a king-poſt to make it lighter and cheaper. The *capſtan* conſiſts of an upright ſhaft, upon which are fixed two *drums*; about which a rope may be wound up, and two levers or arms by which it may be turned round. There is alſo a ſcrew of iron coiled round the lower part of the ſhaft, to ſhew the properties of the ſcrew as a mechanic power. The rope which goes round the *drum* paſſes over one of the pulleys near the top of the frame, and under another pulley near the bottom of the frame. As two *drums* of different ſizes are employed, it is neceſſary to have an upright roller to conduct the rope in a proper direction to the pulleys, when either of the *drums* is uſed. Near the frame, and in the direction in which the rope runs, is laid a platform or road of deal boards, one board in breadth, and twenty or thirty feet long, upon which a ſmall ſledge loaded with different weights may be drawn. Plate XIX. Fig. 1.

F. F. The frame. b. b. Braces to keep the frame ſteady. a. a. a. Angular braces to ſtrengthen the tranſom; and alſo a *king-poſt*. S. A round, taper, ſhaft, ſtrengthened above and below the mortices with iron hoops. L L. Two arms or levers by which the ſhaft, &c. are to be moved round. D D. The drum, which has two rims of different circumferences. R. The roller to conduct the rope. P. The pulley, round which the rope paſſes to the larger drum. P 2. Another pulley to anſwer to the ſmaller drum. P 3. A pulley through which the rope paſſes when experiments are tried with levers, &c. P 4. Another pulley through which the rope paſſes when the ſledge is uſed. Ro. The road of deal boards for the ſledge to move on. Sl. The ſledge with pieces of hard wood attached to it to guide it on the road.

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[*] Extracted by permiſſion from "Practical Education by Maria Edgeworth, and Richard Lovell Edgeworth, F. R. S. and M. R. I. A."

Uſes of the Panorganon.

As this machine is to be moved by the force of men or children, and as their force varies not only with the ſtrength and weight of each individual, but alſo according to the different manner in which that ſtrength or weight is applied, it is, in the firſt place, requiſite to eſtabliſh one determinate mode of applying human force to the machine; and alſo a method of determining the relative force of each individual whoſe ſtrength is applied to it.

To eſtimate the Force with which a Perſon can draw horizontally by a Rope over his Shoulder.

Experiment 1. Hang a common long ſcale-beam (without ſcales or chains) from the top or *tranſom* of the frame, ſo as that one end of it may come within an inch of one ſide or poſt of the machine. Tie a rope to the hook of the ſcale-beam, where the chains of the ſcale are uſually hung, and paſs it through the pulley P 3, which is about four feet from the ground; let the perſon pull this rope from 1 towards 2, turning his back to the machine, and pulling the rope over his ſhoulder, Fig. 6. As the pulley may be either too high or too low to permit the rope to be horizontal, the perſon who pulls it ſhould be placed ten or fifteen feet from the machine, which will leſſen the angular direction of the cord, and the inaccuracy of the experiment. Hang weights to the other end of the ſcale-beam, till the perſon who pulls can but juſt walk forward, pulling fairly without propping his feet againſt any thing. This weight will eſtimate the force with which he can draw horizontally by a rope over his ſhoulder *. Let a child who tries this walk on the board with dry ſhoes; let him afterwards chalk his ſhoes, and afterwards try it with his ſhoes ſoaped: he will find that he can pull with different degrees of force in theſe different circumſtances; but when he tries the following, let his ſhoes be always dry, that his force may be always the ſame.

To ſhew the Power of the three different Sorts of Levers.

Experiment 2. Inſtead of putting the cord that comes from the ſcale-beam, as in the laſt experiment, over the ſhoulder of the boy, hook it to the end 1 of the lever L, Fig. 2. This lever is paſſed through a ſocket, Fig. 3. in which it can be ſhifted from one of its ends towards the other; and can be faſtened at any place by the ſcrew of the ſocket. This ſocket has two gudgeons, upon which it, and the lever which it contains can turn. This ſocket and its gudgeons can be lifted out of the holes in which it plays, between the rail R R, Plate XIX. Fig. 2. and may be put into other holes at R R, Fig. 5. Loop another rope to the other end of this lever, and let the boy pull as before. Perhaps it ſhould be pointed out, that the boy muſt walk in a direction contrary to that in which he walked before; viz. from 1 towards 3. The height to which the weight aſcends, and the diſtance to which the boy advances, ſhould be carefully marked and meaſured; and it will be found, that he can raiſe the weight to the ſame height, advancing through the ſame ſpace as in the former experiment. In this caſe, as both ends of the lever moved through equal ſpaces, the lever only changed the direction of the motion, and added no mechanical power to the direct ſtrength of the boy.

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* Were it thought neceſſary to make theſe experiments perfectly accurate, a ſegment of a pulley, the radius of which is half the length of the ſcale-beam, ſhould be attached to the end of the beam; upon which the cord may apply itſelf, and the pulley (P 3.) ſhould be raiſed or lowered, to bring the rope horizontally from the man's ſhoulder when in the attitude of drawing.

Experiment 3. Shift the lever to its extremity in the *ſocket*; the middle of the lever will be now oppoſite to the pulley, Fig. 4; hook to it the rope that goes through the pulley P 3, and faſten to the other end of the lever the rope by which the boy is to pull. This will be *a lever of the ſecond kind*, as it is called in books of mechanics; in uſing which, *the reſiſtance is placed between the centre of motion or fulcrum, and the moving power.* He will now raiſe double the weight that he did in Experiment II. and he will advance through double the ſpace.

Experiment 4. Shift the lever, and the ſocket which forms the axis, (without ſhifting the lever from the place in which it was in the ſocket in the laſt experiment) to the holes that are prepared for it at R R, Fig. 5. The free end of the lever E will now be oppoſite to the rope, and to the pulley (over which the rope comes from the ſcale-beam). Hook this rope to it, and hook the rope by which the boy pulls to the middle of the lever. The effect will now be different from what it was in the two laſt experiments; the boy will advance only half as far, and will raiſe only half as much weight as before. This is called *a lever of the third ſort.* The firſt and ſecond kinds of levers are uſed in quarrying; and the operations of many tools may be referred to them. The third kind of lever is employed but ſeldom, but its properties may be obſerved with advantage whilſt a long ladder is raiſed, as the man who raiſes it is obliged to exert an increaſing force till the ladder is nearly perpendicular. When this lever is uſed, it is obvious, from what has been ſaid, that the power muſt always paſs through leſs ſpace than the thing which is to be moved; it can never, therefore, be of ſervice in gaining power. But the object of ſome machines is to increaſe velocity, inſtead of obtaining power, as in a ſledge-hammer moved by mill-work. (V. the plates in Emerſon's Mechanics, No. 236.)

The experiments upon levers may be varied at pleaſure, increaſing or diminiſhing the mechanical advantage, ſo as to balance the power and the reſiſtance, to accuſtom the learners to calculate the relation between the power and the effect in different circumſtances; always pointing out, that whatever exceſs there is in the power*, or in the reſiſtance, is always compenſated by the difference of ſpace through which the inferior paſſes.

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* The word *power* is here uſed in a popular ſenſe, to denote the ſtrength or efficacy that is employed to produce an effect by means of any engine.

The experiments which we have mentioned are ſufficiently ſatisfactory to a pupil, as to the immediate relation between the power and the reſiſtance; but the different ſpaces, through which the power and the reſiſtance move when one exceeds the other, cannot be obvious, unleſs they paſs through much larger ſpaces than levers will permit.

Experiment 5. Place the ſledge on the fartheſt end of the wooden road, Fig. 1.; faſten a rope to the ſledge, and conduct it through the loweſt pulley P 4, and through the pulley P 3, ſo as that the boy may be enabled to draw it by the rope paſſed over his ſhoulder. The ſledge muſt now be loaded, till the boy can but juſt advance with ſhort ſteps ſteadily upon the wooden road; this muſt be done with care, as there will be but juſt room for him beſide the rope. He will meet the ſledge exactly on the middle of the road, from which he muſt ſtep aſide to paſs the ſledge. Let the time of this experiment be noted. It is obvious that the boy and the ſledge move with equal velocity, there is therefore no mechanical advantage obtained by the pulleys. The weight that he can draw will be about half a hundred, if he weigh about 9 ſtone; but the exact force with which the boy draws is to be known by Experiment 1.

The Wheel and Axle.

This organ is uſually called in mechanics, *The axis in peritrochio.* A *hard* name, which might well be ſpared, as the word windlaſs or capſtan would convey a more diſtinct idea to our pupils.

Experiment 6. To the largeſt drum, Fig. 1. faſten a cord, and paſs it through the pulley P downwards, and then through the pulley P 4 to the ſledge placed at the end of the wooden road, which is fartheſt from the machine. Let the boy, by a rope faſtened to the extremity of one of the arms of the capſtan, and paſſed over his ſhoulder, draw the capſtan round; he will wind the rope round the drum, and draw the ſledge upon its road. To make the ſledge advance twenty-four feet upon its road, the boy muſt have walked circularly 144 feet, which is ſix times as far, and he will be able to draw about three hundred weight, which is ſix times as much as in the laſt experiment.

It may now be pointed out, that the difference of ſpace, paſſed through by the power in this experiment, is exactly equal to the difference of weight, which the boy could draw without the capſtan.

Experiment 7. Let the rope be now attached to the ſmaller drum; the boy will draw nearly twice as much weight upon the ſledge as before, and will go through double the ſpace.

Experiment 8. Where there are a number of boys, let five or ſix of them, whoſe power of drawing (eſtimated as in Experiment I.) amounts to ſix times as much as the force of the boy at the capſtan, pull at the end of the rope which *was* faſtened to the ſledge; they will balance the force of the boy at the capſtan: either they, or he, by a ſudden pull may advance, but if they pull fairly, there will be no advantage on either part. In this experiment the rope ſhould paſs through the pulley P 3, and ſhould be coiled round the larger drum. And it muſt be alſo obſerved, that in all experiments upon the motion of bodies, in which there is much friction, as where a ſledge is employed, the reſults are never ſo uniform as in other circumſtances.

The Pulley.

Upon the pulley we ſhall ſay little, as it is in every body's hands, and experiments may be tried upon it without any particular apparatus. It ſhould, however, be diſtinctly inculcated, that the power is not increaſed by a fixed pulley. For this purpoſe, a wheel without a rim, or, to ſpeak with more propriety, a number of ſpokes fixed in a nave ſhould be employed, (Fig. 9.) Pieces like the heads of crutches ſhould be fixed at the ends of theſe ſpokes, to receive a piece of girth-web, which is uſed inſtead of a cord, becauſe a cord would be unſteady; and a ſtrap of iron with a hook to it ſhould play upon the center, by which it may at times be ſuſpended, and from which at other times a weight may be hung.

Experiment 9. Let this ſkeleton of a pulley be hung by the iron ſtrap from the tranſom of the frame; faſten a piece of web to one of the radii, and another to the end of the oppoſite radius. If two boys of equal weight pull theſe pieces of girth-web, they will balance each other; or two equal weights hung to theſe webs will be in equilibrio. If a piece of girth-web be put round the uppermoſt radius, two equal weights hung at the ends of it will remain immoveable; but if either of them be pulled, or if a ſmall additional weight be added to either of them, it will deſcend, and the web will apply itſelf ſucceſſively to the aſcending radii, and will detach itſelf from thoſe that are deſcending. If this movement be carefully conſidered, it will be perceived, that the web in unfolding itſelf, acts in the ſame manner upon the radii as two ropes would if they hung to the extremities of the oppoſite radii in ſucceſſion. The two radii which are oppoſite, may be conſidered as a lever of the firſt ſort, where the center is in the middle of the lever; as each end moves through an equal ſpace, there is no mechanical advantage. But if this ſkeleton-pulley be employed as a common *block* or *tackle*, its motions and properties will be entirely different.

Experiment 10. Fig. 9. Nail a piece of girth-web to a poſt, at the diſtance of three or four feet from the ground; faſten the other end of it to one of the radii. Faſten another piece of web to the oppoſite radius, and let a boy hold the ſkeleton-pulley ſuſpended by the web; hook weights to the ſtrap that hangs from the center. The end of the radius to which the fixed girth-web is faſtened will remain immoveable; but, if the boy pulls the web which he holds in his hand upwards, he will be able to lift nearly double the weight, which he can raiſe from the ground by a ſimple rope, without the machine, and he will perceive that his hand moves through twice as great a ſpace as the weight aſcends: he has therefore the mechanical advantage, which he would have by a lever of the ſecond ſort, as in experiment III. Let a piece of web be put round the under radii, let one end of it be nailed to the poſt, and the other be held by the boy, and it will repreſent the application of a rope to a moveable pulley; if its motion be carefully conſidered, it will appear that the radii, as they ſucceſſively apply themſelves to the web, repreſent a ſeries of levers of the ſecond kind. A pulley is nothing more than an infinite number of ſuch levers; the cord at one end of the diameter ſerving as a fulcrum for the *organ* during its progreſs. If this *ſkeleton-pulley* be uſed horizontally inſtead of perpendicularly, the circumſtances which have been mentioned will appear more obvious.

Upon the wooden road lay down a piece of girth-web; nail one end of it to the road; place the pulley upon the web at the other end of the board, and bringing the web over the radii, let the boy, taking hold of it, draw the loaded ſledge faſtened to the hook at the center of the pulley: he will draw nearly twice as much in this manner as he could without the pulley *.

Here the web lying on the road ſhews more diſtinctly, that it is quieſcent where the loweſt radius touches it; and if the radii, as they tread upon it, are obſerved, their points will appear at reſt, whilſt the center of the pulley will go as faſt as the ſledge, and the top of each radius ſucceſſively (and the boy's hand which unfolds the web) will move twice as faſt as the center of the pulley and the ſledge.

If a perſon, holding a ſtick in his hand, obſerves the relative motions of the top, and the middle, and the bottom of the ſtick, whilſt he inclines it, he will ſee that the bottom of the ſtick has no motion on the ground, and that the middle has only half the motion of the top. This property of the pulley has been dwelt upon, becauſe it elucidates the motion of a wheel rolling upon the ground; and it explains a common paradox, which appears at firſt inexplicable, "The bottom of a rolling wheel never moves *upon* the road." This is aſſerted only of a wheel moving over hard ground, which, in fact, may be conſidered rather as laying down its circumference upon the road, than as moving upon it.

The inclined Plane and the Wedge.

The *inclined plane* is to be next conſidered. When a heavy body is to be raiſed, it is often convenient to lay a ſloping artificial road of planks, up which it may be puſhed or drawn. This mechanical power, however, is but of little ſervice without the aſſiſtance of wheels or rollers: we ſhall therefore ſpeak of it as it is applied in another manner, under the name of *the wedge*, which is in fact a moving inclined plane; but if it is required to explain the properties of the inclined plane by the Panorganon, the wooden road may be raiſed and ſet to any inclination that is required, and the ſledge may be drawn upon it as in the former experiments.

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* In all theſe experiments with the ſkeleton-pulley ſomebody muſt keep it in its proper direction; as from its ſtructure, which is contrived for illuſtration, not for practical uſe, it cannot retain its proper ſituation without aſſiſtance.

Let one end of a lever, Fig. 7. with a wheel at one end of it, be hinged to the poſt of the frame, by means of a gudgeon driven or ſcrewed into the poſt. To prevent this lever from deviating ſideways, let a ſlip of wood be connected with it by a nail, which ſhall be faſt in the lever, but which moves freely in a hole in the rail. The other end of this ſlip muſt be faſtened to a ſtake driven into the ground at three or four feet from the lever, at one ſide of it, and towards the end in which the wheel is fixed, Fig. 10. which is a *vue d'oiſeau*, in the ſame manner as the treadle of a common lathe is managed, and as the treadle of a loom is ſometimes guided *.

Experiment 11. Under the wheel of this lever place an inclined plane or half-wedge, Fig. 7. on the wooden road, with rollers under it, to prevent friction †; faſten a rope to the foremoſt end of the wedge, and paſs it through the pulleys (P. 4. and P. 3.) as in the fifth experiment. Let a boy draw the ſledge by this rope over his ſhoulder, and he will find, that as it advances it will raiſe the weight upwards; the wedge is five feet long, and elevated one foot. Now, if the perpendicular aſcent of the weight, and the ſpace through which he advances be compared, he will find that the ſpace through which he has paſſed will be five times as great as that through which the weight has aſcended; and that *this* wedge has enabled him to raiſe five times as much as he could raiſe without it, if his ſtrength were applied, as in Experiment I, without any mechanical advantage. By making this wedge in two parts hinged together, with a graduated piece to keep them aſunder, the wedge may be adjuſted to any given obliquity; and it will be always found, that the mechanical advantage of the wedge may be aſcertained by comparing its perpendicular elevation with its baſe. If the baſe of the wedge is 2, 3, 4, 5, or any other number of times greater than its height, it will enable the boy to raiſe reſpectively 2, 3, 4, or 5 times more weight than he could do in Experiment I, by which his power is eſtimated.

The Screw.

The ſcrew is an inclined plane wound round a cylinder; the height of all its revolutions round the cylinder taken together, compared with the ſpace, through which the power that turns it paſſes, is the meaſure of its *mechanical advantage* ‡. Let the lever, uſed in the laſt experiment, be turned in ſuch a manner as to reach from its gudgeon to the ſhaft of the Panorganon, guided by an attendant lever as before, Fig. 8. Let the wheel reſt upon

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* In a loom this ſecondary lever is called *a lamb*, by miſtake, for *lam*; from *lamina*, a ſlip of wood.

† There ſhould be three rollers uſed; one of them muſt be placed before the ſledge, under which it will eaſily find its place, if the bottom of the ſledge near the foremoſt end is a little ſloped upwards. To retain this foremoſt roller in its place till the ſledge meets it, it ſhould be ſtuck ſlightly on the road with two ſmall bits of wax or pitch.

‡ *Mechanical advantage* is not a proper term, but our language is deficient in proper technical terms. The word *power* is uſed ſo indiſcriminately, that it is ſcarcely poſſible to convey our meaning, without employing it more ſtrictly.

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the loweſt *helix* or thread of the ſcrew; as the arms of the ſhaft are turned round, the wheel will aſcend, and carry up the weight which is faſtened to the lever [*]. As the ſituation of the ſcrew prevents the weight from being ſuſpended exactly from the center of the ſcrew, proper allowance muſt be made for this in eſtimating the force of the ſcrew, or determining the mechanical advantage gained by the lever: this can be done by meaſuring the perpendicular aſcent of the weight, which in all caſes is better, and more expeditious, than meaſuring the parts of a machine, and eſtimating its force by calculation; becauſe the different diameters of ropes, and other ſmall circumſtances, are frequently miſtaken in eſtimates.

The ſpace paſſed through by the moving power, and by that which it moves, are infallible data for eſtimating the powers of engines. Two material ſubjects of experiments yet remain for the Panorganon; friction, and wheels of carriages. We repeat, that it is not intended in this, or in any other part of our deſign, to write treatiſes upon ſcience; but merely to point out methods of initiating young people in the rudiments of knowledge, and of giving them a clear and diſtinct view of thoſe principles upon which they are founded. No preceptor, who has had experience, will cavil at the ſuperficial knowledge of a boy of twelve or thirteen upon theſe ſubjects; he will perceive, that the general view, which we wiſh to give our pupils of the uſeful arts and ſciences, muſt certainly tend to form a taſte for literature and inveſtigation. The *ſcioliſt* has learned only to *talk*—we wiſh to teach our pupils to *think*, upon the various objects of human ſpeculation.

The Panorganon may be employed in trying the reſiſtance of air and water; the force of different muſcles; and in a great variety of amuſing and uſeful experiments. In academies, and private families, it may be erected in the place allotted for amuſement, where it will furniſh entertainment for many a vacant hour. When it has loſt its novelty, the ſhaft may from time to time be taken down, and a ſwing may be ſuſpended in its place. It may be conſtructed at the expence of five or ſix pounds: that which ſtands before our window was made for leſs than three guineas, as we had many of the materials beſide us for other purpoſes.

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[*] In this experiment, the boy ſhould pull as near as poſſible to the ſhaft, within a foot of it, for inſtance, elſe he will have ſuch mechanical advantage as cannot be counterbalanced by any weight which the machine would be ſtrong enough to bear.

IV.

Experiments and Obſervations on the Light which is ſpontaneouſly emitted, with ſome Degree of Permanency, from various Bodies. By Nathaniel Hulme, *M. D. F. R. S. and A. S.*

(Concluded from page 427.)

SECTION IV.

Other Bodies or Subſtances have a Power of preſerving ſpontaneous Light for ſome Time, when it is applied to them.

EXPERIMENTS.

EXPER. 1. Some luminous matter ſcraped from the herring, was mixed with a ſolution of two drams of Epſom ſalt in two ounces of cold pump water: after ſhaking very well for ſome time the phial which contained them, the whole liquid became richly impregnated with light, and continued ſhining above twenty-four hours. This experiment was frequently repeated, and with the ſame effect.

Exper. 2. Two drams of Glauber's ſalt and two ounces of water being mixed with herring light, the ſolution was thereby quickly made very lucent, and remained ſo until the ſucceeding evening.

Exper. 3. Mackerel-light, being mixed with two drams of Rochelle ſalt or tartarized natron and two ounces of water, cauſed the fluid to be very luminous.

Exper. 4. Two drams of ſoda phoſphorata and two ounces of water, mixed with herring-light, formed a very lucent fluid, which retained the light for a long time.

Exper. 5. Herring-light, with one dram of ſaltpetre or nitrated kali and two ounces of water, made the ſolution pretty luminous.

Exper. 6. Half a dram of common ſalt diſſolved in two ounces of water, with the addition of mackerel-light, compoſed a very ſhining mixture, which retained its ſplendour for the ſpace of a day or two. The ſame effect was produced by herring-light.

Exper. 7. Two ounces of ſea water, being agitated with the light of a mackerel, ſoon obtained a brilliant illumination. The ſea water preſerved its luminouſneſs for ſeveral days. The experiment was ſucceſsfully repeated.

Exper. 8. Two drams of pure honey, that had not been clarified, or expoſed to heat, were diſſolved in two ounces of water; and, after the admiſſion of ſome mackerel-light, and ſhaking the phial, the ſolution was fully impregnated with light, which was viſible the next evening.

Exper. 9. Two drams of purified or refined ſugar being diſſolved in two ounces of water, and mixed with the ſhining matter of a herring, the fluid acquired a great degree of lucidneſs. The ſame effect took place when the experiment was made with ſoft brown ſugar.

N. B. It is almoſt needleſs to mention, that the degree of illumination in theſe liquids muſt depend upon the quantity of lucific matter applied; but, in general, as much as can be ſcraped off by the blunt point of a moderately-ſized knife, at a few times, will be ſufficient, being aſſiſted by a ſtrong agitation of the containing phial.

OBSERVATION.

Theſe experiments enable us to take light and diffuſe it through water, ſo as to render the whole liquid moſt brilliantly luminous, or, in other words, to impregnate water with light. By theſe means, the light is ſo extended in its ſurface, and combined in ſuch a manner, as to become exceedingly convenient and uſeful for various other experiments.

SECTION V.

When ſpontaneous Light is extinguiſhed by ſome Bodies or Subſtances, it is not loſt, but may be again revived in its former Splendour, and that by the moſt ſimple Means.

EXPERIMENTS.

Exper. 1. On the 1ſt of June, 1795, the following experiments were made, to know what was the beſt proportion of Epſom ſalt to water, in order to produce the moſt luminous liquid. Some ſhining matter was taken from a mackerel, and mixed with a ſolution of ſeven drams of the ſalt in one ounce of water; and its light was immediately extinguiſhed. The ſame effect enſued, but in a leſs degree, with a ſolution of ſix, and one of five drams. In a ſolution of two drams, in the ſame quantity of water, the liquid was luminous; but much more ſo when only one dram of ſalt was uſed. Obſerving the extinction of light to take place, as above, in the more ſaturated ſolutions, while the diluted ſolutions were luminous, it occurred to me to endeavour to diſcover what became of the extinguiſhed light, in the former caſe, and whether it might not be revived by dilution. For this purpoſe, I took the ſolution of ſeven drams of ſalt in one ounce of water, in which the lucid matter from a mackerel had been extinguiſhed, and diluted it with ſix ounces of cold pump water; when, to my great aſtoniſhment, light in a moment burſt out of darkneſs, and the whole liquid became beautifully luminous! This revived light remained above 48 hours, that is, as long as other light in general does, which has never been extinguiſhed. Hence, it had loſt nothing of its vivid luminous powers by its extinction.

Exper. 2. The laſt experiment was then reverſed. A ſolution of one dram of Epſom ſalt in one ounce of water, was brilliantly illuminated with mackerel light. Then, ſix drams of the ſalt were put into this luminous liquid; and, after ſhaking the phial very well for a little time, to promote the ſolution of the ſalt, the light was totally extinguiſhed. But the ſame light was again recovered, by the addition of ſix ounces of water.

In this manner the light may be frequently extinguiſhed, and as often revived. In one inſtance, the ſame light, by a repetition of this method, was made to undergo ten extinctions.

Exper. 3. A good quantity of herring-light, being mixed with a ſolution of four drams of common ſalt in two ounces of water, was immediately extinguiſhed. Then, fourteen ounces of cold pump water were added thereto, and the whole liquid was at once finely illuminated. On the next evening it appeared ſtill very lucid; and likewiſe on the ſucceeding night.

Exper. 4. The experiment was reverſed. Half a dram of the ſalt, being diſſolved in two ounces of water, had herring-light mixed therewith, ſo as to be made very luminous. On the addition of two drams more of the ſalt, the lucidneſs wrs inſtantly deſtroyed: but the light was again recovered, by pouring eight ounces of cold water upon the extinguiſhed luminous fluid. The revived light was very vivid the next evening.

Exper. 5. Two ounces of ſea water were illuminated with mackerel-light, and then extinguiſhed by adding two drams of common ſalt. The light was again reſtored, by diluting the ſolution with eight ounces of cold ſpring water.

N. B. If the illuminated liquid be uncommonly brilliant, it may ſometimes require more ſalt to extinguiſh the light completely, than is here ſpecified; in that caſe, the meaſure of water for dilution, muſt be always calculated in exact proportion to the weight of ſalt employed.

SECTION VI.

Spontaneous Light is rendered more vivid by Motion.

EXPERIMENTS.

Exper. 1. A quantity of illuminated liquid was poured into a broad veſſel, which was placed in the laboratory. The next evening, on examination, it appeared to be quite dark. But a finger, or rod, being drawn through it, was followed by a luminous line.

Exper. 2. A phial, containing a pretty large portion of liquid impregnated with light, having been at reſt a number of hours, the liquid ſeemed to have loſt its luminous quality, except a little glimmer floating at the top. It was then gently moved, and the light diffuſed itſelf gradually through the whole liquid: on agitation, the lucidneſs was much increaſed; and, the briſker the motion, the more vivid was the illumination.

SECTION VII.

Spontaneous Light is not accompanied with any Degree of ſenſible Heat, to be diſcovered by a Thermometer.

EXPERIMENTS.

Exper. 1. A luminous herring, and another that was quite freſh and not luminous, were placed for a conſiderable time in the ſame degree of temperature. A thermometer was then applied to each of them, but no difference of heat could be diſcovered.

Exper. 2. The ſoft roe of a herring, in an exceedingly lucid ſtate, and a thermometer, were kept together for ſome time in the laboratory. The roe was then put upon the bulb of the thermometer, without affecting it.

Exper. 3. A mackerel, which ſhone with very brilliant light, was alſo put to the teſt of a thermometer, but the inſtrument remained ſtationary.

Exper. 4. The bulb of a thermometer was ſurrounded by many ſmall pieces of ſhining wood, uncommonly luminous, which were kept in that ſituation for ſome time; but the light made no alteration upon the thermometer.

Exper. 5. Illuminated liquids, and ſpring water, being kept together in the laboratory, always preſerved the ſame degree of temperature.

SECTION VIII.

The Effects of Cold on ſpontaneous Light.

EXPERIMENTS.

The Light of Fiſhes.

Exper. 1. Five ſmall gallipots, containing three pieces of ſoft-roe of herring, and two of the herring itſelf, all very luminous, were placed in a frigorific mixture, compoſed of ſnow and ſea-ſalt; and, in about an hour and a half, the light was quite extinct, and the bodies totally frozen. The gallipots were then removed into a veſſel of cold water, that their contents might be gradually thawed; which being done, they all recovered their priſtine luminous ſtate. The pieces were afterwards obſerved to ſhine during three ſucceeding nights.

Exper. 2. A ſmall phial, containing three or four drams of liquid impregnated with light, was placed in a frigorific mixture. As the liquid froze, its lucidneſs gradually diminiſhed; and, when it was quite congealed, the light perfectly diſappeared. The phial was then taken out, and put into cold water, at about 49° temperature, that the ice might be gradually liquefied; and, when that was accompliſhed, the whole fluid became as luminous as before.

The

The Light of ſhining Wood.

Exper. 3. A fragment of ſhining wood was put into a ſmall wide-mouthed phial, which was plunged into a frigorific mixture. As the cold affected the wood, the light gradually faded, and at laſt was totally imperceptible. The phial was then taken out, and placed in water at about 62°; by this change of temperature, the frozen wood gradually thawed, and then regained its former luſtre.

The Light of Glow-worms.

Exper. 4. A ſmall phial, containing a luminous dead glow-worm, was expoſed to the cold of the frigorific mixture; as the coldneſs penetrated the phial, the light diminiſhed, and at length was totally extinct. But, by placing the phial in water at about 62°, the glowing property of the inſect ſoon returned. In this experiment, the glow-worm was evidently congealed; for it adhered to the ſide of the glaſs, and was covered with a hoar-froſt. This experiment was frequently repeated, and with the ſame reſult.

OBSERVATION.

By theſe experiments we learn, that cold extinguiſhes ſpontaneous light in a temporary manner, but not durably, as the ſubſtances of the third ſection do; becauſe the light revived again in its full ſplendour, as ſoon as it was expoſed to a moderate degree of temperature.

SECTION IX.

The Effects of Heat on ſpontaneous Light.

EXPERIMENTS.

The Light of Fiſhes.

Exper. 1. One ſide of a luminous herring was held before the fire, for a ſhort ſpace of time, but ſo as to receive its heat very ſtrongly. It was then conveyed into the laboratory; when that ſide which had been expoſed to the fire was found quite dark, but the other continued ſtill luminous. The fiſh was preſerved till the next evening, but the extinguiſhed light did not re-appear.

Exper. 2. A whole herring, finely ſhining, was thrown into a quantity of boiling-hot water, and the light was immediately extinguiſhed: after keeping it there for ſome time, it was taken out, but the light did not revive.

The

The Light of ſhining Wood.

Exper. 3. A piece of ſhining wood, its light being very faint, was put into tepid water at about 90 degrees of temperature, and it became in a ſhort time much more lucid. Another piece, at 96°, was rendered beautifully luminous.

Exper. 4. A pretty thick piece of ſhining wood was put into a gallipot, and ſunk under water by means of a weight, together with a thermometer, at the temperature of 64°. Boiling-hot water was then added by ſpoonfuls; and the light, at firſt, was rendered much more vivid, but ſoon after began to decreaſe, and was apparently extinct at about 110°. I ſay apparently, becauſe on the next evening the light had ſomewhat revived; which ſhows, that the heat of 110° was not ſufficient to extinguiſh totally all the light inherent in this piece of wood.

Exper. 5. Finding that 110 degrees of heat did not wholly extinguiſh the light of ſhining wood, a good many fragments, of different ſizes, were then ſubmitted to the power of boiling water, and detained therein for ſome time, in order that the heat might penetrate them thoroughly. The effect was, that the light became quickly extinct, and did not, as before, re-appear on the following evening.

The Light of Glow-worms.

Exper. 6. A dead ſhining glow-worm was put upon two ounces of water, contained in a wide-mouthed phial, at the temperature of 58°. The phial was then ſunk, about two or three inches deep, in boiling-hot water; and, as the heat communicated itſelf to the contents of the phial, the light of the glow-worm became much more vivid.

Exper. 7. Another lucid dead glow-worm was put into warm water, at 114°, to ſee if that degree of heat would extinguiſh the light; but, on the contrary, its glowing property was augmented. All the water was then poured off, yet the inſect continued to ſhine for ſome length of time.

Exper. 8. The effect of that heat which is obtained from dry ſolid bodies by friction, was next tried upon the light of the glow-worm. Two living glow-worms were put into a one-ounce phial, with a glaſs ſtopple; and, though they were perfectly dark at the time, yet, if the phial was briſkly rubbed with a ſilken or linen handkerchief, till it became pretty warm, it ſeldom failed to make them diſplay their light very finely. This experiment was very frequently repeated. It had the ſame illuminating effect upon the light of a dead glow-worm.

Exper. 9. The complete influence of 212 degrees of heat was now applied to the light of a glow-worm, by pouring upon one when dead, but in a luminous ſtate, ſome boiling water. Its light was inſtantly extinguiſhed thereby, and did not revive. The experiment was repeated, and with the ſame reſult.

Any of the ſaline Solutions mentioned in the fourth Section, being impregnated with luminous Matter, and left ſome Time at reſt, are rendered more lucid by a moderate *Degree of Heat.*

Exper. 10. A quantity of illuminated ſolution was depoſited in the laboratory. The next evening, when it was examined, it appeared in a manner quite dark; but, by putting the phial which contained it into hot water, the light revived, and was ſoon rendered exceedingly vivid.

Exper. 11. About a pint of ſolution impregnated with light, had become obſcure, by time and reſt, as is the nature of this mixture. Such a quantity of boiling-hot water was then added to it, as only to give it a ſmall degree of warmth, and it quickly cauſed it to appear luminous.

Exper. 12. Illuminated liquid, to the quantity of four ounces, was placed in the laboratory until the next evening, when it had become almoſt dark. One ſpoonful of boiling-hot water being put into it, the light re-appeared; and, by means of two more, it was rendered conſiderably lucid.

Their Light is extinguiſhed by a great *Degree of Heat.*

Exper. 13. Some boiling water being poured upon three or four ounces of illuminated liquid, in an earthen veſſel, the light was immediately extinguiſhed; and, though afterwards kept a conſiderable time for inſpection, and often agitated, to ſtir up the hidden light, yet no remains of any ſhining property could be perceived. This experiment was frequently repeated, and always with the ſame reſult.

Exper. 14. Four ounces of very luminous liquid, together with a thermometer, were put into a ſmall earthen veſſel, glazed white, the better to reflect light. Boiling-hot water was then added, by ſpoonfuls at a time, and by ſlow degrees. The firſt few ſpoonfuls made it conſiderably more lucid; and then, by adding more, the light began to fade, and at length was gradually extinguiſhed. This effect took place, in one inſtance, when the liquid was heated to 96°; in another, to 98°; and in a third, to 100°. Hence, this ſpecies of light, when thus united with water, ſeems to be extinguiſhed at from 96 to 100 degrees of heat. This is a very elegant and pleaſing method, of knowing how much heat is required to extinguiſh the light; becauſe it meaſures it exactly, provided the hot water be added in ſmall quantities, and by ſlow degrees, as above directed. To prevent the poſſibility of any light reviving after an experiment of this kind, would require a much greater heat than that of 100°. The intention of the preſent experiment was only to ſhow, that all light may be apparently extinguiſhed, at ſo low a degree of temperature as from 96° to 100°.

Exper. 15. A phial of an ounce and a half was filled with ſome very luminous liquid, but not corked. It was then ſuſpended by a ſtring, in a quart of boiling-hot water contained in a white earthen mug, and the light was wholly extinguiſhed in about three or four minutes. After this, the phial was kept in the water ſome time longer, was then taken out to cool, and well ſhaken, but the light did not revive. It was examined the next day, and agitated again, but no luminous appearance could be diſcovered; a proof that all the light had been totally extinguiſhed by the power of heat.

If much Heat be applied to the Bottom of a Tube filled with illuminated Liquid, which has been ſome Time at reſt, the Light will deſcend in luminous Streams, from the Top of the Tube to the Bottom, and be gradually extinguiſhed.

Exper. 16. A glaſs cylindrical tube, cloſed at one end, being 9 inches long, with a bore of $1\frac{1}{10}$ inch, when uſed, was put into a gallipot $3\frac{1}{2}$ inches deep, and $3\frac{1}{2}$ wide, which held about 12 ounces of boiling water, and was placed in another larger veſſel, to receive the overflowing water upon the immerſion of the tube. The tube being filled over night with ſome very luminous liquid, was placed in the laboratory until the next evening. The light had then aſcended plentifully to the top of the fluid, (the reſt being dark,) and, taking the circular ſhape of the tube, formed a very lucid ring. The veſſels with the boiling-hot water were then carried into the dark laboratory; and the tube being gently and carefully placed (without ſhaking) in the gallipot, the light was, generally in about half a minute, ſeen plainly to deſcend in ſtreams from the top to the bottom; illuminating the whole fluid in its deſcent in a beautiful manner, and then was gradually extinguiſhed. The extinction of the light began at the top of the tube, and ended at the bottom.

Exper. 17. The experiment was alſo made with a tube 19 inches high, $\frac{1}{2}$ an inch in bore, having ſeveral curvatures, and ſealed hermetically at its lower end. Both extremities were made ſtraight for a few inches; the one to be immerſed in the water, and the other to prevent the liquid running out. The luminous ring being formed as above mentioned, the tube was put into the gallipot of boiling-hot water; and, in a ſhort time, the light began to deſcend from the top, and came waving down, in a pleaſing manner, to the bottom of the tube in the hot water, and then was by degrees extinguiſhed. The whole length of the tube, including the curvatures. was 26 inches.

The moſt eligible ſolutions for this curious experiment, are thoſe made with Epſom ſalt, Glauber's ſalt, ſea-ſalt, and ſal ammoniac: if either of the two former be uſed, the proper proportion is, one dram of ſalt to each ounce of water; if either of the two latter, 15 grains to each ounce of water will be ſufficient.

N. B. The experimentaliſt, before he views the deſcent of the light in the tube, ſhould always remain in the dark for ſome little time, in order to get rid of all extraneous light adhering to the organs of viſion, and to accommodate the eye to darkneſs.

SECTION X.

The Effects of the human Body, and of the animal Fluids, upon ſpontaneous Light.

The living Body.

Exper. 1. On touching the luminous matter of fiſhes, the light adhered to the fingers and different parts of the hands; remained very lucid for ſome little time, and then gradually diſappeared. But the ſame kind of matter being applied to pieces of wood, ſtone, and the like, of the ſame temperature as the laboratory, continued luminous on theſe ſubſtances for many hours.

Exper. 2. A piece of red blotting-paper, about one inch ſquare, and four times doubled, was finely illuminated by matter from a herring, and applied to the upper part of the inſide of the thigh. After the expiration of 15 or 20 minutes, it was taken off; and, on examination, the light was quite extinguiſhed. The experiment was repeated ſeveral times, and with the ſame effect. Another piece of the like paper was illuminated at the ſame time, and placed in the laboratory; where it retained its light above 48 hours.

Exper. 3. A piece of ſhining wood was placed upon the palm of the hand, and incloſed therein for ſome time; on inſpection, it was found to be more lucid than before. Many trials of this kind were made, with the like ſucceſs.

Exper. 4. A dead glow-worm, being but ſlightly luminous, was breathed upon ſeveral times; and its light increaſed both in magnitude and brightneſs. The experiment was frequently repeated, with the ſame reſult.

Animal Fluids.

Blood.

Exper. 5. A perſon having received a contuſion, but otherwiſe in health, was bled. The next day, ſome herring-light was mixed with about two ounces of the craſſamentum or red coagulated part of the blood, by ſtirring them well together with a knife: it cauſed it to be ſlightly luminous, but the light was not of long duration. Nearly the ſame reſult followed the mixture of lucid matter with the recent craſſamentum of perſons labouring under inflammatory diſeaſes, as the pleuriſy and rheumatiſm.

Exper. 6. But, when mixed with craſſamentum that had been kept for ſome time, and become black and ſomewhat offenſive to the ſmell, the light ſeemed to be more quickly extinguiſhed.

Exper. 7. A ſingular phænomenon happened ſeveral times, on mixing fiſh-light with putreſcent bloody ſerum. It would not incorporate, but was ejected in globules, like quickſilver when rubbed with any unctuous ſubſtance, and afterwards adhered to the ſide of the veſſel in which the mixture was made, in the form of a lucid ring.

Exper. 8. The luminous matter of a herring was mixed with about two ounces of pure ſerum, from the healthy ſubject of the 5th experiment: it ſoon became finely illuminated, and retained its ſhining appearance for a long time, whenever it was ſtirred or agitated.

Exper. 9. The recent ſerum, drawn from patients afflicted with inflammatory complaints, was illuminated pretty much in the ſame manner as in the 8th experiment; and often retained light above 48 hours.

Urine.

Exper. 10. Mackerel-light being mixed, by ſtrong agitation, with ſome freſh urine from a healthy perſon, a glimpſe of light was retained at firſt, and then was gradually extinguiſhed. But ſtale and pungent urine, being incorporated with luminous matter, had ſtill greater extinguiſhing effect.

Bile.

Exper. 11. Some bile, taken from a perſon who died of a ſuppreſſion of urine, had herring-light mixed with it, which ſoon became extinct. Another trial was made, with a different bile, and with the ſame reſult.

Milk.

Exper. 12. Human milk not being eaſily obtained, ſome mackerel-light was incorporated, by agitation, with two ounces of freſh cow's milk, which was thereby rendered finely luminous, and continued ſhining above 24 hours. Freſh cream alſo retained ſome light; though it was not ſo viſible as with milk, owing probably to its thickneſs. But, when either milk or cream turn ſour, they contract a very extinguiſhing property. A quart of milk was kept five days, in a moderately cool place, in the month of June; by that time, it was changed into a mixture ſomewhat reſembling curds and whey, that is, into a ſmall ſmooth coagulated part, and a very thin one, both which were acidulous. Some fine mackerel-light was mixed with two ounces of each of them, in ſeparate phials, and they extinguiſhed it immediately.

V.

Obſervations on the Theory of Electric Attraction and Repulſion. By the Rev. GEORGE MILLER, *F. T. C. D.*[*]

BEFORE that the theory of a ſingle electric fluid was propoſed, no difficulty occurred in the explanation of the attractions and repulſions obſerved to ariſe from electricity. If we admit that there are two diſtinct electric fluids, each of which ſtrongly attracts the other, but conſiſts of particles mutually repulſive; it becomes eaſy to account for the attraction ſubſiſting between bodies in different ſtates of electricity, and the repulſion between thoſe in the ſame. But when Dr. Franklin [†], obſerving that a man, ſtanding upon a non-conductor, could not electrify himſelf, but that he could electrify another perſon alſo ſtanding upon a non-conductor, was induced to regard the operation of exciting electricity only as a transfer of one and the ſame fluid from one body to another; it was found to be difficult to reconcile to the new theory the mutual repulſion of bodies in that ſtate which is, according to this theory, denominated negative electricity. Dr. Franklin [‡] acknowledged that he could not aſſign a ſatisfactory reaſon for it; and Dr. Prieſtley [§] has propoſed it, as one of the queries remaining to be ſolved for completing the ſcience of electricity. Many attempts have been made to obviate this apparent objection to the ſimple theory of a ſingle fluid; but the difficulty ſeems ſtill to be as great as it was in the time of Franklin.

[‖] Æpinus has applied a very elaborate ſyſtem of mathematical reaſoning to the ſolution of electrical phænomena, and has adopted as the baſis of his theory, the ſame opinion which Franklin had entertained concerning the nature of the electric fluid; but he has combined with this opinion other principles ſo inadmiſſible, that his reaſonings cannot be regarded as juſt explications of the phænomena. He has aſſumed, apparently without any other reaſon than its importance to his concluſion, that the particles of all other ſubſtances repel each other. His ſyſtem muſt therefore be conſidered, not as a phyſical ſolution agreeable to the known laws of natural operations, but merely as an ingenious exerciſe of mathematical ability.

M. De Luc, who rejected the ſolutions of Æpinus has endeavoured to ſupply the deficiency. [**] Having remarked that the divergence of the balls of an electrometer, included in the receiver of an air-pump, is continually diminiſhed during the progreſs of exhauſtion; he conſiders it as proved, that the cauſe of all electrical movements, whether of attraction or of repulſion, is the action of the air. This principle he applies in the following manner. When two bodies are in ſimilar ſtates of electricity, either poſitive or negative, they will conſpire to modify, either by giving or receiving the electric fluid, the ſtate of the intermediate air, whilſt that of the exterior air is only modified by either of them ſingly; and therefore the ſtate of the exterior air will differ more from that of the electrified bodies, than the ſtate of the intermediate air. In this caſe he contends that a repulſion muſt take place, becauſe each body muſt move towards that part of the ſurrounding medium, whoſe electrical ſtate is moſt different from its own. On the other hand, when bodies are in different ſtates of electricity, they will mutually counteract the changes, which they might ſeparately produce in the ſtate of the intermediate air; but each will operate on the exterior air without any compenſation, In this caſe the ſtate of the intermediate air will continue to differ from that of each body as much as at the firſt inſtant, whilſt the ſtate of the exterior air is ſeparately modified by each body according to its reſpective ſtate of electricity. The two bodies therefore, moving towards that part of the ſurrounding medium, whoſe electrical ſtate is moſt different from their own, will at the ſame time move towards each other.

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[*] Iriſh Tranſactions, VII. 139.

[†] Dr. Prieſtley's Hiſtory of Electricity, p. 161.

[‡] Dr. Prieſtley's Hiſtory of Electricity, p. 165.

[§] Ibid. p. 492.

[‖] Journal de Phyſique, Dec. 1787.

[**] Ibid. Juin 1790.

This theory very ingeniouſly avoids the difficulty of explaining the caſe of electrical repulſion, by reſolving it into an attraction towards the ſurrounding medium. It ſeems however to be liable to two objections. In the firſt place, inſtead of aſſuming unauthorized principles with the preceding theory, it omits the conſideration of one whoſe exiſtence ſeems to be aſcertained by experiments. If a body be in either ſtate of electricity, it will induce in an adjacent body the contrary ſtate, until it ſhall have come within a certain diſtance. This property, which has been aſcertained by various experiments, indicates a repulſive force ſubſiſting between the portions of the electric fluid that belong to the adjacent bodies; and this theory makes no allowance for ſuch a repulſion. The fundamental principle of it is merely a diffuſion of the electric fluid, and is[*] thus ſtated by M. de Luc: "the electric matter tends towards all ſubſtances, and the more ſtrongly in the ſame pro"portion in which they poſſeſs a ſmaller quantity." In the ſecond place, it does not appear, when carefully conſidered, to afford any aſſiſtance towards the removal of the grand difficulty, the mutual repulſion of bodies negatively electrified. If two bodies negatively electrified be placed at a ſmall diſtance, they will both, according to M. de Luc's explanation, receive the electric fluid from the intermediate air, which will conſequently retain a ſmaller portion than the ſurrounding atmoſphere. From the law above-mentioned it ſhould follow, that the redundant fluid of the exterior air ſhould by diffuſion be communicated both to the bodies and to the intermediate ſpace; but no reaſon appears, which would induce us to ſuppoſe that the bodies themſelves ſhould recede to a greater diſtance. M. de Luc does indeed endeavour to prove that ſuch a motion ſhould take place, but by an experiment whoſe ſolution contradicts his own theory. He ſuſpended by a ſilk thread a large, but light, metallic ball, and preſented it in a ſtate of poſitive electricity to a body negatively electrified. The former was attracted towards the latter until it arrived at a certain diſtance, at which it diſcharged its electricity. Hence he concluded, in general, that when a body has more of the electric fluid than the neighbouring bodies, and is leſs diſpoſed to reſiſt its own motion than to abandon the exceſs of its electric matter, it will move towards that place which contains leſs of this matter. But in this experiment he conſiders the two bodies as acting on each other at a diſtance without any reference to the intermediate air.

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[*] "La loi ſuivante ſuffit ſeule: La matière électrique tend vers toutes les ſubſtances, d'autant plus "fortement, qu'elles en poſſèdent moins." Journal de Phyſique, Juin 1790.

Mr. Cavallo *, in the laſt edition of his treatiſe on electricity, has obſerved, that the mutual repulſion of two bodies negatively electrified is ſtill ſuppoſed to contradict the theory of Franklin; and has therefore deemed it neceſſary to obviate the objection by a very particular detail. For this purpoſe he has premiſed the following propoſitions: Prop. 1. No electricity can appear on the ſurface of a body, or no body can be electrified either poſitively or negatively, unleſs the contrary electricity can take place on other bodies contiguous to it. Prop. 2. There is ſomething on the ſurface of bodies, which prevents the ſudden incorporation of the two electricities, viz. of that poſſeſſed by the electrified body with the contrary electricity poſſeſſed by the contiguous air, or other ſurrounding bodies. Prop. 3. Suppoſing that every particle of a fluid has an attraction towards every particle of a ſolid; if the ſolid be left at liberty in a certain quantity of that fluid, it will be attracted towards the common centre of attraction of all the particles of the fluid. To this laſt propoſition he has ſubjoined the two following corollaries: 1. † the ſame thing muſt happen, when the quantity of fluid is ſmaller than the bulk of the body; 2. if the attraction of the particles of the fluid be exerted only towards the ſurface of the ſolid, the effect will be the ſame when the body is of a regular ſhape; but the difference will in any caſe be inconſiderable.

With regard to the ſolution founded upon theſe principles it muſt be remarked, that it is not derived ſimply from a conſideration of the ſuppoſed nature of the electric fluid; but from a mixed ſtatement of that nature and of properties aſſumed merely from experiments as matters of fact. The firſt and ſecond propoſitions expreſs thoſe properties, and, though the experiments to which the former refers, may be explained by aſcribing the phænomena to the repulſive nature of the fluid, yet the latter is aſſumed without any ſuch reference. "Without examining," ſays Mr. Cavallo, "the nature, the extent, and the laws of this "property in bodies, it will be ſufficient for the preſent purpoſe to obſerve, that the fact "is certainly ſo; for otherwiſe a body could not poſſibly be electrified, or it would not "remain electrified for a ſingle moment." From theſe principles thus aſſumed, Mr. Cavallo deduces the exiſtence of atmoſpheres of contrary electricity exiſting in the air contiguous to the bodies; and from the attractions which are thereby occaſioned he infers the apparent repulſion of the electrified bodies.

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* Vol. III. p. 192.

† Of this corollary Mr. Cavallo does not appear to make any diſtinct application.

If theſe atmoſpheres be conceived to be formed by the repulſive nature of the fluid, ſome allowance ſhould be made for the mutual repulſion of the two redundant portions belonging to bodies poſitively electrified. This however ſeems to be neglected for the purpoſe of explaining the repulſion of bodies negatively electrified. But the difficulty ſeems to be only changed. If the negative atmoſphere adjacent to a body poſitively electrified be cauſed by the repulſion of the redundant fluid of the body, it will be neceſſary to ſhew that this repulſion is overpowered by the attraction ſubſiſting between that redundant fluid and the portion of air thus deprived of a part of its electric fluid.

But the reality of theſe atmoſpheres of contrary electricity may well be queſtioned. It ſeems to require, that we ſhould conceive a portion of air contiguous to each body to be permanently, during the mutual repulſion of the bodies in a ſtate of electricity oppoſite to that of the bodies. But * it is aſcertained experimentally, that the air ſurrounding any electrified body acquires the ſame electricity which had been poſſeſſed by the body, and retains it even after the removal of the body. This muſt be ſuppoſed, agreeably to the known laws of electricity, to be communicated by the alternate attraction and repulſion of the adjacent particles of air. Each particle muſt be firſt attracted towards the body, and, when by contact it has acquired the electricity of the body, repelled from it. Inſtead therefore of a permanent ſtate of contrary electricity conſtituting theſe ſuppoſed atmoſpheres, each adjacent ſpace muſt be occupied by particles, ſome of which are attracted and others repelled. The time requiſite for thus reducing the electricity of the body to an equilibrium with that of the ſurrounding air, is ſufficient for explaining the continuance of the electricity of the bodies, without the aid of the ſecond propoſition; and the firſt propoſition is deduced only from a conſideration of bodies in a ſolid ſtate.

Poſſibly a more diſtinct application of a principle, already in ſome degree adopted both by Doctor Prieſtley and Mr. Cavallo, may remove all the difficulties of this inquiry. At leaſt I will hope, that it may lead to ſuch a conſideration of the queſtion, as may ſubject the merits of the theory itſelf to a fair and deciſive diſcuſſion. This principle is ſaturation. † Doctor Prieſtley has explained the communication of the redundant fluid of a body poſitively electrified to another, a part of whoſe fluid had been previouſly expelled, by ſuppoſing that it was more ſtrongly attracted by the other body, than by its own which had more than its natural ſhare; and ‡ Mr. Cavallo has in the ſame manner accounted for the mutual attraction of bodies in different ſtates of electricity.

In applying this principle to the ſolution of electric phænomena three forces muſt be conſidered: 1ſt. the attraction ſubſiſting between each body and its own portion of the electric fluid; 2dly, the attraction which may ſubſiſt between each body and the portion of fluid belonging to the other; and 3dly, the repulſion ſubſiſting between the two portions of the electric fluid.

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* Cavallo's Complete Treatiſe on Electricity, Vol. I. p. 326.

† Hiſtory of Electricity, p. 253.

‡ Vol. I. p. 109.

That the attraction ſubſiſting between two bodies in oppoſite ſtates of electricity may be explained, it is neceſſary to conſider previouſly the caſe of two bodies in their natural or ordinary ſtate. In this caſe the force ſubſiſting between each body and its own portion of the electric fluid is not in a ſtate of ſaturation, becauſe it muſt be ſufficiently ſtrong to counterbalance the elaſticity of the fluid. Each body is therefore ſtill capable of being attracted by the fluid belonging to the other, and each portion of the fluid is alſo capable of ſuch attraction. This force, if it ſhould operate alone, would draw the bodies together; but the mutual repulſion of the two portions of the fluid tends to produce the oppoſite effect. The quieſcence of the bodies proves the equality of theſe forces.

If two bodies in oppoſite ſtates of electricity be brought together, the body poſitively electrified cannot be attracted towards the remaining electric fluid belonging to the other, becauſe this body may be conſidered as ſaturated with the fluid, and that portion of the fluid as ſaturated with ſolid matter. For the oppoſite reaſons an attraction will take place between the body negatively electrified and the fluid belonging to the former. It remains to be ſhewn, that this attractive force may exceed the mutual repulſion of the two portions of fluid. It muſt be obſerved, that the repulſion remains the ſame, becauſe the ſum of the two quantities of fluid is not altered; whereas the attraction is augmented by the unequal diſtribution of the fluid. The one body is charged with more fluid than that which its own attracting force is capable of retaining, and the redundant fluid will conſequently be ſtrongly impelled towards the other body, whoſe attractive power is at the ſame time increaſed by the deficiency of its own portion of fluid.

In the caſe of two bodies ſimilarly electrified the bodies may be either both poſitively, or both negatively electrified. When they are both poſitively electrified, they are both ſaturated with the electric fluid; and when they are both negatively electrified, both remaining portions of the electric fluid are reciprocally ſaturated with ſolid matter. In neither caſe therefore can any attraction take place between either body and the fluid belonging to the other. Conſequently, the repulſion exiſting between the two portions of the fluid muſt operate without reſiſtance, and the two bodies be repelled from each other.

Should this ſolution of electric attraction and repulſion be admitted, it will perhaps alſo remove the difficulty of magnetic repulſion. In this part of philoſophy it has been found difficult to explain the repulſion of the correſponding poles agreeably to the theory of a magnetic fluid. In every magnetical body the equilibrium of this fluid is ſuppoſed to be diſturbed, and one part of the body is conceived to be overcharged with the fluid, whilſt the other is undercharged. The difficulty was to explain the repulſion of the undercharged poles, as in electricity to explain the repulſion of bodies negatively electrified. Mr. Kirwan has indeed, in a Memoir contained in the Sixth Volume of the Tranſactions of the Academy, referred the phænomena of magnetiſm to cryſtallization; but his mention of the term *ſaturated* in that Memoir ſeems to imply, that he does not mean to exclude the ſuppoſition of a magnetic fluid. If this be adopted, the preceding ſolution may be applied to the phenomena of magnetiſm, in the ſame manner in which it has been already applied to thoſe of electricity.

The theory, according to which the preceding ſolution has been propoſed, ſuppoſes the electric fluid a *ſingle* fluid; but it is not neceſſary that it ſhould be conceived to be abſolutely *ſimple*. We know, for inſtance, that atmoſpheric air is a combination of at leaſt two diſtinct fluids; and yet explain the phænomena of the barometer, air-pump, and condenſer, as depending merely on its preſence or abſence, without any reference to the compoſition of its nature. In the ſame manner ſome electric phænomena may be juſtly explained by conſidering them as the effects of the different diſtribution of the ſame fluid; whilſt its phoſphoric ſmell, its power of changing blue vegetable colours to red, and its combuſtion may poſſibly be derived from its decompoſition.

VI.

Deſcription of a new Rotatory Engine for raiſing Water, and for other Purpoſes: By a Correſpondent.

To Mr. NICHOLSON.

SIR,

I SUBMIT to your judgment, and ſhall be much gratified by your opinion on the engine of which I ſend you the incloſed drawing. If you think it may be intitled to a place in your collection, either as an apparatus, or as affording ſome hints to the improvement of hydraulics, the whole of my views reſpecting it will be anſwered. I will confeſs to you, that I have not conſtructed it upon a ſcale of actual work, and am well aware how many ſubordinate points of organization require to be ſettled, before any mechanical contrivance can obtain its beſt and moſt effective form: but as it has already been the ſubject of a conſiderable degree of meditation, I have ventured to offer it to you in its preſent ſtate.

I am, Sir,

Your obliged Servant,

London, Dec. 14, 1800. O. B.

DESCRIPTION.

In Plate XX. the upper figure repreſents an horizontal ſection of the main part of the engine, and the lower figure ſhews the ſame engine, as it would appear to an obſerver viewing it at right angles to the former direction of ſight: the ſame letters in both denoting the ſame parts. A A repreſents an elliptical veſſel of wood, or rather of metal, having its top and bottom flat, and joined to the ſides by flanches or borders. The ſides are upright, and may be of any height according to the intended capacity. In this veſſel revolves the cylinder B, B, having the ſame axis, and touching the internal circumference at two oppoſite lines, where the place of contact is made water tight by leathering, packing, or any of the uſual methods. At oppoſite parts of the ſurface of the cylinder are two flaps C, C, which are urged outwards by ſprings, which cauſe them to bear againſt the interior ſurface of the ellipſis, and when preſſed down they fall into cavities in the cylinder, ſo as to complete the defective portion of its face. It muſt be underſtood that theſe flaps, as well as the cylinder itſelf, are of the height to fill up the interval between the flat faces of the top and bottom, and are ſecured ſo as to be water tight. Laſtly, E E are two pipes leading downwards to the water intended to be raiſed, and F F are two pipes leading upwards to the reſervoir, or receptacle, to which it is to be conveyed. The firſt mover is applied at the extremity D of the axis.

The action. Let us ſuppoſe an engine of this conſtruction to be fixed on board a ſhip centrically beneath one of the capſtans, and that by the common contrivance of a gear bar, or other equivalent piece, the lower extremity of the axis of the capſtan is connected with that of the machine, and a convenient number of men ſet to work it in the direction H G. The immediate conſequence will be, that the ſpaces H, H, behind the flaps C, C, will be enlarged, and as they are air tight, the preſſure of the atmoſphere will drive water from the well through the pipes E E into thoſe ſpaces. As the flaps C proceed towards F, they will be preſſed in by the elliptical concavity, and at laſt the ſpaces G G will diſappear, and the greateſt part of the ſpaces H H (then conſtituting the whole internal part of the veſſel) will be nearly full of water. By the continuance of the proceſs the two flaps will have paſſed the places of contact, and begin to open on the oppoſite ſides of the ellipſis, driving before them the water and air up the pipes F F; at the ſame time that the ſpaces H H become again enlarged, and draw a freſh ſupply of water through E, E. A conſtant and almoſt equable ſtream is thus produced through the apparatus, which has this advantage, that by increaſing the number of men at the capſtan, the velocity of this current may be increaſed, ſo as to equal, as it ſhould ſeem, any exigency the ſtate of the veſſel might produce.

If the ſuction pipes were occaſionally led into the ſea, and the forcing pipes into a moderate ſized air veſſel, like that of the fire engine, a very powerful ſtream of water might be directed to any part of the ſhip, in caſe of the dreadful accident of fire; or for the common purpoſes of cleanlineſs of the births of men or animals, upon which ſo much of their health and comforts depend.

Shall I extend my ſpeculations farther, and expreſs my conjecture, that this might form no contemptible ſteam engine, if ſteam were admitted through E into H, and condenſed ſo as to render the preſſure of the atmoſphere active in G upon the flap, when the condenſation were made; or that it might receive the action of a deſcending ſtream of water, and work a mill, or other uſeful machine, &c. &c. Or ſhall I not rather leave this viſionary region of fanciful indulgence, and commit the fate of my engine at once to the cenſure or praiſe of yourſelf and readers?

O. B.

⁂ Many engineers have exerciſed their talents in contriving machines which ſhould raiſe water without the alternate action of the pump; but I do not recollect having ſeen any conſtruction preciſely the ſame as that of my correſpondent. In the *Recueil des Machines et Inventions approuvées par l'Académie Royale des Sciences*, tom. I. page 103, there is a machine by the celebrated M. Amontons, for raiſing water by a drum or cylinder, fixed with its axis horizontal, and an ellipſis revolving in it againſt two flaps or valves attached to the cylinder; the effect of which was to afford two pair of variable ſpaces ſimilar to the engine before us. It is dated 1699.

Mr. Thomas Dickinſon in 1790 obtained a patent for a new engine on a rotatory principle, of which the contrivance ſhews a mind habituated to mechanical reſearch and invention. The principal organ conſiſts of a cylinder, in which another eccentric cylinder revolves, leaving a gibbous ſpace on one ſide, and out of this laſt cylinder iſſues two ſliders croſſing each other, and ſo contrived as to length and other expedients, that they ſweep the cavity, and afford variable ſpaces for the introduction and extruſion of water. The drawings may be ſeen in the ſecond Volume of the Repertory of Arts, and the whole differs very much from the invention of my correſpondent.

The action of ſteam againſt flaps or valves between two concentric cylinders, forms part of ſome imperfect deſcriptions in the ſpecifications of the celebrated James Watt, and there is alſo a contrivance of this kind looſely deſcribed in the firſt volume of the Iriſh Tranſactions, by John Cooke, Eſq.

I ſhall not attempt to diſcuſs the relative values of theſe inventions, either with regard to each other, or to the engines more generally uſed. The advantage of an inceſſant or continued motion is admitted by all mechanics, and might be eaſily diſplayed. But in engines like every one of the foregoing, the difficulties of ſtuffing or packing the parts which move in contact, and the conſiderable friction and wear they muſt be ſubject to, unleſs improved far beyond their preſent ſtate, are ſo great, that I think few practical men would venture to undertake them. I do by no means preſume to ſay, that they are on that account uſeleſs. Undoubtedly they may be claſſed at leaſt with thoſe products of the underſtanding, by which our knowledge and mental habits are improved, and which form a ſtock whence the moſt ſtriking and unforeſeen diſcoveries are occaſionally drawn. Sheep were marked, loadſtones were examined, and the covers of kettles were blown off many ages before the art of printing, the mariner's compaſs, and the ſteam engine were thought of.

W. N.

VI.

Account of a new Method of bleaching Cotton, as publiſhed by Chaptal, Member of the National Inſtitute. By J. C. Delametherie [*].

THE ſuccefsful experiments made by Berthollet in bleaching vegetable cloth, by means of the oxygenated muriatic acid, ſeem to have brought this art nearly to a ſtate of perfection; but this method is not in every inſtance equally œconomical: it requires to be performed by ſkilful operators, in order that the goods may not be affected by a ley too corroſive, or applied at an improper time, independent of which conſideration, it is deſirable that every proceſs ſhould be completely diſcloſed, in order that the artiſt may chooſe ſuch means as may be beſt ſuited to his purſuits.

This conſideration has induced me to deſcribe a very ſimple and œconomical proceſs for bleaching cotton thread; it is as follows:

At the height of about four decimetres (18 inches) and an half above the grate of a common furnace, a copper boiler is placed, of a round form, five decimetres (20 inches) in depth, and one metre and a third, (3½ feet) in diameter. The projecting rim of the cauldron, which is about two decimetres (8 inches) reſts upon the brick work of the furnace. The remainder of the kiln is raiſed of free ſtone, and forms an oval boiler or digeſter about two metres (6 feet and a half) in height, and its width, when meaſured at the centre, is about one metre and a third, or 3½ feet.

The upper part of this veſſel has a round orifice about half a metre (19½ inches) in diameter, which is cloſed, when neceſſary, by a large moveable ſtone, or by a copper lid, adapted for the purpoſe. On the flanch of the copper veſſel, which forms the bottom of this kind of digeſter, a grating is laid, which conſiſts of bars of wood, placed near enough to prevent the cotton that is put on them from falling through, and ſufficiently ſtrong to ſupport the weight of eight hundred kilogrammes, (or 1½ cwt.)

When this ſtructure is completed, the cotton having been previouſly divided into hanks or parcels is ſlightly impregnated with a ſolution of ſoda, rendered cauſtic by the addition of lime. This operation is performed in a trough of wood or ſtone, in which the cotton is worked by men with their feet defended with wooden ſhoes. As ſoon as the cotton is ſufficiently impregnated with the alkaline liquor, it is conveyed to the digeſter, and piled upon the wooden grate. In this ſituation the exſuding liquor runs through the bars into the copper boiler, where it forms a ſtratum of fluid, and which allows the whole maſs to be heated without danger of burning either the cotton or the metal. The alkaline ley is compoſed of Alicant ſoda, in quantity one tenth part of the weight of the cotton, and in a veſſel of the dimenſions above deſcribed, there may be employed at each time, forty myriagrammes

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[*] Journal de Phyſique, Vendemaire, an. 9, page 305.

grammes (about 800 French pounds, or 880 avoird.) of cotton. The denſity of the ley is in general about two degrees, (qu. by what inſtrument?) After the cotton is properly diſpoſed in the boiler, the cover is put on, and ſcarcely any iſſue left for the vapours diſengaged by the heat, in order that they may acquire a more conſiderable degree of heat, and re-act with increaſed force upon the cotton. When the digeſter is charged, the fire is lighted in the furnace *, and the ley ſubmitted to a gentle ebullition from twenty to thirty-ſix hours. It is then left to cool, the cover is taken off, the cotton carefully waſhed, and expoſed in the bleaching ground for two or three days, by ſpreading it on frames during the day, and ſpreading it on the graſs at night. Thus the cotton acquires a beautiful degree of whitenefs; and if by accident ſome portions of the ſkains ſhould remain unbleached, they are replaced in the boiler, and ſubjected to a ſecond operation, or otherwiſe they are left in the field for ſeveral days longer. Theſe ſhades in the bleached cotton are the conſequence of its not having been completely and equally impregnated with ley; but they may alſo proceed from too cloſe preſſure in certain parts, at the time of ſtowing it in the boiler. When the ley is ſuppoſed to be exhauſted by ebullition, the boiler is uncovered, and the cotton wetted with an additional quantity of the ſolution of ſoda; for unleſs this precaution is taken it may eaſily burn.

From theſe particulars it would be eaſy to form a notion of the œconomy of this proceſs, by calculating the price of the articles, and the time employed in the operation, if there were not a more ſimple method of eſtimating its advantages, namely, the low price at which cotton is bleached in all the manufactories where this diſcovery is practiſed. In the ſouth of France, where it is now generally adopted, cotton is bleached at the rate of about eight francs, for forty kilogrammes, (or rather cheaper than a penny a pound.) This proceſs was brought from the Levant, a ſhort time after we had obtained the method of producing the Adrianople red. It was reduced to practice, but hitherto kept ſecret, under the name of *blanchiment à la fumée*, (vapour bleaching.)

Extract from the Bulletin of the Philomatic Society.

This proceſs has been employed with the greateſt ſucceſs by Bourlier, at Bons-Hommes, near Paris.

The Engliſh, who never neglect an opportunity of improving their manufactures, made trial of the proceſs of Chaptal, as ſoon as they received information of it. It has completely ſucceeded, as appears by the following extract of a letter from a man of ſcience at London †.

"A new method of bleaching has juſt been tried at Balynah, and has completely ſucceeded. The principle of the proceſs appears to have been publiſhed by a French

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* The conſtruction here deſcribed is for a fire of pitcoal; but if wood be uſed, the dimenſions of the fire-place muſt be different. In the latter caſe the grate would be uſeleſs, and the bottom of the veſſel too far from the fire.--Delametherie.

† The extract is in French, from which I retranſlate.---N.

“ chemiſt, Chaptal, who is much reſpected by our manufacturers. I ſpeak of the art of diſcolouring piece goods in a digeſter, by cauſtic alkaline ley. Though our firſt attempts did not perfectly ſucceed, we were not diſcouraged. The linen was expoſed to the action of vapour in the apparatus, but it was not equally affected, as it appeared to be blotched in ſeveral places; we were, therefore, obliged to conſtruct an apparatus, in order to unroll and ſeparate the goods, and to expoſe the greateſt ſurface poſſible to the action of the vapour. Suppoſe the boiler of a ſteam engine, in the form of an elongated ellipſis, provided with a ſafety valve, two tubes with cocks, to ſhew the conſumption of the liquor, and a mercurial gage, to aſcertain the ſtrength of the ſteam. This boiler is bedded in maſonry, or brick work, that it may reſiſt the exceſſive preſſure which neceſſarily takes place. In the interior part of the apparatus are ſix reels, three at each end, alternating with each other, in order that the action of the ſteam may be more equable upon the goods. Theſe reels are ſlowly and uniformly carried round by ſimple tooth and pinion work of wood, and the firſt motion is given to an axis which paſſes out of the boiler through a ſtuffing box, which prevents the eſcape of vapour. At the top is an opening of about ſixteen inches diameter, with a rim or flanch, on which the cover is fitted, and firmly ſecured by ſcrews. Between the two metallic faces are placed ſtrips of ſoaked leather, to prevent the vapour from eſcaping. When the cover is taken off, the workmen can enter the boiler, to diſpoſe the goods upon the rollers, each of which contains about fifteen or twenty pieces, making in the whole about forty-five or ſixty. The raw material, namely, cunnamara kelp, is an article of inconſiderable expence, or elſe the ſoda extracted from ſea ſalt, in which there remains indeed a ſmall portion indecompoſed, but which we procure at a very reaſonable price. It is rendered cauſtic by the addition of ſome good lime, which is made from our lime-ſtone of Parre, with theſe a ley is formed, which is equal to fourteen degrees of our hydrometer. In this lixivium the piece goods are boiled, and then conveyed to the digeſter, on the bottom of which the ley ſtands to about five inches in depth. The workman ſtands upon a perforated ſtage, which prevents him from ſtepping into the ley while he is arranging the pieces: after which, having placed them on the rollers, the apparatus is cloſed, the fire lighted, and the operation begins. As ſoon as ebullition takes place, the handle on the outſide is inceſſantly turned, and as ſoon as the roller at one end is filled, the handle is ſhifted to the other roller, and the turning performed in the contrary direction. In this manner the operation is continued till the whole of the contents is bleached. From this deſcription you may eaſily underſtand how this operation is performed; I ſhall, however, take the firſt opportunity of ſending you a plan and deſcription of the apparatus, if you wiſh for further information. You are at liberty to make whatever uſe you pleaſe of this account: the expence of bleaching is not more than one farthing per yard, including coals, workmen's wages, &c. as well as intereſt for the capital employed in the apparatus.”

VII.

On the Chemical Effects of the Pile of Volta. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

THERE is ſomething ſo faſcinating in the eaſe with which the preſent ſyſtem of chemiſtry can be applied to moſt of the phenomena of nature and art, that I may ſuffer the deriſion perhaps of many philoſophers, if I even queſtion its application in any. However, as truth ſhould be the object of all who cultivate a knowledge of nature, I ſhall venture to mention a few circumſtances.

I have lately repeated moſt of the experiments which have been made on the pile of Volta, and have read with much attention the opinions which you have publiſhed of your own, and of ſome of your learned correſpondents.

When two wires of platina are uſed, and when they are placed in water, two gaſes are produced, the one having the properties of oxygen, the other of hydrogen gas. Theſe wires may be placed at very conſiderable diſtances from each other, and yet if they are in the ſame veſſel of water, they produce theſe airs as ſpeedily and in as great quantities as when they are ever ſo cloſe to each other.

Now, Sir, I wiſh to know how it happens, according to any ſyſtem, that the two component parts of water ſhould be made to appear at ſuch diſtances from each other. Does the hydrogen of the decompoſed particle of water on the zinc ſide of the pile, fly away inſtantly as the oxygen is produced on that ſide, to the wire connected with the ſilver? If it does, why do we not ſee the bubbles in its paſſage? Or does the oxygen paſs from the wire connected with the ſilver to that connected with the zinc? Or are there two currents.

In the ordinary modes of reaſoning on theſe ſubjects, we generally ſuppoſe that when one of the component parts of a ſubſtance is ſeparated or is fixed, the other appears inſtantly in ſome way or other, and cloſe to it. If, for inſtance, a bar of red-hot iron be immerſed in water, the oxygen unites with the iron, and forms an oxide, and the hydrogen ariſes immediately from it, wherever the oxygen becomes fixed. This ſeems perfectly well explained by the preſent theory of chemiſtry. In the pile, if copper wires be ſubſtituted for platina, one wire will be oxidated whilſt the hydrogen ariſes from the other at a diſtance. When the phoſphuret of lime is dropped into water, the bubbles of phoſphorated hydrogen appear cloſe to it, as ſoon as the oxygen unites with the ſaid phoſphorus. This alſo appears to be well explained by the modern theory. If the particles of water be compoſed of the two baſes of oxygen and hydrogen gas, and if one of thoſe particles be decompoſed, and one of its principles appear, I contend that ſome account ſhould be given of the other. It is a new principle for it inſenſibly to hurry through the water for a diſtance of ſix inches or more, and there to make its appearance in the character of gas. Volta's diſcovery of the pile ſeems to open a very large field of inquiry; it has already altered the arrangement of many facts in philoſophical chemiſtry. As an individual experimentaliſt, I can ſay it has led me to many inveſtigations which have explained difficulties which heretofore conſiderably perplexed me.

I may hereafter take the liberty of ſubmitting to you ſome further experiments and obſervations on this ſubject, and ſome opinions reſpecting the generally received doctrine of the decompoſition of water.

If you ſhould deem this worthy a place in your learned Journal, by inſerting it you will oblige

Your obedient humble ſervant,

AN EXPERIMENTALIST.

December 21, 1800.

⁂ The elucidation of the above and many other difficulties of the new galvanic philoſophy muſt be left to the experimental reſearches of thoſe able men who are now employed upon it. But I may here remark that it is probable that the local proximity of chemical effects dependant on each other may not be ſuch as my correſpondent apprehends, but that ſome diſtance both of ſpace and time may intervene between all ſuch phenomena. The current from the pile may perhaps teach us to generalize and correct our notions on this head. Is it not a parallel fact in chemiſtry that the vegetations of a metal (precipitated from an acid by the contemporaneous ſolution of another metal) are formed and depoſited at the diſtance of many inches from the place of ſolution? When lead, for example, is precipitated by zinc, and the vegetation towards the end of the proceſs is formed near the bottom of a tall veſſel,—is not the ſolution of the zinc an evidence, according to our common proceſs of reaſoning, that lead exiſts alſo in the ſolvent at that place of action; and if ſo, why is not that lead ſeparated inſtead of the other very remote portion?—N.

VIII.

Memoir of ſeveral newly diſcovered Properties of phoſphorated Hydrogen Gas. By Citizen RAYMOND, *Profeſſor of Chemiſtry at the Central School of the Department of the Ardèche**.

WE are indebted to Citizen Gengembre for the important diſcovery of phoſphorated hydrogen gas. Before his time no chemiſt had diſcovered an elaſtic fluid, which poſſeſſed the remarkable property of taking fire by the contact of air alone, without the neceſſity of increaſing its temperature, or placing it in contact with a body in a ſtate of ignition.

The undulated, and continually increaſing crown, which is afforded by this gas when burned by a ſingle bubble at a time, in any place where the air is perfectly calm; the luſtre and magnificence which attend this combuſtion, when effected in pure oxygen gas; the ſudden penetration of the two gaſes, and their total converſion into water and phoſphoric acid. Theſe were the only known facts which had intereſted chemiſts in the hiſtory of phoſphorated hydrogen gas when I attempted to diſcover whether this elaſtic fluid did not poſſeſs other properties, which, although they might not afford a ſpectacle of equal brilliancy with thoſe above-mentioned, might not on that account be leſs worthy the attention of philoſophers.

The well known property imparted by ſulphur to hydrogen, and reciprocally by hydrogen, namely, that both, when combined, are ſoluble in water, though each, taken ſeparately, is perfectly indiſſoluble, had already led me to imagine that the ſame effect might take place in a combination of phoſphorus and hydrogen; and that theſe ſubſtances being previouſly united, might then perhaps become ſuſceptible of partaking of the liquidity of water, by communicating to the latter ſome new properties, nearly ſimilar to thoſe poſſeſſed by the ſolution of ſulphurated hydrogen gas, commonly called hepatiſed water.

To deſtroy or effectually to confirm this ſuſpicion, I took a flint glaſs decanter, which I filled with newly diſtilled water, of the temperature of twelve degrees; I then inverted it on the ſhelf of a pneumatic trough, in order to receive the phoſphorated hydrogen gas, which is afforded by the decompoſition of water, by a mixture of phoſphorus and recently ſlaked lime. (This proceſs is deſcribed in the Annales de Chimie, for the year 1787.) As ſoon as the decanter was about half full of phoſphorated hydrogen gas, I removed it from the ſhelf, carefully cloſing its orifice with my finger, and then briſkly ſhaking it, in order to cauſe a more rapid combination of the gas with the water, in the ſame manner as is done to promote the ſolution of carbonic acid or ſulphurated hydrogen gaſes.

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* Annales de Chimie, XXXV. 225.

By the cloſe adheſion of my finger to the mouth of the decanter, I ſoon perceived that a conſiderable vacuum was produced, or, in other words, that a great portion, or perhaps even the whole of the phoſphorated hydrogen gas contained in the decanter had become diſſolved in with the water.

I then, not without difficulty, withdrew my finger from the orifice of the decanter, in order to examine more attentively the properties of the fluid it contained, as well as to diſcover whether ſome particles of gas, capable of inflammation by the contact of air, might not remain. But ſcarcely had the atmoſpheric air entered the decanter, when a ſtrong exploſion took place, attended with a very brilliant light. From this effect I was well convinced that the whole of the phoſphorated hydrogen gas had not been abſorbed by the water contained in the decanter, and therefore I ſuddenly cloſed the orifice, to prevent the continuance of the combuſtion, which would certainly have produced a conſiderable quantity of phoſphoric acid, and, conſequently, have produced a degree of uncertainty in the reſult of my enquiries.

The decanter being exactly cloſed, I again ſhook it ſeveral times, from a notion that I ſhould thus ſucceed in completely fixing the laſt portions of hydrogen gas, which ſtill remained undiſſolved.

In hopes of ſucceſs, and impatient to aſcertain the new properties which might have been acquired by the impregnated water, I reſolved to open the bottle a ſecond time, and expoſe its contents to the contact of the air. Another exploſion ſoon followed, but leſs violent than the firſt. After this event, I did not again cloſe the bottle, but obſerved that an extremely pale flame continued to eſcape from it during ſeveral minutes.

When there no longer remained any apparent ſigns of combuſtion, I proceeded to ſmell and taſte the fluid in the decanter. Its ſmell was extremely diſagreeable, and very different from that afforded by gaſeous phoſphorated hydrogen: and its taſte, though very bitter, was nevertheleſs inſipid and nauſeous. Its colour inclined to yellow.

With the tincture of turnſol it ſoon became perceptibly red, which I attributed to the ſmall portion of phoſphoric acid produced at the moment when the exploſions took place, as well as during the time of the gradual inflammation which ſucceeded the ſecond detonation.

I was now, therefore, obliged to repeat the operation, by diſſolving ſome phoſphorated hydrogen a ſecond time in water, in order to aſcertain, by a greater attention to my experiment, in what exact proportion this ſolution might be effected, and alſo to prevent the combuſtion from taking place in the veſſel: otherwiſe my trials would have remained inconcluſive.

(To be concluded in our next.)

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Premiums offered by the Board of Agriculture.

THIS Board having been required by a committee of the Houſe of Lords, "to examine "into, and report to their lordſhips, the beſt means of converting certain portions of graſs "lands into tillage, without exhauſting the ſoil, and of returning the ſame to graſs, after "a certain period, in an improved ſtate, or at leaſt without injury," and being deſirous that their information, on a ſubject of ſo great importance, ſhould be compleat—adapted to every ſort of ſoil, and founded on the moſt ample experience—have come to the reſolution of offering the following premiums for that purpoſe, viz.

To the perſon who ſhall produce, on or before the firſt day of February, 1801, the beſt and moſt ſatisfactory eſſay on the ſubject before mentioned, diſtinguiſhing reſpectively, what part of the plan recommended, or of the details given, is the reſult of actual experiment, accurate obſervation, or well-authenticated information — *Two Hundred Pounds.*

For the ſecond beſt—*One Hundred Pounds.*

For the third beſt—*Sixty Pounds.*

For the fourth beſt—*Forty Pounds.*

And to ſuch perſons who may communicate information, which, though uſeful, may be conſidered of leſs importance, ſmaller rewards, proportioned to the opinion of the Board.

It is required that each eſſay ſhall fully detail the courſe of crops, regard being had to the varieties of ſoil, and the time propoſed for continuing the land under tillage.

Alſo, to explain the caſes in which it may be eligible to drain land, previous to tillage.

In what caſes paring and burning are advantageous, with directions thereon, regard being had to the ſubſequent cropping.

The depth to which graſs lands ſhould, at firſt breaking up, be ploughed.

Whether the crops, intended for cattle and ſheep, are to be fed on the land, and by which kind of ſtock, or carted off.

To ſtate—

- The crop with which the graſs ſeeds, in each caſe, ought to be ſown, when the land ſhall be again laid down:
- The ſorts and quantities of graſs ſeeds for each kind of ſoil, and whether to be provided by landlord or tenant:
- Whether it be beſt to mow or feed the graſs in the firſt year after laying down—to detail the management in each caſe:
- The manuring which may be thought neceſſary:

The principle on which an increaſe of rent ought to be eſtimated, where permiſſion may be given to break up old paſture now under leaſe.

The Board requires that theſe objects ſhould be particularly attended to, with relation to the leading qualities of land, viz.

Clay, in all its diſtinctions; and ſoils too ſtrong or wet for turnips:

Loam, in all its diſtinctions, fit for turnips:

Sand, including warrens and heaths, as well as rich ſands:

Chalk land, and downs:

Peat, including moory, ſedgy, rough bottoms, and fens.

It is hoped that no perſon will be deterred from communicating his knowledge to the Board, on account of his experience being confined to one of theſe ſoils only.

The Board reſerves the power of withholding any premium, in the caſe of no eſſay being deemed ſufficiently important to merit it.

The eſſays which ſhall obtain any premium, or other reward, to remain the property of the Board.

Each eſſay to be ſent (ſealed) without a name, but with a mark, or motto; and accompanied by a ſealed letter with the ſame mark or motto, containing the name and addreſs of the author; and this letter will not be opened, unleſs one of the prizes, or ſome other reward, ſhall be adjudged to him.

All communications to be addreſſed to Lord Carrington, Preſident, Sackville-ſtreet.

Prizes offered by the Claſs of Mathematical and Phyſical Sciences of the National Inſtitute of France in its Public Sitting, 15th Germinal in the year 8 (April 4, 1800.)

The firſt Prize.

The claſs of Mathematical and Phyſical Sciences having propoſed a ſecond time in the year 6, as the ſubject of a prize to be determined at the public ſitting of Germinal in the year 8, the anatomical compariſon of the liver in the different claſſes of animals; and no memoir having been tranſmitted to them on this ſubject, the claſs has thought proper to withdraw it, and propoſe the following queſtion:

It is required to determine by anatomical and chemical experiments, what are the phænomena of the torpid ſtate which certain animals, ſuch as the dormouſe, the marmot, &c. experience during the winter, with regard to the circulation of the blood, reſpiration, and irritability: to aſcertain what are the cauſes of this ſleep, and why it is peculiar to thoſe animals.

The candidates are invited to examine more particularly into the differences preſented by thoſe animals in their lethargic and their ordinary ſtates, with regard to the pulſe, the heat of the blood, the frequency of reſpiration, the quantity of oxygen conſumed in a given time, and the excitability by galvaniſm. They will alſo examine the anatomical differences which diſtinguiſh theſe animals, from ſuch as are not ſubject to become torpid in the winter, and enquire how far theſe differences may be ſufficient to explain the phænomena of this lethargic ſtate.

The prize, which is a gold medal of the value of one kilogramme (15450 grains, or about 128l. ſterling) will be given at the public ſitting of the 15th Germinal, in the year 11 (April 4, 1803.)

The memoirs will be received till the 15th Meſſidor in the year 10, and not afterwards, (July 2d. 1802.)

The ſecond Prize.

The ſame claſs propoſed in the year 6, as the ſubject of a prize to be determined at the public meeting of Germinal in the year 8, the following queſtion:

To aſcertain by accurate experiment the influence of atmoſpheric air, of light, of water, and of earth in vegetation.

Though the inſtitution has received no memoir on this intereſting queſtion, it has thought proper to propoſe it again; but as the different works which are neceſſary for the complete ſolution require much time and many experiments, and it can ſcarcely be expected, that all the elucidation which is deſired can be obtained from the candidates during the intended prolongation of the term;

The claſs has determined, that in caſe no memoir ſhould be received at this ſecond concurrence, in which the queſtion ſhall have been treated in its utmoſt extent, the prize ſhall be given to that piece which ſhall contain only a ſeries of experiments, facts, and obſervations, which ſhall appear the moſt proper to increaſe the information we already poſſeſs reſpecting ſome parts of the problem propoſed.

The prize will be a gold medal of the value of one kilogramme (15450 grains, or about 128l. ſterling.) It will be given at the public ſitting 15 Meſſidor, year 10, (July 2, 1802.) The memoirs muſt be ſent before the firſt Nivoſe in the year 10, (Dec. 21, 1801.)

Third Prize.

What are the characters which diſtinguiſh among vegetable and animal ſubſtances, thoſe which ſerve as ferments, from thoſe in which they excite the fermentative proceſs?

The prize will be a gold medal of the value of a kilogramme, and it will be diſtributed at the public ſitting of the 15 Germinal in the year 10.

The memoirs muſt be ſent before the firſt Nivoſe in the ſame year.

Candles with wooden Wicks.

Profeſſor Medicus of Heidelberg has given an account of the candles with wooden wicks, which for ſome years paſt have been manufactured and uſed at Munich, from the original inſtructions of Count Rumford, whoſe active ſervices in advancing the comforts of ſocial life are ſufficiently known *. The profeſſor uſed them for a whole winter, and found them ſuperior to every other kind of candle in the following advantages: they give the ſame quantity of light as a wax candle (bougie); they burn with an equal and conſtant flame; they do not ſputter, and never run over.

The only difference between theſe and common candles conſiſts in the wick. The profeſſor did not know the actual proceſs of the manufactory in wrapping the cotton round the wood. The editors of the Annales think, that the flake of cotton delivered from the carding engine, and then preſſed between rollers, would be exceedingly well adapted to the purpoſe; and there is no doubt that various methods may be adopted for ſo eaſy a proceſs. Any kind of reſinous wood may be uſed, and tallow of the firſt quality is applied to this object in Bavaria. The candles are ſold one eighth dearer; but as it is ſtated that they laſt one fourth longer, there muſt be a conſiderable ſaving.

The ſize of the wood is about that of a ſtraw, and the pieces are dipped in wax or tallow, ſo as to have a very ſlight covering of the material. They are then rolled up in the carded cotton till they have acquired the ſize of a common wick. The covering muſt be very equally diſtributed. The editors propoſe, in order to detach the wick from the reſt of the cotton without the uſe of ſciſſors, that a ſtraight edge of iron ſhould be preſſed on the place where the ſeparation is intended, and the wick then drawn away.

The candles are made in a mould as uſual, taking care to place the wick exactly in the middle. Some experiments would no doubt require to be made, particularly to aſcertain the beſt dimenſions or proportions of wood, cotton, and tallow, for candles of different ſizes. The ſnuffers would require to be ſharper than for cotton wicks. I do not remember how we ſnuffed the lob chock at China; but I think we broke off the charred piece with the fingers, or any convenient implement.

Ink capable of reſiſting the Action of Oxygenated Muriatic Acid.

Since the diſcovery of the bleaching power of the oxygenated muriatic acid, and particularly that of diſcharging the traces of common ink, it has become an object of ſerious inveſtigation to form an atramentous fluid, which ſhould not be ſubject to a treatment ſo obviouſly inimical to the written evidence of the moſt important contracts in human ſociety. Many German chemiſts have made experiments with their uſual intelligence and ſkill to reſolve this problem. Among the receipts for this purpoſe the following are the principal[*]:

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* The account is in Reim's Neue Fortgeſetzte Sammlung Vermiſchter OEkonomiſchen Schriften. 12 Th.---I have not this work, but recur to the Annales des Arts et Manufactures II. 100. I do not know whether the Count was led to this conſtruction by his own meditations, or from a knowledge of the Chineſe candle, called the *Lob chock*, for an account of which ſee our firſt Vol. page 72.---N.

1. By Weſtrumb. Boil one part of Brazil wood, and three parts of pounded nut galls, with forty-ſix parts (the whole by weight) of pure water. When the liquid is reduced to thirty-two parts, paſs it through a ſtrainer while hot upon one part and half of very pure ſulphate of iron, one part and quarter of gum arabic, and a quarter of a part of fine ſugar. When all theſe ſubſtances are diſſolved, add one part and a quarter of good indigo ground as fine as poſſible, and three quarters of a part of purified lamp black.

As the indigo is the ingredient in this ink which reſiſts the oxygenated acid, and as it is only mechanically ſuſpended, it is of importance that the fluid ſhould be ſtirred well every time it is uſed, leſt the black colour of the fluid ſhould deceive the writer.

2. By Boſſe. Boil for about a quarter of an hour, one part by weight of Brazil wood, with twelve parts of water. Add half a part of allum, evaporate the whole to eight parts, and mix one ounce (q. part?) of manganeſe, levigated to a very fine powder, and half a part of powdered gum arabic.

3. Apparently by the editor of the Annales. Boil for eight minutes one part by weight of Brazil wood, with three parts of powdered nut galls in nine parts of vinegar, and an equal quantity of water. After ſtraining the liquid, diſſolve therein half a part of ſulphate of iron, and one part of gum arabic, after which add a ſolution of half a part of indigo in one part of concentrated ſulphuric acid. The oxygenated muriatic acid, if applied, will diſſolve the oxide of iron in this ink, but it cannot decompoſe the indigo, and the great object is to prepare the ink in ſuch a manner, as that it ſhall not be diſcharged by that means.

4. The ſame editor alſo obſerves, that the common ink may be rendered incapable of being completely diſcharged by any action which the paper can withſtand, if inſtead of water, or other liquids, the expreſſed juice of green vegetables be uſed; ſuch for example as the lathiris linn. the ſambucus niger, or common graſs.

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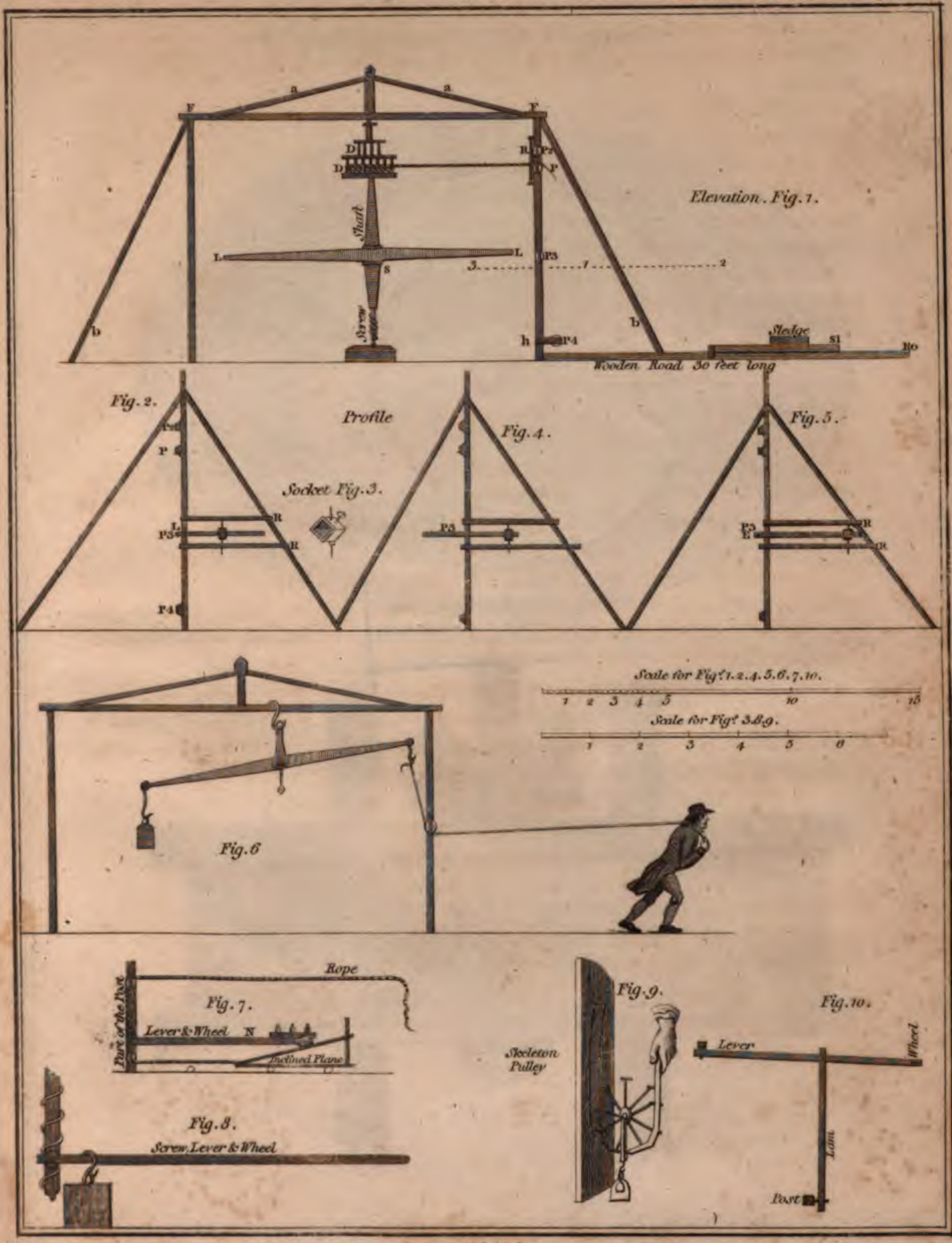
[*] Annales des Arts et Manufactures II. 106.

Panorganon.



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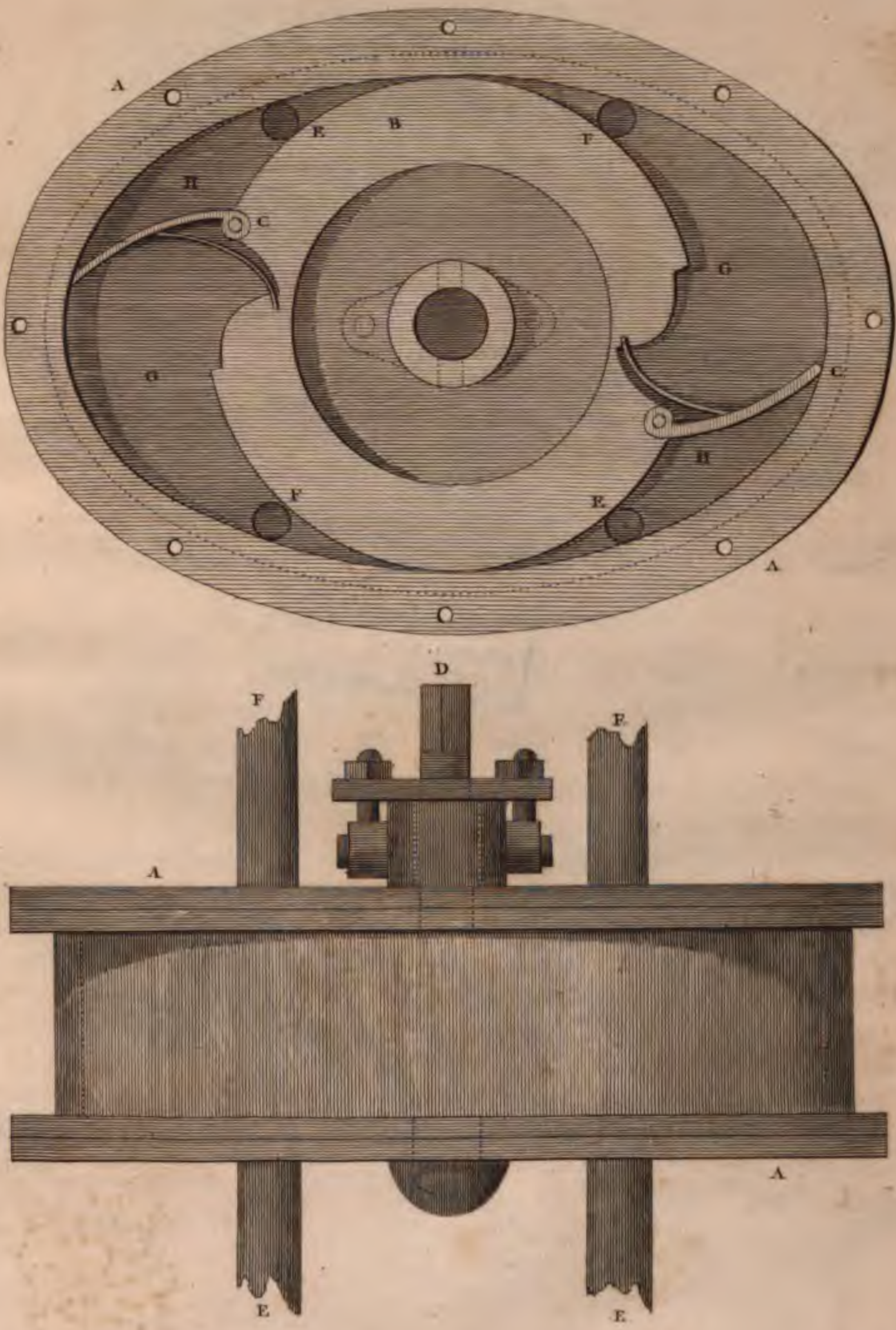
Rotatory hydraulic Engine



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ASTOR, LENOX AND
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A

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OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY 1801.

ARTICLE I.

Obſervations on a Method of reſtoring the Utility of Wells, which have been abandoned in conſequence of the Mephitization of the Ground. By Cit. CADAT-DE-VAUX, *of the Society of Agriculture of the Department of the Seine (in France.) Communicated at the Sitting of that Society on the* 16 *Brumaire, in the year* 8.*

THE well of an houſe in *la Rue de Beaune* was infected for ſeventeen years by the drainings, or tranſudations from the neighbouring privies. The proprietor deſiring, if poſſible, to reſtore it to the uſe of the inhabitants, applied to C. Dufour, who reſides in *la rue traverſe* f. St. Germain, and has ſucceſsfully attended to that department of hydraulics which relates to ſprings, wells, and fountains, who obtained the deſired effect by an happy application of the *arteſian proceſs* (procedé artéſien.)

The arteſian fountains and wells, as they are denominated in Flanders and Belgia, derive their name from the Province of Artois, where they were probably invented, and are made as follows:

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* Tranſlated from the *Decade Philoſophique*, An. VIII. 10 frimaire, p. 398.

The ground where one of theſe fountains or wells is intended to be made, is perforated with the borer; and it depends upon circumſtances, whether the reſult ſhall be a well or a fountain, that is to ſay, upon the greater or leſs elevation of the reſervoir from which the fluid is ſupplied.

In the perforation in the ground is placed a wooden pipe, which is driven down with a mallet, after which the boring is again continued, in order that the pipe may be driven ſtill lower. By means of the borer, the ſtrata of gravel, ſtones, and even rocks, if preſent, are perforated. In proportion as the cavity of the inſtrument becomes loaded, it is drawn out and emptied, and in time, (for this is not a very ſpeedy operation) by the addition of new portions of wooden pipe, the boring is carried to great depths, and water is at laſt obtained, unleſs the labour ſhall have proved to be in vain; a thing which ſometimes, though very ſeldom, happens.

If the reſervoir, or vertical head of the water obtained, ſhould prove higher in its level than the ſurface of the ground, the water ſprings up, and the reſult is not a well, but a fountain. C. Dufour has thus obtained one at Courtalin, which affords water in ſufficient abundance to ſupply the paper manufactory of C. C. Réveillon and La Garde.

If on the contrary the level be lower, the water cannot riſe above ſome elevation beneath the upper orifice of the cylinder. It is then an arteſian well.

A fountain or ſpouting ſpring may be hoped for when the boring is performed on the ſide of an hill, or in a diſtrict environed by mountains or hills; but in the plains at a great diſtance from theſe natural reſervoirs, it is ſcarcely to be expected that this operation will afford more than a well.

In thoſe caſes where no more than a metre (yard) or two is wanting to give the water that elevation which would produce a fountain ſpring, an excavation or baſon is made to the proper depth round the pipe, which being ſawed off affords a jet of water, and fills the baſon, whence it may be drawn off by a bucket, or pump.

The arteſian wells are preferable in every reſpect to our wells. They are leſs expenſive, and the ſupply of water is more certain and abundant. In fact, in the common conſtruction of our wells, when the workmen have arrived at the water, and the ſprings gain upon them, it becomes neceſſary to fix the windlaſs, and too frequently a well is made which ſupplies but a moderate quantity of water, and is dry in ſummer.

In this caſe there is no remedy but to uſe the arteſian proceſs; that is to ſay, in order to recover the water in theſe dry wells, it is neceſſary to perforate the bottom, inſert a cylindrical pipe, and proceed to ſeek for water at a greater depth.

This method of adding to the depth of wells already in exiſtence, and affording a ſcanty ſupply of water, is due to an inhabitant of Thuringia. The œconomical ſociety of Leipſic in its Collection of Memoirs (Semeſtre St. Michel 1785) has publiſhed this proceſs. One of its members, who had a well which had become dry, applied the arteſian proceſs with great ſucceſs. The author ſpeaks of this method as capable of ſpeedily affording a large quantity of water, as of great utility in a camp or fortreſs; and when the waters near the ſurface are not of a good quality, this is the beſt method of proceeding in ſearch of better at a great depth, as Cit. Dufour has done in the well which forms the ſubject of the preſent obſervation.

After the well in queſtion had been emptied, C. Dufour bored the bottom, and placed a pipe of ten feet in the hole. This depth was ſufficient to paſs through the bed of mephitized earth, and reach the lower body of water, which being thus inſulated by the cylinder, riſes pure into the body of the pump, which is fixed for that purpoſe.

I have thought it uſeful to give publicity to this laſt proceſs, more eſpecially with regard to Paris, where the neceſſaries and drains have ſucceſſively infected the ground by their bad conſtruction, and rendered a great number of wells uſeleſs. This method appears to be the only one capable of remedying this inconvenience, and reſtoring the utility of theſe abandoned wells.

I ſhould reproach myſelf for not entering into a longer detail reſpecting theſe arteſian wells and fountains, on account of the preference they deſerve over our ordinary wells, by the advantages they preſent. But knowing that my colleague C. Gillet Caumont poſſeſſes very intereſting facts reſpecting them, I have invited him, and he has promiſed the Society to extract from a work undertaken on this object, whatever may relate to its rural and œconomical uſes, and to publiſh it [*].

II.

Memoir on ſeveral newly diſcovered Properties of phoſphorated Hydrogen Gas. By Citizen Raymond, *Profeſſor of Chemiſtry at the Central School of the Department of the Ardèche.*

(Concluded from Page 475.)

FROM the quantity of water and gas which I had at firſt attempted to diſſolve, as well as from the exploſions which took place the inſtant the bottle was unſtopped, I was certain that the diſtilled water could not, at a temperature of twelve degrees, diſſolve an equal volume of phoſphorated hydrogen gas. In the ſecond experiment, therefore, I had the precaution to fill the decanter only one third with the above gas; after which I ſhook the mixture ſeveral times, in order to facilitate the combination of the gas with the water, and at the ſame time to make the union as complete as poſſible. I then unſtopped the decanter, and inverted it in a ſmall tub, full of newly diſtilled water, to aſcertain whether it would be entirely filled with this water, by the mere preſſure of the air, and that I might alſo by this means be enabled to judge whether the whole of the gas had become liquefied.

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[*] It is at preſent a common practice in London, ſince the general introduction of the ſteam engine into private manufactories, to ſink a well, below the ſtratum containing the ſurface water, into a quickſand, where plenty of ſoft water is met with. The arteſian proceſs undoubtedly deſerves to be better known from its ſimplicity, cheapneſs, and other good qualities. Dr. Darwin has deſcribed a ſomewhat ſimilar operation in the Philoſ. Tranſ. Vol. LXXV. 1, and in the ſame work, Vol. LXXIV. may be ſeen the Account of the King's Wells at Harwich, by Tho. Hyde Page, Eſq. and Mr. Vulliamy's overflowing well, in our Journal II. 276.---N.

I accordingly ſaw that a portion of the water in the tub was ſuddenly preſſed into the decanter, but not ſo much as to fill it entirely, which ſhewed that ſome of the gas ſtill remained uncombined with the water. I next made trial of the abſorbed gas by paſſing a few bubbles through the liquid, which took fire by contact of the external air, and thus proved that the phoſphorated hydrogen gas had not been at all decompoſed either by the agitation, or by its contact with the diſtilled water; whereas it ſpeedily loſes its high degree of combuſtibility, when collected in veſſels filled with undiſtilled water, or with water that had been diſtilled for a length of time. This effect I attribute to the quantity of air held in ſolution by common water, the oxygen of which coming in contact with a portion of phoſphorus ſeparates it from the hydrogen, by converting it into phoſphoreus oxide, which being perfectly inſoluble, is depoſited on the inſide of the glaſs, without affording any perceptible combuſtion in this kind of oxidation; becauſe the oxygen of the atmoſpheric air, which partakes of the liquidity of water, being thus deprived of a great part of the caloric and light combined with it, during its aerial aggregation, cannot produce both thoſe effects in a perceptible manner, when it paſſes from one combination to another in this ſtate of liquid aggregation.

Having again ſhaken the decanter, after cloſing its mouth, I ſucceeded by means of the ſmall quantity of water which had entered, and of which I kept an accurate account in obtaining the abſorption of the reſt of the gas; for on opening the veſſel again, it ſoon became entirely filled with water. Hence I may venture to affirm, from the diverſified trials, as well as from ſeveral others which I ſhall paſs over, that water which has been deprived of its air by diſtillation, is capable of diſſolving and liquifying at the ordinary temperature, rather leſs than one fourth of its bulk of phoſphorated hydrogen gas, and that by this proportion it is completely ſaturated.

This ſolution, prepared according to the above directions, and preſerved from the contact of air, has always exhibited the following properties: its colour is nearly ſimilar to that of roll brimſtone, though in general pale. Its ſmell is ſtrong and diſagreeable, and its taſte extremely bitter, though inſipid and nauſeous.

On examining this ſolution in the dark, it did not appear in the leaſt luminous, a proof that the phoſphorus had intimately combined with the hydrogen,

When diſtilled in a ſmall retort with the pneumato-chemical apparatus, a little below the temperature of boiling water, and particularly if ſubmitted when recently prepared, it afforded a conſiderable quantity of phoſphorated hydrogen gas, equally pure and combuſtible as that obtained by heating cauſtic alkalies, or quick lime with phoſphorus, and the addition of a ſmall quantity of water. What remained in the retort after the diſengagement

ment of the gas had entirely ceaſed, was nothing but pure water, without ſmell, taſte, or colour; and in every reſpect ſimilar to that newly diſtilled.

When expoſed to the air of the atmoſphere, it ſoon depoſited a remarkable quantity of red oxide of phoſphorus, and at the ſame time emitted ſome hydrogen gas, but which was no longer ſuſceptible of inflammation, except by the contact of an ignited body. By keeping the ſolution expoſed to the air for a length of time, and occaſionally increaſing or renewing the points in contact by a gentle agitation, it becomes completely decompoſed, or in other words, it reſolves itſelf entirely into phoſphoreous oxide, and pure hydrogen gas.

The tinctures of turnſol and violets do not undergo any change in their colour by contact with the liquid phoſphorated hydrogen, a proof that this fluid is neither acid nor alkaline.

The ſulphuric and nitric acids, and the muriatic acid either ſimple or oxygenated, on being added to this liquor, produced no remarkable effect; neither did the application of potaſh, ſoda, nor ammonia.

The oxydes of mercury and lead were ſpeedily reduced, and immediately converted into metallic phoſphurets, by their mixture with the ſolution of phoſphorated hydrogen gas.

When poured into the nitrate of ſilver this ſolution inſtantly produces a black precipitate in great abundance, which does not change its colour, and which before the blow pipe exhibits all the characters of metallic phoſphurets.

When brought into contact with the nitric ſolution of mercury, it alſo produced a very conſiderable precipitate in great abundance, which is at firſt black, but ſoon becomes white and cryſtallized, in proportion as it paſſes from the ſtate of phoſphuret to that of the mercurial phoſphate, by abſorbing oxygen either from the nitric acid in which it takes place, or the atmoſpheric air with which it is in contact.

The ſolution of lead by the nitric acid is likewiſe decompoſed by the hydro-phoſphorated liquor, but with leſs activity than the ſolutions of ſilver and mercury. In this decompoſition ſome phoſphuret of lead is alſo produced, which in time becomes converted into phoſphate.

The ſulphate of copper alſo, after a certain time, exhibits a conſiderable quantity of black precipitate, when a portion of phoſphorated hydrogen gas is added to the ſolution. This precipitate, like that obtained by the decompoſition of the nitrate of ſilver, retains its black colour; which ſeems to ſhew, that it cannot without great difficulty be converted into phoſphate.

The ſulphate of iron did not appear to be affected by this liquid.

Nitrate of arſenic, when poured into this fluid, does not undergo any perceptible decompoſition, till the expiration of ſeveral days; but at length a precipitate is generated of a beautiful yellow colour, in the form of ſmall grains, which is capable of remaining expoſed to the air for a length of time, without undergoing any change. This precipitate is the arſenical phoſphuret.

CONCLUSIONS.

CONCLUSIONS.

The following are the new properties which I conceive I have been the firſt to diſcover in the phoſphorated hydrogen gas.

1. This gas is capable of uniting with diſtilled water in the proportion of about one-fourth of its bulk, when the ſolution is effected at about ten degrees of the French thermometer [*].

2. It communicates to the water in which it is diſſolved a ſtrong and diſagreeable ſmell, as well as a bitter taſte; which may at ſome future period be uſed with ſucceſs in the treatment of various diſeaſes, either from the facility with which this preparation is decompoſed, or on account of the effect produced by the phoſphorus it contains, in the formation of animal ſubſtances.

3. When water, purified of its air, is uſed for the purpoſe of diſſolving this gas, and if, when thus diſſolved, it is carefully preſerved in cloſely ſtopped veſſels, it may be kept for a long time without undergoing decompoſition; ſo that by heating the ſolution, the whole of the phoſphorated hydrogen gas which it contains may be diſengaged in the gaſeous ſtate.

4. When water has been by this means deprived of the phoſphorated gas which it had diſſolved, it again returns to its original ſtate of purity; whence it follows that its new properties are derived from the preſence of this gas alone.

Laſtly, this ſolution is capable of ſpeedily reducing ſeveral metallic oxydes, whether alone, or diſſolved by acids, and of forming with them, by double elective attraction, water and metallic phoſphurets: which combinations have only hitherto been obtained in the dry way only; that is to ſay, by heating metals with phoſphorus, or rather by the decompoſition of phoſphoric glaſs, or metallic phoſphates, by means of metals and carbon.

III.

Deſcription of the underground inclined Plane, executed at Walkden Moor in Lancaſhire, by his Grace the Duke of Bridgewater. By the Rev. FRANCIS EGERTON [†].

I BEG leave to preſent to the Society an account of the under-ground inclined plane, which the Duke of Bridgewater has lately made at Walkden-Moor, between Worſley and Bolton, in Lancaſhire. To this account I have ſubjoined two plans, with a table of reference to each.

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[*] It is probable that at the temperature of zero, a greater quantity might be diſſolved by the water; but for want of a ſufficient quantity of phoſphorus, I was not able to aſcertain this fact.

[†] Extracted from the Tranſactions of the Society for the Encouragement of Arts, &c. for the year 1800. The letter is addreſſed to their ſecretary, and the Society has voted their annual gold medal to his Grace.

At Worſley the Duke of Bridgewater's navigation begins; it goes weſt to Leigh, and eaſt to Mancheſter, where it locks up into the Rochdale canal. In its way to Mancheſter, it turns out, in a weſtern direction, near Longford Bridge, to meet the Grand Trunk Canal, above Preſton Brook; and from thence it goes north-weſt to Runcorn, where it locks down into the Merſey, in the tide-way to Liverpool.

To this navigation above-ground, which, in all its directions, is extended through a length of * forty miles, upon one level, without tunnel or lock, except the locks at the extremities. At Worſley, an underground navigation is joined, which goes to the different mines of coal under Walkden-Moor; from which mines, by theſe navigations above-ground and under-ground, Mancheſter and various other places are ſupplied with that valuable article.

The canals of this under-ground navigation lie upon two levels, or ſtories.

The lower is upon the ſame level with the open navigation, which it joins at Worſley; and conſiſts, in the different lines which it purſues to the different ſeams of coal, of near twelve miles of tunnelling.

The higher is thirty-five yards and a half perpendicular height above the level of the lower, and varies from thirty-eight to ſixty-one perpendicular yards below the ſurface of the earth, and conſiſts of near ſix miles of tunnelling.

The tunnelling of each level is ten feet four inches wide, and eight feet ſix inches deep; and the depth of water, three feet ſeven inches.

Before a communication was made by an inclined plane, the coals were diſcharged by hand from the boats on the higher level, and were let down the pits in tubs by an engine and a break-wheel into thoſe upon the lower. To convey the boats themſelves from the canals of the higher level into that of the lower, was the intent of making this underground inclined plane. By the help of this machinery, the whole buſineſs is now done at once, without diſcharging or damaging the coal, and at one fourth of the expence: for the boats of the higher level are bodily let down the inclined plane, and are floated from the foot of it through nearly three miles, in a ſtrait line, of the lower level canal, into the open navigation at Worſley: and, whereas they were before obliged to be drawn up to the ſurface of the earth at great inconvenience and expence, to be repaired at a work-ſhop on Walkden-Moor, they now come of themſelves, in their courſe of buſineſs, to be repaired at the great dock-yard at Worſley.

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* *Forty miles upon one level.*] Adding to theſe forty miles, nearly twelve miles of the Duke of Bridgewater's underground navigable canal, which lie upon his lower main level, and including eighteen miles of the Grand Trunk Canal betwixt the loweſt lock between Middlewich and Preſton-Brook, there are ſeventy miles of navigable canal, without a lock, upon one level, eighty-two feet above low-water mark; whereby a communication is obtained between London, Liverpool, Briſtol, and Hull. At this loweſt lock the Grand Trunk Navigation locks down, to be upon a level with the Duke of Bridgewater's.

The place where the inclined plane is conſtructed, is adapted in a ſingular way for the purpoſe. There is a bed of white rock, or grit, eight yards twelve inches deep, which dips one in four, lying exactly in the direction moſt convenient for the communication between the two levels; which bed of rock is hollowed into a tunnel, driven upon the riſe of the metals, by blaſting with gunpowder, and working it down with wedges and hammers. In this tunnel, formed through a rock reaching from the lower to the higher level, the inclined plane is fixed; and, by its being in the heart of a rock, the whole workmanſhip can be pinned, ſecured, and compacted together at the top, bottom, and ſides, moſt effectually:—an advantage which no inclined plane above-ground can have, and which renders this a ſingular production, no where perhaps to be imitated.

The run of the inclined plane is one hundred and fifty-one yards, beſides eighteen yards, the length of the locks, at the north or upper end: and the fall is one in four, correſponding with the dip of the rock.

Of theſe one hundred and fifty-one yards, about ninety-four yards are formed into a double waggon-way, in order to let two boats, namely, the empty and the loaded boat, paſs up and down; and are divided by a brick wall, ſupporting the roof, in which are openings for a perſon to eſcape out of the way of the boats; which double-waggon way joins in one, about fifty-ſeven yards from the lower level.

The whole width of the double waggon-way is nineteen feet; and of the ſingle waggon-way, after the junction, ten feet.

Theſe waggon-ways are ſupplied with iron rails, or gullies, laid on ſleepers, down the whole run; and the height of the roof, above the iron rails, is eight feet.

At the top of the inclined plane there is a double lock, or rather two locks, ſide by ſide, formed in the heart of the ſame rock, which deliver the loaded boats from the higher level down the inclined plane, and receive the empty boats from the lower. The length of that part of the tunnel in which theſe are formed, is eighteen yards; the width or diameter, twenty feet ſix inches; and the height of the roof, at the north end and above the locks, at *d d*, *Plate XXI. Fig.* 1. twenty-one feet, to admit the break-wheel.

The bottom, or ſouth end of the inclined plane, is ſix feet nine inches under the ſurface of the water, where the loaded boat floats off the carriage upon the canal of the lower level.

The depth of the locks, under water, at the north end, is four feet ſix inches; at the ſouth end it is eight feet.

The wall between the locks is nine inches above the ſurface of the level water; its breadth is three feet.

The diameter of the horizontal main ſhaft, upon which the rope works to let the loaded boats down, and to draw the empty boats up, is four feet eleven inches, and its circumference is fifteen feet five inches. The main-rope is two inches and a half in diameter, and ſeven inches and a half in circumference. It is lapped round with a ſmall cord of about an inch in circumference, for the length of about one hundred and five yards, to prevent its wearing, which it does chiefly when it drags upon the bottom, when at work, at the place where the waggon-ways unite; and, for the ſame purpoſe, rollers of eight inches diameter are fixed at intervals down the run of the inclined plane. Moreover, a hollow caſt-iron roller of eight inches and a half diameter is fixed acroſs the weſt lock, parallel to the upper weſt lock-gate, and near the north end of the lock, but half a yard higher than the gate, in order to bear up the rope, and to prevent it from ſwagging.

A hold-faſt rope is faſtened to the main-rope, to ſtay each boat upon its waggon, as they go up or down. It is marked *k k*, in *Fig.* 2, *Plate XXI.* and its uſes are more particularly detailed in the table of reference, at *k k*, to that plate.

Upon this horizontal main-ſhaft is a break-wheel above mentioned, which regulates the motion of the loaded boat going down the inclined plane.

The number of iron teeth, or cogs, in the ſpur-wheel, which is faſtened to the ſide of the break-wheel, is three hundred and ſeventy-two; and the little nut-wheel, *No.* 3, *Fig.* 4. which ſets it in motion, contains eleven teeth, or cogs. The nut-wheel is ſupported by two uprights from the pillar to the roof, and works between them. Two winches or handles, *No.* 4 4, *Fig.* 4. on its axis, put the main-ſhaft, *d d*, *Fig.* 2. or *No.* 1, *Fig.* 4. in motion. The power of both united enables a man, who uſes a force equal to forty pounds weight, to ſet forward two tons upon the waggon-road: and this force, multiplied at the winches or handles, may be uſed to ſet forward the loaded boat out of one lock, and to bring the empty boat into the other. The boats being thus put in motion, the little nut-wheel is diſengaged from the main-ſhaft, by a ſlide drawing the little nut ſideways, ſo as to diſengage the teeth, or cogs, from the cogs of the ſpur-wheel. The weight of four tons going down bring up about one.

The ſpur-wheel, however, which is faſtened to the break-wheel, *No.* 2, *Fig.* 4. is ſeldom uſed, as it is occaſionally only put in motion to regulate the ſtretch of the ropes when new, and to draw the light boat into the lock, when, at any time, it may happen to be over-weighted with materials, ſuch as mortar, props, ſlabs, &c. for the uſe of the higher level collieries, and will not move of itſelf, upon a balance, out of the lower level.

The length of the carriage, or cradle, is thirty feet; its width is ſeven feet four inches. It moves upon four ſolid caſt-iron rollers, which run upon caſt-iron plates; on one ſide of each of which there are iron creſts, which ſtand two inches higher than the plates, and prevent the carriage from running off the road.

The weight of neat coal, contained in the loaded boat, is about twelve tons: the boat weighs about four tons; and the carriage, or cradle, in which the boat is placed, when conveyed down the inclined plane, is about five tons:—in all about twenty-one tons.

At this inclined plane thirty loaded boats are now let down, with eaſe, in about eight hours; that is to ſay, four boats are let down in a little more than an hour. The boats uſed in theſe collieries are of different ſizes and dimenſions; ſome will carry ſeven, ſome eight and a half, ſome twelve tons.

The weight of neat coal, independently of the weight of the carriage and boats which is let down the inclined plane, in twelve-ton boats, in eight hours, will conſequently be three hundred and ſixty tons. The weight of the carriage, ſuppoſe five tons, let down in the ſame time, will be one hundred and fifty tons; and the weight of the boat, ſuppoſe four tons, thirty times down, in eight hours, will be one hundred and twenty tons:—in all ſix hundred and thirty tons down in eight hours.

The weight of the carriage thirty times up, and thirty boats up, in eight hours, will be

Carriage, at 5 tons, 30 times up = 150 tons
Boat, at 4 tons, 30 times up = 120 tons

In all 270 tons
up in eight hours.

So that there will be 630 tons down
270 tons up

In all 900 tons moved at the inclined plane, in 8 hours, excluſive of an indeterminate quantity of materials occaſionally brought up for the uſe of the higher level collieries.

The various feeders which are looſened by opening the coals in the higher level collieries, as well as three ſufficient reſervoirs, which may occaſionally be reſorted to, and uſed in a dry ſeaſon, keep the higher level always to its height, and afford a conſtant ſupply of water to fill the locks, for the purpoſe of working the inclined plane.

This inclined plane was begun in September, 1795; it was finiſhed, and in uſe, in October, 1797.

Of this, as of moſt of his other great works, the Duke of Bridgewater was himſelf the planner and contriver:—to project greatly, and to execute completely, are the perfection of genius.

The ſingularity of the place in which it is conſtructed; the original boldneſs of the deſign; the ingenuity and mechaniſm diſplayed in planning and executing it; the diſpatch with which it has been finiſhed; the ſimplicity, beauty, and harmony of its parts, tending to one united whole; and, above all, the perfection to which it is proved to have been brought, now that it is practically in uſe; render it equally aſtoniſhing with any other of the ſtupendous works which have been ſo ably planned, and ſo ſucceſsfully executed, by the firſt projector and patriotic father of inland navigation.

I have the honour to be, &c.

FRANCIS H. EGERTON.

Bridgewater-Houſe, March 5, 1800.

Reference to Plate XXI. being a Plan and Section of his Grace the DUKE OF BRIDGEWATER'S *Under-ground Inclined Plane, at Walkden-Moor Colliery, near Worſley, in Lancaſhire.*

Figure 2.

a to *b*, Dip of the metals and waggon-road on the under-ground inclined plane. From *b*, on the lower level, to the mouth of the tunnel, is three miles.

A, The eaſt lock.

B, The weſt lock.

C, Repreſents a ſection of the lock; the dotted line ſhows the horizontal depth, and the black line under it, the ſlope upon which the waggon-wheels run to receive the loaded boat, or to bring the empty boat into the lock.

d d, The main-ſhaft, four feet eleven inches diameter, upon which the ropes work to wind the boats up and down; and here alſo the break-wheel is faſtened on, together with a ſpur-wheel, and a nut-wheel. See *Fig*. 4. *No*. 1.

e, A paſſage betwixt the higher level and the locks.

f f, A loaded boat going down, and an empty boat going up the under-ground inclined plane.

G, A brick wall from the ſole to the top of the inclined plane, in order to give additional ſupport to the roof.

h h h h, Openings through the brick wall *G*, into which a perſon may ſtep out of the way of the boats, at the time they are paſſing up and down.

i, A bell, which is rung by the rope dotted to *b*, upon the lower level, at the bottom of the under-ground inclined plane, to give notice when the empty boat is upon the waggon, or cradle, and when the men below are ready, that the loaded boat may be let down by the men above.

k k, Holdfaſt-ropes faſtened to the main-ropes. and hooked on to a ring at the ſouth end of each boat, as it goes up or down, in order to ſtay the boats upon the waggon or cradle, that they may not ſwag, or ſlip off. Theſe holdfaſt-ropes are ſpliced on to the end of the main-ropes, and run above and between the two bridle-ropes when they are faſtened to the iron uprights, which are upon each ſide of the waggons, or cradles: and they run over the north end of the boat, to be hooked on to the ſouth end.

l l, The bridle-ropes faſtened to the main-ropes at *O*, and ſecured to two iron uprights upon each ſide of the waggon, or cradle.

O O, The places where the main-ropes, the bridle-ropes, and the holdfaſt-ropes, are faſtened all together.

No. 1. An open ſpace driven into the ſide of the lock *A*, to which a pit is ſunk from the higher level, in order to convey the water out of the locks down to the lower level, and alſo to force a current of freſh air into the lower level collieries.

No. 2. A paddle to let the water out of the lock *A*, into the pit *No.* 1.

No. 3. A paddle to let the water out of the lock *B*, through a culvert, repreſented by dotted lines, under the lock *A*, into the pit *No.* 1.

No. 7, 7. Paddles in the lock-gates, to let the water out of the higher level into the locks.

No. 8, 8. The two north lock-gates, one to each lock, which turn upon the heels of the gates, and ſwing round when they are opened or ſhut.

No. 10, 10. Two ſtops or cloughs, one to each lock, which ſerve as lock-gates to the ſouth end, and are raiſed and let down by a windlaſs.

S, A ſtop, which is uſed occaſionally when the lock-gates want repairing.

T, The place where the boats which are to paſs to or from the lower ſingle waggon-way are directed, at pleaſure, into either part of the double waggon-way, by a moveable iron ſleeper or plate at that point, upon which ſleeper or plate the wheels of the boat-carriage or cradle run.

Table of Reference to Plate XXI. Fig. 4, *of his Grace the* DUKE OF BRIDGEWATER'S *Under-ground Inclined Plane, at Walkden-Moor Colliery, near Worſley, in Lancaſhire.*

1. Main-ſhaft, on which the rope laps.

2. Break-wheel, on one ſide of which the ſpur-wheel is faſtened.

3. Nut-wheel, out of geer, but which ſlides into the ſpur-wheel, when uſed to draw the empty boat into the lock occaſionally, and which is ſupported by two uprights from the pillar to the roof.

4, 4. Winches or handles, to work the nut and ſpur-wheel.

5, 5. The main-ropes faſtened to the boats, and which are lapped to prevent their wearing.

6. The ſpur-wheel, which is faſtened on one ſide of the break-wheel; and on which break-wheel is a ſtrong iron-jointed timber brace, which, according to the preſſure given thereto by the man who attends it, will allow the loaded boat to deſcend quick or ſlow, or detain it in its paſſage.

7, 7. Paddles in the lock-gates, to let the water out of the higher level into the lock.

8. A hollow caſt-iron roller, to prevent the main-ropes from ſwagging.

9. Shroud-wheel, to prevent the ropes going over the end of the main-ſhaft, ſlipping off, jerking, or breaking. This ſtands three inches above the main-ſhaft.

IV.

Concerning the Engine for raiſing Water by the lateral Communication of Motion. In a Letter from the Inventor, Mr. WILLIAM CLOSE.

LETTER III.

To MR. NICHOLSON.

S I R, *Dalton, Jan.* 12, 1801.

SINCE the date of my ſecond letter on raiſing water by the lateral communication of motion, I have occaſionally turned my thoughts to the ſubject, and made ſome improvement in the plan of the apparatus for producing a ſeries of elevations, which I ſhall communicate in the preſent letter.

The height to which the lateral communication of motion will cauſe the preſſure of the atmoſphere to raiſe water in one column, depends upon the height of the water in the reſervoir above the conical tube. When the height of the charge is five feet, the ſtraiteſt diameter of the conical tube equal to ſix inches, and its other dimenſions adapted to the preſſure; the extent of the column may be eſtimated at eleven feet. Under ſimilar circumſtances, when water is to be raiſed to the height of twenty-three feet, the following method may be purſued.

Raiſe the water four times by the preſſure of the atmoſphere, through four vertical tubes, each riſing to the height of ſeven feet and an half above the brim of the ciſtern from which it is to receive water, as is repreſented in Fig. 3, Pl. XIII. When the charge of the reſervoir is five feet, it will be of no uſe to make the tubes much longer than ſeven feet and an half, as the aſcent through the ſpace above will be ſlow, and the extenſion of the elevation in this way will ſeldom compenſate for the loſs of time.

Fig. 1, in Pl. XXI. repreſents the firſt veſſel above the reſervoir; C part of the water tube, and E part of the air pipe which leads to the conical tube.

Let the air pipe E be made four times the width that would be required for the elevation of one column, and let the valve repreſented by *m* be adopted to its top.

The air pipe repreſented by G ſhould be made wide for this veſſel; in all thoſe above it may be omitted.

Into the top of the veſſel D inſert three ſmall air pipes O P Q, and carry each of them to a different veſſel on the top of a water tube. The length of each of theſe above the ciſtern from which it is to raiſe a column of water, muſt rather exceed the length of the loweſt rarefying tube above the loweſt reſervoir. They will require no valves.

When the reſt of the apparatus is completed upon the principles before laid down, the lateral communication of motion will rarefy the air equally in all its parts, and the water will aſcend through all the vertical tubes, into and nearly fill the veſſels upon their tops.

But before it quite reaches the tops of the tubes O P Q, it will begin to deſcend in a full ſtream through the loweſt rarefying tube E, which is the ſhorteſt: the lateral action now ceaſing to rarefy the air, the water will riſe no higher, but will remain at the ſame elevation above each of the ciſterns, until the external valves ſhall open. After this the valve *m* will cloſe the end of its tube, and prevent the diminution of the jet; the air from without will enter the loweſt veſſel D, through the tube G; part of it will aſcend from this veſſel through the empty branches O P Q of the rarefying tube into the veſſels above; and at the ſame time the raiſed water will flow out of all the veſſels into different ciſterns.

If all the veſſels are connected together by their air pipes, inſtead of each being connected to the loweſt, the ſame equality of action will take place. In both caſes, as the elevation of the water in each of the tubes muſt correſpond with the degree of rarefaction in the air before it can begin to aſcend, it is evident that the tubes will all be full at the ſame time, although they may contain columns of unequal height, when the air begins to rarefy: the imperfection of one or two of the valves to the water tubes can therefore be attended with little inconvenience.

If the air pipes were ſeparately connected with the conical tube, ſome of the veſſels might be filled with water before others: this inequality, indeed, might be removed by bringing all the tubes into one; but in either caſe, if the external valves were to remain too long cloſe, the raiſed water would deſcend through the air pipes, and waſte rapidly from ſeveral ciſterns at once. The arrangement that has been recommended will be free from this inconvenience, however long the valves may remain ſhut.

By proportioning the widths of the water tubes and the capacities of the veſſels upon their tops, it will be eaſy to raiſe a little more water into the firſt ciſtern, than will be delivered into the ſecond; and a little more into the ſecond, than will be carried into the third; and ſo on, in order to prevent any of the lower ciſterns from becoming empty of raiſed water. The waſte thus incurred will be very inconſiderable. If, however, more water ſhould be raiſed into any of the higher ciſterns than thoſe below can long afford, it it will be proper to make part of it deſcend through tubes for the purpoſe, and repleniſh the lower ones.

As the jet will bear the admiſſion of ſome air without diminution, it is not neceſſary that the valve *m* ſhould perfectly cloſe the end of the air tube: if it did, it would not be eaſy to open. When a cock is uſed inſtead of this valve, it muſt be fixed in the tube at ſome depth below the ſurface of the water in the reſervoir, and a weight hung upon an arm or lever proceeding from it, to keep open the paſſage through the tube. This weight will alſo ſerve to counterbalance the empty veſſel which contains the ſyphon, and opens the valves. A cock can be attended with little inconvenience in this ſituation: if, after long continued uſe, a little water ſhould be admitted, it will cauſe no perceptible diminution of power.

A ſecond and third ſeries of elevations may be cauſed by connecting more air pipes with the conical tube; but as the introduction of ſo many pipes into the throat would diminiſh its width, and greatly retard the expenditure, it will be beſt to introduce them all through holes made in the circumference of the tube, and to take off their ends even with its inſide. When water is thus raiſed in an additional number of columns, the water tubes ſhould be made ſtill ſhorter than was recommended before, and to prevent the water which is raiſed by the ſecond ſeries from being waſted, the rarefying tubes ſhould be carried up to ſuch height above each veſſel, that the column may counterbalance the active preſſure of the atmoſphere before it arrives at the top. Recourſe may be had to this method when ſeveral conical tubes are employed, as is repreſented in Fig. 3, Pl. XIII. In this laſt caſe, however, it will be unneceſſary to make any addition to the loweſt veſſel, as the expenditure can only take place from the loweſt ciſtern.

In all the arrangements that have been hitherto deſcribed, the ſtream through the conical tube is ſuffered to run one half of its time without producing any uſeful effect. It will, however, be of very great advantage to keep the lateral action conſtantly in work in rarefying the air in different parts of the apparatus. The raiſed water may then be let out of one ſet of veſſels while it is riſing into another ſet, and the water that flows out of one deſcending veſſel, and has opened the valves of the firſt ſet, may, by being carried into a ſmall ciſtern a ſecond time, and delivered from thence by a ſyphon into another deſcending veſſel, be made to open the valves of the ſecond ſet.

As it may ſometimes happen that all the valves may be half open, and much air be thus admitted into the ſtream, it is likely that cocks fixed in the rarefying tubes, and placed in water, would on this occaſion be preferable to valves, particularly when the rarefying tubes can be filled with water, for then no air can deſcend into the jet. I think, however, that this can only be neceſſary when ſeveral air pipes are connected with the conical tube, and the air would enter the ſtream on all ſides.

In this manner by the aſſiſtance of the lateral action of a ſtream of water through one conical tube, and the preſſure of the atmoſphere, an uſeful quantity of water may be raiſed to a conſiderable height.

I am, SIR,

Your humble ſervant,

WILLIAM CLOSE.

V.

On the Power of penetrating into Space by Teleſcopes; with a comparative Determination of the Extent of that Power in natural Viſion, and in Teleſcopes of various Sizes and Conſtructions; illuſtrated by ſelect Obſervations. By WILLIAM HERSCHEL, LL. D. F. R. S.*

IT will not be difficult to ſhew that the power of penetrating into ſpace by teleſcopes is very different from magnifying power, and that, in the conſtruction of inſtruments, theſe two powers ought to be conſidered ſeparately.

In order to conduct our preſent inquiry properly, it will be neceſſary to examine the nature of luminous bodies, and to enter into the method of viſion at a diſtance. Therefore, to prevent the inaccuracy that would unavoidably ariſe from the uſe of terms in their common acceptation, I ſhall have recourſe to algebraic ſymbols, and to ſuch definitions as may be neceſſary to fix a preciſe meaning to ſome expreſſions which are often uſed in converſation, without much regard to accuracy.

By luminous bodies I mean, in the following pages, to denote ſuch as throw out light, whatever may be the cauſe of it: even thoſe that are opaque, when they are in a ſituation to reflect light, ſhould be underſtood to be included; as objects of viſion they muſt throw out light, and become intitled to be called luminous. However, thoſe that ſhine by their own light may be called ſelf-luminous, when there is an occaſion to diſtinguiſh them.

The queſtion will ariſe, whether luminous bodies ſcatter light in all directions equally; but, till we are more intimately acquainted with the powers which emit and reflect light, we ſhall probably remain ignorant on this head. I ſhould remark, that what I mean to ſay, relates only to the phyſical points into which we may conceive the ſurfaces of luminous bodies to be divided; for, when we take any given luminous body in its whole conſtruction, ſuch as the ſun or the moon, the queſtion will aſſume another form, as will appear hereafter.

That light, flame, and luminous gaſes are penetrable to the rays of light, we know from experience; † it follows therefore, that every part of the ſun's diſk cannot appear equally luminous to an obſerver in a given ſituation, on account of the unequal depth of its luminous atmoſphere in different places*. This regards only bodies that are ſelf-luminous. But the greateſt inequalities in the brightneſs of luminous bodies in general, will undoubtedly be owing to their natural texture; which may be extremely various, with regard to their power of throwing out light more or leſs copiouſly.

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* Philoſophical Tranſactions, 1800, p. 49.

† In order to put this to a proof, I placed four candles behind a ſcreen, at $\frac{3}{4}$ of an inch diſtance from each other, ſo that their flames might range exactly in a line. The firſt of the candles was placed at the ſame diſtance from the ſcreen, and juſt oppoſite a narrow ſlit, two-thirds of an inch long, and $\frac{1}{4}$ broad. On the other ſide of the ſcreen I fixed up a book, at ſuch a diſtance from the ſlit that, when the firſt of the candles was lighted, the letters might not be ſufficiently illuminated to become legible. Then, lighting ſucceſſively the ſecond, third, and fourth candles, I found the letters gradually more illuminated, ſo that at laſt I could read them with great facility; and, by the arrangement of the ſcreen and candles, the light of the ſecond, third, and fourth, could not reach the book, without penetrating the flames of thoſe that were placed before them.

Brightneſs, I aſcribe to bodies that throw out light; and thoſe that throw out moſt are the brighteſt.

It will now be neceſſary to eſtabliſh certain expreſſions for brightneſs in different circumſtances.

In the firſt place, let us ſuppoſe a luminous ſurface throwing out light, and let the whole quantity of light thrown out by it be called L.

Now, ſince every part of this ſurface throws out light, let us ſuppoſe it divided into a number of luminous phyſical points, denoted by N.

If the copiouſneſs of the emiſſion of light from every phyſical point of the luminous ſurface were equal, it might in general be denoted by c; but, as that is moſt probably never the caſe, I make C ſtand for the mean copiouſneſs of light thrown out from all the phyſical points of a luminous object. This may be found in the following manner: let c expreſs the copiouſneſs of emitting light, of any number of phyſical points that agree in this reſpect; and let the number of theſe points be n. Let the copiouſneſs of emiſſion of another number of points be c', and their number n'. And if, in the ſame manner, other degrees of copiouſneſs be called c^2, c^3, *&c.* and their numbers be denoted by n^2, n^3, *&c.* then will the ſum of every ſet of points, multiplied by their reſpective copiouſneſs of emitting light, give us the quantity of light thrown out by the whole luminous body. That is, $L = cn + c'n' + c^2n^2$, *&c.*; and the mean copiouſneſs of emitting light, of each phyſical point, will be expreſſed by $\frac{cn + c'n' + c^2n^2, \&c.}{N} = C$.

It is evident that the mean power, or copiouſneſs of throwing out light, of every phyſical point in the luminous ſurface, multiplied by the number of points, muſt give us the whole power of throwing out light, of the luminous body. That is $CN = L$.

I ought now to anſwer an objection that may be made to this theory. Light, as has been ſtated, is tranſparent; and, ſince the light of a point behind the ſurface of a flame will paſs through the ſurface, ought we not to take in its depth, as well as its ſuperficial dimenſions? In anſwer to this, I recur to what has been ſaid with regard to the different powers of throwing out light, of the points of a luminous ſurface. For, as light muſt be finally emitted through the ſurface, it is but referring all light ariſing from the emiſſion of points behind the ſurface, to the ſurface itſelf, and the account of emitted light will be equally true. And this will alſo explain why it has been ſtated as probable, that different parts of the ſame luminous ſurface may throw out different quantities of light.

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* See the Paper on the Nature and Conſtruction of the Sun. Phil. Tranſ. for 1795, page 46.

Since, therefore, the quantity of light thrown out by any luminous body is truly repreſented by CN, and that an object is bright in conſequence of light thrown out, we may ſay that brightneſs is truly defined by CN. If however, there ſhould at any time be occaſion for diſtinction, the brightneſs ariſing from the great value of C, may be called the intrinſic brightneſs; and that ariſing from the great value of N, the aggregate brightneſs; but the abſolute brightneſs, in all caſes, will ſtill be defined by CN.

Hitherto we have only conſidered luminous objects, and their condition with regard to throwing out light. We proceed now to find an expreſſion for their appearance at any aſſigned diſtance; and here it will be proper to leave out of the account, every part of CN which is not applied for the purpoſe of viſion. L repreſenting the whole quantity of light thrown out by CN, we ſhall denote that part of it which is uſed in viſion, either by the eye or by the teleſcope, l. This will render the concluſions that may be drawn hereafter more unexceptionable; for, the quantity of light l being ſcattered over a ſmall ſpace in proportion to L, it may reaſonably be looked upon as more uniform in its texture; and no ſcruples about its inequality will take place. The equation of light, in this preſent ſenſe, therefore, is $CN = l$.

Now, ſince we know that the denſity of light decreaſes in the ratio of the ſquares of the diſtances of the luminous objects, the expreſſion for its quantity at the diſtance of the obſerver D, will be $\frac{l}{D^2}$.

In natural viſion, the quantity l undergoes a conſiderable change, by the opening and contracting of the pupil of the eye. If we call the aperture of the iris a, we find that in different perſons it differs conſiderably. Its changes are not eaſily to be aſcertained; but we ſhall not be much out in ſtating its variations to be chiefly between 1 and 2 tenths of an inch. Perhaps this may be ſuppoſed under-rated; for the powers of viſion, in a room completely darkened, will exert themſelves in a very extraordinary manner. In ſome experiments on light, made at Bath, in the year 1780, I have often remarked, that after ſtaying ſome time in a room fitted up for theſe experiments, where on entering I could not perceive any one object, I was no longer at a loſs, in half an hour's time, to find every thing I wanted. It is however probable that the opening of the iris is not the only cauſe of ſeeing better after remaining long in the dark; but that the tranquillity of the retina, which is not diſturbed by foreign objects of viſion, may render it fit to receive impreſſions ſuch as otherwiſe would have been too faint to be perceived. This ſeems to be ſupported by teleſcopic viſion; for it has often happened to me, in a fine winter's evening, when, at midnight, and in the abſence of the moon, I have taken ſweeps of the heavens, of four, five, or ſix hours duration, that the ſenſibility of the eye, in conſequence of the excluſion of light from ſurrounding objects, by means of a black hood which I wear upon theſe occaſions, has been very great; and it is evident, that the opening of the iris would have been of no ſervice in theſe caſes, on account of the diameter of the optic pencil, which, in the 20 feet teleſcope, at the time of ſweeping, was no more than ,12 inch. The effect of this increaſed ſenſibility was ſuch, that if a ſtar of the 3d magnitude came towards the field of view, I found it neceſſary to withdraw the eye before its entrance, in order not to injure the delicacy of viſion acquired by long continuance in the dark. The tranſit of large ſtars, unleſs where none of the 6th or 7th magnitude could be had, have generally been declined in my ſweeps, even with the 20 feet teleſcope. And I remember, that after a conſiderable ſweep with the 40 feet inſtrument, the appearance of Sirius announced itſelf, at a great diſtance, like the dawn of the morning, and came on by degrees, increaſing in brightneſs, till this brilliant ſtar at laſt entered the field of view of the teleſcope, with all the ſplendour of the riſing ſun, and forced me to take the eye from that beautiful ſight. Such ſtriking effects are a ſufficient proof of the great ſenſibility of the eye, acquired by keeping it from the light.

On taking notice, in the beginning of ſweeps, of the time that paſſed, I found that the eye, coming from the light, required near 20′, before it could be ſufficiently repoſed to admit a view of very delicate objects in the teleſcope; and that the obſervation of a tranſit of a ſtar of the 2d or 3d magnitude, would diſorder the eye again, ſo as to require nearly the ſame time for the re-eſtabliſhment of its tranquillity.

The difficulty of aſcertaining the greateſt opening of the eye, ariſes from the impoſſibility of meaſuring it at the time of its extreme dilatation, which can only happen when every thing is completely dark; but, if the variation of a is not eaſily to be aſcertained, we have, on the other hand, no difficulty to determine the quantity of light admitted through a teleſcope; which muſt depend upon the diameter of the object-glaſs, or mirror; for, its aperture A may at all times be had by meaſurement.

It follows, therefore, that the expreſſion $\frac{a^2 l}{D^2}$ will always be accurate for the quantity of light admitted by the eye; and that $\frac{A^2 l}{D^2}$ will be ſufficiently ſo for the teleſcope. For it muſt be remembered, that the aperture of the eye is alſo concerned in viewing with teleſcopes; and that, conſequently, whenever the pencil of light tranſmitted to the eye by optical inſtruments exceeds the aperture of the pupil, much light muſt be loſt. In that caſe, the expreſſion $A^2 l$ will fail; and therefore, in general, if m be the magnifying power, $\frac{A}{m}$ ought not to exceed a.

As I have defined the brightneſs of an object to the eye of an obſerver at a diſtance, to be expreſſed by $\frac{a^2 l}{D^2}$, it will be neceſſary to anſwer ſome objections that may be made to this theory. Optical writers have proved, that an object is equally bright at all diſtances. It may, therefore, be maintained againſt me, that ſince a wall illuminated by the ſun will appear equally bright, at whatſoever diſtance the obſerver be placed that views it, the ſun alſo, at the diſtance of Saturn, or ſtill farther from us, muſt be as bright as it is in its preſent ſituation. Nay, it may be urged, that in a teleſcope, the different diſtance of ſtars can be of no account with regard to their brightneſs, and that we muſt conſequently be able to ſee ſtars which are many thouſands of times farther than Sirius from us; in ſhort, that a ſtar muſt be infinitely diſtant not to be ſeen any longer.

Now, objections ſuch as theſe, which ſeem to be the immediate conſequence of what has been demonſtrated by mathematicians, and which yet apparently contradict what I aſſert in this paper, deſerve to be thoroughly anſwered.

It may be remembered, that I have diſtinguiſhed brightneſs into three different ſorts *. Two of theſe, which have been diſcriminated by *intrinſic* and *abſolute* brightneſs, are, in common language, left without diſtinction, In order to ſhew that they are ſo, I might bring a variety of examples from common converſation; but, taking this for granted, it may be ſhewn that all the objections I have brought againſt my theory have their foundation in this ambiguity.

The demonſtrations of opticians, with regard to what I call *intrinſic* brightneſs, will not oppoſe what I affirm of *abſolute* brightneſs; and I ſhall have nothing farther to do than to ſhew that what mathematicians have ſaid, muſt be underſtood to refer entirely to the intrinſic brightneſs, or illumination of the picture of objects on the retina of the eye; from which it will clearly follow, that their doctrine and mine are perfectly reconcilable; and that they can be at variance only when the ambiguity of the word brightneſs is overlooked, and objections, ſuch as I have made, are raiſed, where the word brightneſs is uſed as *abſolute*, when we ſhould have kept it to the only meaning it can bear in the mathematicians' theorem.

The firſt objection I have mentioned is, that the ſun, to an obſerver on Saturn, muſt be as bright as it is here on earth. Now by this cannot be meant, that an inhabitant ſtanding on the planet Saturn, and looking at the ſun, ſhould *abſolutely* receive as much light from it as one on earth receives when he ſees it; for this would be contrary to the well known decreaſe of light at various diſtances. The objection, therefore, can only go to aſſert, that the picture of the ſun, on the retina of the Saturnian obſerver, is as *intenſely* illuminated as that on the retina of the terreſtrial aſtronomer. To this I perfectly agree. But let thoſe who would go farther, and ſay that therefore the ſun is *abſolutely* as bright to the one as to the other, remember that the ſun on Saturn appears to be a hundred times leſs than on the earth; and that conſequently, though it may there be *intrinſically* as bright, it muſt here be *abſolutely* † an hundred times brighter.

The next objection I have to conſider, relates to the fixed ſtars. What has been ſhewn in the preceding paragraph, with regard to the ſun, is ſo intirely applicable to the ſtars, that it will be very eaſy to place this point alſo in its proper light. As I have aſſented to the demonſtration of opticians with regard to the brightneſs of the ſun, when ſeen at the diſtance of Saturn, provided the meaning of this word be kept to the *intrinſic* illumination of the picture on the retina of an obſerver, I can have no heſitation to allow that the ſame will hold good with a ſtar placed at any aſſignable diſtance. But I muſt repeat, that the light we can receive from ſtars is truly expreſſed by $\frac{a^2 l}{D^2}$; and that therefore their abſolute brightneſs muſt vary in the inverſe ratio of the ſquares of their diſtances. Hence I am authoriſed to conclude, and obſervation abundantly confirms it, that ſtars cannot be ſeen by the naked eye, when they are more than ſeven or eight times farther from us than Sirius; and that they become, comparatively ſpeaking, very ſoon inviſible with our beſt inſtruments. It will be ſhewn hereafter, that the viſibility of ſtars depends on the penetrating power of teleſcopes, which, I muſt repeat, falls indeed very ſhort of ſhewing ſtars that are many thouſands of times farther from us than Sirius; much leſs can we ever hope to ſee ſtars that are all but infinitely diſtant.

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* See page 497.

† See the definition of *abſolute* brightneſs, page 497.

If now it be admitted that the expreſſions we have laid down are ſuch as agree with well known facts, we may proceed to viſion at a diſtance; and firſt with reſpect to the naked eye.

Here the power of penetrating into ſpace, is not only confined by nature, but is moreover occaſionally limited by the failure in brightneſs of luminous objects. Let us ſee whether aſtronomical obſervations, aſſiſted by mathematical reaſoning, can give us ſome idea of the general extent of natural viſion. Among the reflecting luminous objects, our penetrating powers are ſufficiently aſcertained. From the moon we may ſtep to Venus, to Mercury, to Mars, to Jupiter, to Saturn, and laſt of all to the Georgian planet. An object ſeen by reflected light at a greater diſtance than this, it has never been allowed us to perceive; and it is indeed much to be admired, that we ſhould ſee borrowed illumination to the amazing diſtance of more than 18 hundred millions of miles; eſpecially when that light, in coming from the ſun to the planet, has to paſs through an equal ſpace, before it can be reflected, whereby it muſt be ſo enfeebled as to be above 368 times leſs intenſe on that planet than it is with us, and when probably not more than one-third part of that light can be thrown back from its diſk *.

The range of natural viſion with ſelf-luminous objects, is incomparably more extended, but leſs accurately to be aſcertained. From our brighteſt luminary, the ſun, we paſs immediately to very diſtant objects; for, Sirius, Arcturus, and the reſt of the ſtars of the firſt magnitude, are probably thoſe that come next; and what their diſtance may be, it is well known, can only be calculated imperfectly from the doctrine of parallaxes, which places the neareſt of them at leaſt 412530 times farther from us than the ſun.

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* According to Mr. Bouguer, the ſurface of the moon abſorbs about two-thirds of the light it receives from the ſun. See *Traité d'Optique*, page 122.

In order to take a ſecond ſtep forwards, we muſt enter into ſome preliminary conſiderations, which cannot but be attended with conſiderable uncertainty. The general ſuppoſition, that ſtars, at leaſt thoſe which ſeem to be promiſcuouſly ſcattered, are probably one with another of a certain magnitude, being admitted, it has already been ſhewn in a former paper*, that after a certain number of ſtars of the firſt magnitude have been arranged about the ſun, a farther diſtant ſet will come in for the ſecond place. The ſituation of theſe may be taken to be, one with another, at about double the diſtance of the former from us.

By directing our view to them, and thus penetrating one ſtep farther into ſpace, theſe ſtars of the ſecond magnitude furniſh us with an experiment that ſhews what phænomena will take place, when we receive the illumination of two very remote objects, equally bright in themſelves, whereof one is at double the diſtance of the other. The expreſſion for the brightneſs of ſuch objects, at all diſtances, and with any aperture of the iris, according to our foregoing notation, will be $\frac{a^2 l}{D^2}$; and a method of reducing this to an experimental inveſtigation will be as follows:

Let us admit that α Cygni, β Tauri, and others, are ſtars of the ſecond magnitude, ſuch as are here to be conſidered. We know, that in looking at them and the former, the aperture of the iris will probably undergo no change; ſince the difference in brightneſs, between Sirius, Arcturus, α Cygni, and β Tauri, does not ſeem to affect the eye ſo as to require any alteration in the dimenſions of the iris; a, therefore becomes a given quantity, and may be left out. Admitting alſo, that the latter of theſe ſtars are probably at double the diſtance of the former, we have D^2 in one caſe four times that of the other; and the two expreſſions for the brightneſs of the ſtars, will be l for thoſe of the firſt magnitude, and $\frac{1}{4} l$ for thoſe of the ſecond.

The quantities being thus prepared, what I mean to ſuggeſt by an experiment is, that ſince ſenſations, by their nature, will not admit of being halved or quartered, we come thus to know by inſpection what phænomenon will be produced by the fourth part of the light of a ſtar of the firſt magnitude. In this ſenſe, I think we muſt take it for granted, that a certain idea of brightneſs, attached to the ſtars which are generally denominated to be of the ſecond magnitude, may be added to our experimental knowledge; for by this means, we are informed what we are to underſtand by the expreſſions $\frac{a^2 l}{\odot^2}$, $\frac{a^2 l}{\overline{\text{Sirius}}|^2}$, $\frac{a^2 l}{\overline{\beta\ \text{Tauri}}|^2}$ We cannot wonder at the immenſe difference between the brightneſs of the ſun and that of Sirius; ſince the two firſt expreſſions, when properly reſolved, give us a ratio of brightneſs of more than 170 thouſand millions to one; whereas the two latter, as has been ſhewn, give only a ratio of four to one.

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[*] Phil. Tranſ. for the year 1796, page 166, 167, 168.

[†] The names of the objects $\odot$, Sirius, β Tauri, are here uſed to expreſs their diſtance from us.

What has been ſaid will carry us, with very little addition, to the end of our unaſſiſted power of viſion to penetrate into ſpace. We can have no other guide to lead us a third ſtep than the ſame beforementioned hypotheſis; in conſequence of which, however, it muſt be acknowledged to be ſufficiently probable, that the ſtars of the third magnitude may be placed about three times as far from us as thoſe of the firſt. It has been ſeen, by my remarks on the comparative brightneſs of the ſtars, that I place no reliance on the claſſification of them into magnitudes *; but, in the preſent inſtance, where the queſtion is not to aſcertain the preciſe brightneſs of any one ſtar, it is quite ſufficient to know that the number of the ſtars of the firſt three different magnitudes, or different brightneſſes, anſwers, in a general way, ſufficiently well to a ſuppoſed equally diſtant arrangement of a firſt, ſecond, and third ſet of ſtars about the ſun. Our third ſtep forwards into ſpace, may therefore very properly be ſaid to fall on the pole-ſtar, on γ Cygni, ε Bootis, and all thoſe of the ſame order.

As the difference, between theſe and the ſtars of the preceding order, is much leſs ſtriking than that between the ſtars of the firſt and ſecond magnitude, we alſo find that the expreſſions $\frac{a^2 l}{\overline{\beta\ \text{Tauri}}|^2}$, and $\frac{a^2 l}{\overline{\text{Polaris}}|^2}$, are not in the high ratio of 4 to 1, but only as 9 to 4, or $2\frac{1}{4}$ to 1.

Without tracing the brightneſs of the ſtars through any farther ſteps, I ſhall only remark, that the diminution of the ratios of brightneſs of the ſtars of the 4th, 5th, 6th, and 7th magnitude, ſeems to anſwer to their mathematical expreſſions, as well as, from the firſt ſteps we have taken, can poſſibly be imagined. The calculated ratio, for inſtance, of the brightneſs of a ſtar of the 6th magnitude, to that of one of the 7th, is but little more than $1\frac{1}{3}$ to 1; but will we find by experience, that the eye can very conveniently perceive it. At the ſame time, the faintneſs of the ſtars of the 7th magnitude, which require the fineſt nights, and the beſt common eyes to be perceived, gives us little room to believe that we can penetrate much farther into ſpace, with objects of no greater brightneſs than ſtars.

But, ſince it may be juſtly obſerved, that in the foregoing eſtimation of the proportional diſtance of the ſtars, a conſiderable uncertainty muſt remain, we ought to make a proper allowance for it; and, in order to ſee to what extent this ſhould go, we muſt make uſe of the experimental ſenſations of the ratios of brightneſs we have now acquired, in going ſtep by ſtep forward: for, numerical ratios of brightneſs, and ſenſations of them, as has been noticed before, are very different things. And ſince, from the foregoing conſiderations, it may be concluded, that as far as the 6th, 7th, or 8th magnitude, there ought to be a viſible general difference between ſtars of one order and that of the next following, I think, from the faintneſs of the ſtars of the 7th magnitude, we are authorized to conclude, that no ſtar, eight, nine, or at moſt ten times as far from us as Sirius, can poſſibly be perceived by the natural eye.

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* Phil. Tranſ. for the year 1796, page 168, 169.

The boundaries of viſion, however, are not confined to ſingle ſtars. Where the light of theſe falls ſhort, the united luſtre of ſidereal ſyſtems will ſtill be perceived. In clear nights, for inſtance, we may ſee a whitiſh patch in the ſword-handle of Perſeus[*], which contains ſmall ſtars of various ſizes, as may be aſcertained by a teleſcope of a moderate power of penetrating into ſpace. We eaſily ſee the united luſtre of them, though the light of no one of the ſingle ſtars could have affected the unaſſiſted eye.

Conſiderably beyond the diſtance of the former muſt be the cluſter diſcovered by Mr. Meſſier, in 1764; north following H. Geminorum. It contains ſtars much ſmaller than thoſe of the former cluſter; and a teleſcope ſhould have a conſiderable penetrating power, to aſcertain their brightneſs properly, ſuch as my common 10-feet reflector. The night ſhould be clear, in order to ſee it well with the naked eye, and it will then appear in the ſhape of a ſmall nebula.

Still farther from us muſt be the nebula between η and ζ Herculis, diſcovered by Dr. Halley, in 1714. The ſtars of it are ſo ſmall that it has been called a nebula[†]; and has been regarded as ſuch, till my inſtruments of high penetrating powers were applied to it. It requires a very clear night, and the abſence of the moon, to ſee it with the natural aye.

Perhaps, among the fartheſt objects that can make an impreſſion on the eye, when not aſſiſted by teleſcopes, may be reckoned the nebula in the girdle of Andromeda, diſcovered by Simon Marius, in 1612. It is however not difficult to perceive it, in a clear night, on account of its great extent.

From the powers of penetrating into ſpace by natural viſion, we proceed now to that of teleſcopes.

It has been ſhewn, that brightneſs, or light, is to the naked eye truly repreſented by $\frac{a^2 l}{D^2}$; in a teleſcope, therefore, the light admitted will be expreſſed by $\frac{A^2 l}{D^2}$. Hence it would follow, that the artificial power of penetrating into ſpace ſhould be to the natural one as A to a. But this proportion muſt be corrected by the practical deficiency in light reflected by mirrors, or tranſmitted through glaſſes; and it will in a great meaſure depend on the circumſtances of the workmanſhip, materials, and conſtruction of the teleſcope, how much loſs of light there will be ſuſtained.

In order to come to ſome determination on this ſubject, I made many experiments with plain mirrors, poliſhed like my large ones, and of the ſame compoſition of metal. The method I purſued was that propoſed by Mr. Bouguer, in his *Traité d'Optique*, page 16, fig. 3.; but I brought the mirror, during the trial, as cloſe to the line connecting the two objects as poſſible, in order to render the reflected rays nearly perpendicular.

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[*] See the catalogue of a ſecond thouſand of new nebulæ and cluſters of ſtars, VI. 33, 34. Phil. Tranſ. Vol. LXXIX. page 251.

[†] In the *Connoiſſance des Temps* for 1783, No. 13, it is deſcribed as a nebula without ſtars.

The reſult was, that out of 100 thouſand incident rays, 67262 were returned; and therefore, if a double reflection takes place, only 45242 will be returned.

Before this light can reach the eye, it will ſuffer ſome loſs in paſſing through the eye glaſs; and the amount of this I aſcertained, by taking a highly poliſhed plain glaſs, of nearly the uſual thickneſs of optical glaſſes of ſmall focal lengths. Then, by the method of the ſame author, page 21, fig. 5. I found, that out of 100 thouſand incident rays, 94825 were tranſmitted through the glaſs. Hence, if two lenſes be uſed, 89918; and, with three lenſes, 85265 rays will be tranſmitted to the eye.

Then, by compounding, we ſhall have, in a teleſcope of my conſtruction with one reflection, 63796 rays, out of 100 thouſand, come to the eye. In the Newtonian form, with a ſingle eye lens, 42901; and, with a double eye glaſs 40681 will remain for viſion.

There muſt always remain a conſiderable uncertainty in the quantities here aſſigned; as a newly poliſhed mirror, or one in high preſervation, will give more light than another that has not thoſe advantages. The quality of metal alſo will make ſome difference; but, if it ſhould appear by experiments, that the metals or glaſſes in uſe will yield more or leſs light than here aſſigned, it is to be underſtood that the corrections muſt be made accordingly.

We proceed now to find a proper expreſſion for the power of penetrating into ſpace, that we may be enabled to compare its effects, in different teleſcopes, with that of the natural eye.

Since then the brightneſs of luminous objects is inverſely as the ſquares of the diſtances, it follows, that the penetrating power muſt be as the ſquare roots of the light received by the eye.

In natural viſion, therefore, this power is truly expreſſed by $\sqrt{a^2 l}$; and, ſince we have now alſo obtained a proper correction x, we muſt apply it to the incident light with teleſcopes.

In the Newtonian and other conſtructions where two ſpecula are uſed, there will alſo be ſome loſs of light on account of the interpoſition of the ſmall ſpeculum; therefore, putting b for its diameter, we have $\overline{A^2 - b^2}$ for the real incident light. This being corrected as above, will give the general expreſſion $\sqrt{x l \times \overline{A^2 - b^2}}$ for the ſame power in teleſcopes. But here we are to take notice: that in refractors, and in teleſcopes with one reflection, b will be $= 0$, and therefore is to be left out.

Then, if we put natural light $l = 1$, and divide by a, we have the general form $\frac{\sqrt{x . \overline{A^2 - b^2}}}{a}$ for the penetrating power of all ſorts of teleſcopes, compared to that of the natural eye as a ſtandard, according to any ſuppoſed aperture of the iris, and proportion of light returned by reflection, or tranſmitted by refraction.

In the following inveſtigation we ſhall ſuppoſe $a = 2$ tenths of an inch, as being perhaps nearly the general opening of the iris, in ſtar-light nights, when the eye has been ſome moderate time in the dark. The value of the corrections for loſs of light will ſtand as has been given before.

(To be concluded in our next.)

VI.

Some Obſervations on the Head of the Ornithorhynchus paradoxus. *By* EVERARD HOME, *Eſq. F. R. S.*[*]

THE ſpecimens of this extraordinary animal which have been ſent to Europe, have been deprived of the internal parts, and the ſkins are moſtly dried, and but badly preſerved. Such imperfect ſpecimens have raiſed the curioſity of the naturaliſt, and excited the ardor of the anatomiſt, without ſatisfying their enquiries.

It was natural, under theſe circumſtances, to reſerve any obſervations which had been made upon this newly diſcovered quadruped, till the entire animal ſhould be brought home preſerved in ſpirit, and enable us to examine the ſtructure of its different organs; but, finding that Profeſſor Blumenbach has been led to believe that it was an animal without teeth, an opinion which muſt have ariſen from the imperfect ſtate of the ſpecimen he examined, it appeared highly proper to do away the miſtake, and lay before this learned Society, ſuch obſervations reſpecting the head of this extraordinary animal, as I have been enabled to make.

My opportunities of examining the *Ornithorhynchus* were procured through Sir Joſeph Banks; who permitted me to have drawings made from the ſkin of one of a very large ſize, and which, from having been preſerved in ſpirit, was more perfect than any of the dried ſpecimens.

Any general deſcription of the beak of this animal, which is its moſt conſpicuous peculiarity, becomes unneceſſary, as the accompanying drawings will give a ſufficiently correct idea of the outward appearances, to anſwer the preſent purpoſe.

It was not permitted to examine the head anatomically; but a ſmaller dried ſpecimen, received from Sir Joſeph Banks, furniſhed me with the following obſervations.

The beak of the *Ornithorhynchus*, when it is curſorily examined, appears ſo ſtrongly to reſemble that of the duck, as to lead to the belief of its being calculated for exactly the ſame purpoſes; it will however be found to differ materially from it, in a variety of circumſtances.

The beak is found, upon examination, not to be the animal's mouth, but a part added to the mouth, and projecting beyond it.

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[*] Philoſophical Tranſactions 1800.

The cavity of the mouth is ſituated as in other quadrupeds, and has two grinding teeth on each ſide, both in the upper and lower jaw; but, inſtead of inciſor teeth, the naſal and palate bones are continued forwards, lengthening the anterior noſtrils, and forming the upper part of the beak; and the two portions of the lower jaw, inſtead of terminating at the ſymphiſis, where they join, become two thin plates, and are continued forwards, forming the under portion of the beak.

This ſtructure differs materially from the bill of the duck, and indeed from the bills of all birds, ſince in them, the cavities of the noſtrils do not extend beyond the root of the bill; and, in their lower portions, which correſpond to the under jaw of quadrupeds, the edges are hard, to anſwer the purpoſe of teeth, and the middle ſpace is hollow, to receive the tongue. But, in this animal, the two thin plates of bone are in the centre; and the parts which ſurround them are compoſed of ſkin and membrane, in which a muſcular ſtructure probably is incloſed.

The teeth have no fangs which ſink into the jaw, as in moſt quadrupeds, but are imbedded in the gum; and have only lateral alveolar proceſſes, from the outer and inner edges of the jaw, to ſecure them in their places, but no tranverſe ones between the two teeth.

The tongue is extremely ſhort, not half an inch long; and the moveable portion not more than a quarter of an inch; the papillæ on its ſurface are long, and of a conical form. When the tongue is drawn in, it can be brought intirely into the mouth; and, when extended, can be projected about a quarter of an inch into the beak.

The organ of ſmell, in this animal, differs, in ſome particulars, from that of quadrupeds in general, as well as of birds. The external openings of this organ are placed nearly at the end of the beak, there being only the lip beyond them; while the turbinated bones are in the ſame relative ſituation to the other parts of the ſkull as in quadrupeds; by which means, there are two cavities the whole length of the beak, ſuperadded to the organ of ſmell.

The turbinated bones in each noſtril are two in number, and are diſtinct from each other. That next the beak is the longeſt, has a more variegated ſurface than in the duck, and has the long axis in the direction of the noſtril; the poſterior one is ſhort, projects farther into the noſtril, and the ridges are in a tranſverſe direction.

The poſterior noſtrils do not open directly under the turbinated bones, as in the duck, but about an inch farther back, and are extremely ſmall; the cavities of the noſe, in this animal, are therefore uncommonly extenſive; they reach from the end of the beak nearly to the occiput.

The beak itſelf is formed by the projecting bones already mentioned, covered with a ſmooth black ſkin, which extends ſome way beyond the bones, both in front and laterally, forming a moveable lip. This lip is ſo ſtrong, that, when dried or hardened in ſpirit, it ſeems to be rigid; but, when moiſtened, is very pliant, and, as has been already mentioned, has probably a muſcular ſtructure. The under portion of the beak has a lip equally broad with the upper: this has a ſerrated edge; but the ſerræ are confined to the ſoft part, not extending to the membrane covering the bone, and are not met with in the upper one. The extent of the lips beyond the bones, is diſtinctly marked in the drawings.

There is a very curious tranverſe fold of the external black ſmooth ſkin, by which the beak is covered, projecting all round, exactly at that part where the beak has its origin. Its apparent uſe ſeems to be to prevent the beak being puſhed further into the ſoft mud, in which its prey may lie concealed, than up to this part, which is ſo broad that it muſt completely ſtop its progreſs.

The nerves that ſupply the beak, in their general courſe, ſize, and number, ſeem very cloſely to correſpond with thoſe of the bill of the duck.

The cavity of the ſkull bears a greater general reſemblance to that of the duck than of quadrupeds: there is a very uncommon peculiarity in it, which is, that there is a bony falx of ſome breadth, but no bony tentorium. This is met with in no quadruped that I know of: it is found in a ſmall degree in ſome birds, as the ſpoon-bill, and the parrot; but not at all ſo as to reſemble the falx in this animal.

The orifice of the eye lids is uncommonly ſmall, for the ſize of the animal; but the eye itſelf was not in a ſtate to be examined.

The external opening of the ear was ſo ſmall as not readily to be perceived: it is ſimply an orifice; but the meatus enlarges conſiderably beyond the ſide of the opening, and paſſes ſome way under the ſkin before it reaches the organ, which in this ſpecimen had been deſtroyed. In the duck, the orifice leading to the ear is very large, when compared with the opening in this animal.

When we conſider the peculiarities in the ſtructure of the noſe of this animal, which lives in water, it is natural to conclude the organ is fitted to ſmell in water, and the external noſtrils are ſo placed, to enable it to diſcover its prey by the ſmell; for that purpoſe, the animal can apply its noſe, with great eaſe, to the ſmall receſſes in which its prey may be concealed.

The ſtructure of the beak is not ſuch as enables it to take a firm hold; but, when the marginal lips are brought together, the animal will have a conſiderable power of ſuction, and in that way may draw its prey into its mouth.

EXPLANATION OF THE FIGURES.

Plate XXII.

Fig. 1. A view of the beak, to ſhow the ſituation of the openings of the external noſtrils, marked *a a*.

Fig. 2. Another view of the beak, expoſing the under portion.

Fig. 3. A lateral view, to ſhow the opening of the lips, and the ſituation of the eye and ear. *a*. The eye *b*. The ear.

Fig. 4. A view of the upper jaw and palate, to ſhew the teeth in their ſituation.

Fig. 5. A ſimilar view of the under jaw.

Fig. 6. The bones which form the beak delineated, and the ſoft ſurrounding parts only marked in outline.

Fig. 7. A ſimilar view of the bones forming the lower portion of the beak.

VII.

Inſtances of ſuſpended Animation in Vegetables. By Mr. JOHN GOUGH.

TO MR. NICHOLSON.

SIR,

Kendal, Jan. 20, 1801.

MR. Baker and Sig. Spalanzani have obſerved and deſcribed various aquatic animalculæ which ſuffer apparent death when deprived of water, but reſume their figure and all their natural functions upon being reſtored, at a future period, to their native element. The latter gentleman has alſo extended his enquiries, in hopes of finding ſimilar examples in the vegetable kingdom, but with little ſucceſs; one or two ſpecies of tremella being all the plants he diſcovered anſwering his expectations.

Notwithſtanding the great induſtry of this celebrated naturaliſt, a plant more perfect in its ſtructure than the gelatinous ſubſtances compoſing the genus tremella, appears to have eſcaped his notice; which poſſeſſes in a high degree the faculty of dying apparently, and reviving again, in order to accommodate itſelf to the viciſſitudes of its ſituation.

The plant upon which nature, or more properly, the author of nature, has beſtowed this remarkable property, is the lemna minor, or common duck's meat. Some remarks which I made accidentally on a pond covered with duck's meat, in the beginning of July, 1797, induced me to imagine that this little plant could hardly preſerve its exiſtence during a long fit of dry weather; unleſs it reſembles the wheel animal, in ſuffering a kind of temporary death when deprived of water; and afterwards experiencing what may be called a reſurrection to a ſecond life, upon receiving a freſh ſupply of its native fluid.

In order to diſcover in what degree the lemna poſſeſſes a power of accommodating itſelf to the inconveniences of its ſituation, the following experiments were made. A quantity of the plant was expoſed for four or five hours to the ſun, in which time it became perfectly dry; a number of leaves which had undergone the preceding proceſs, and appeared dead and withered, were placed, at the end of two days, in a jar of freſh water, where they revived immediately, and continued apparently in a healthy ſtate for three weeks;

when an end was put to the experiment, from a conviction, that putrefaction would have deſtroyed the plants in a much ſhorter time, had the indications of life exhibited by them proved deluſive.

The foregoing fact appeared to me, at the time of its diſcovery, a ſurpriſing inſtance of ſuſpended animation in a vegetable; but a ſimilar trial, made at a ſubſequent period, afforded a more extraordinary proof of the exiſtence of an accommodating principle in the ſame plant. A number of leaves, which were dried with thoſe uſed in the laſt experiment, had been accidentally preſerved in a ſmall box, from the beginning of July, 1797, to the end of March, 1800. Being convinced by trial that the plants were ſtill living, notwithſtanding their long continuance in what may be called a torpid ſtate, I placed ſeveral of them in a glaſs jar, which was furniſhed with a ſmall ſyphon, for the purpoſe of changing the water from time to time. The plants, treated in this manner, not only revived after remaining in a ſtate of apparent death for more than two years, but retained ſufficient vigour to produce complete parts of fructification in Auguſt, which is the proper flowering ſeaſon of this ſpecies of lemna.

Various ſpecies of conferva and tremella might be pointed out as endued with the property under conſideration; but I ſhall only mention one circumſtance of the kind at preſent. When ponds dry up in the ſummer, the ſediment left behind forms a paper-like ſubſtance on their bottoms: if a piece of this ſubſtance be put into water, it turns green in a few minutes, on account of the different confervæ, which enter into the compoſition of this natural paper, and revive upon their being ſaturated with their proper fluid.

The principle of accommodation that forms the ſubject of the preſent eſſay, is not confined to aquatic vegetables; for in the courſe of my examination of the ſubject, I found it in the vegetating germs of the feſtuca vivipara; ſeveral of which produced perfect plants after being kept dry ſome of them four, and others five weeks. The knowledge of this circumſtance, and the information I received that barley does not loſe the power of ſprouting by being malted, determined me to enquire more carefully into the nature of ſeeds in this reſpect, and I fixed upon peaſe for the experiment. With this view I permitted a number of them, which had been moiſtened for the purpoſe, to germinate for three days, and then dried them again, by the application of a gentle heat: on ſoaking them afreſh in water, and expoſing them to the air, the proceſs of germination recommenced, not by the production of new parts, for the former ſprouts revived, and advanced in growth as if no interruption had been previouſly offered to them. A repetition of the laſt experiment, made with the ſame peaſe, gave a ſimilar reſult; but a third attempt failed, becauſe the ſeed lobes parted, their mutual connection being nearly broken before the preſent trial, by the enlargement of the germs, and the treatment they had received.

The preceding experiments ſeem to ſuggeſt and authorize the following concluſions.

1ſt. The analogy which has been ſhewn in many caſes to connect vegetables with animals, becomes more extenſive the farther we carry our reſearches; of the truth of which the preſent eſſay affords an example.

2d. This analogy appears to be a neceſſary proviſion in the inſtance under conſideration, which enables a certain deſcription of organic beings belonging to both kingdoms, to accommodate themſelves to the viciſſitudes of a ſituation variable in its nature, and to preſerve not only the life of the individual, but perhaps the exiſtence of the ſpecies, by ſubmitting to a temporary death, or more properly a compleat ſuſpenſion of animation.

The ſeeds of land plants, which germinate on the ſurface of the ground, being expoſed to the inconveniences of the weather, in common with aquatic plants growing in ſhallow ponds, are provided with the ſame principle of accommodation to inſure their ſafety. The exiſtence of this ſalutary property is confirmed by experiment; but our knowledge of the nature of vegetables is as yet too limited to diſcover the ſingularity of conſtitution from which it reſults; we ſee the wiſdom of the contrivance, but cannot inveſtigate the cauſe.

4th. The peculiarity of conſtitution, which diſtinguiſhes viviparous plants appears to be this: germs of this deſcription begin to ſprout when their ſeed lobes have imbibed a quantity of humidity too ſmall to ſuffice in common caſes; for which reaſon they frequently vegetate on the ſtem of the parent plant.

This opinion is corrobated by the following facts:

1ſt. All ſeeds germinate when placed in the air, having their lobes ſaturated with water: on the contrary, they begin to rot when the quantity abſorbed by them is too little.

2d. The proportion of water required to produce ſaturation, in equal weights of different ſeeds, is different.

3d. Some plants, ſuch as the polygonium viviparum, are only viviparous in wet ſeaſons.

I am, SIR,
Your's, &c.
JOHN GOUGH.

VIII.

Extract of a Letter from Doctor G. M. to Dr. William Babington, dated Freiberg, Dec. 17, 1800. *On the State of Galvaniſm and other ſcientific Purſuits in Germany. Communicated by Dr.* BABINGTON.

MY DEAR DOCTOR,

I HAVE long been waiting for an opportunity of making you ſome return for the very valuable and intereſting intelligence in your laſt, and now ſeize the earlieſt occaſion of giving you ſome account of the galvanic labours in Germany; not but that I think it poſſible that you have already anticipated them in England; they are, however, of ſuch an important nature, that I think it adviſeable to give them to you on chance. The principal galvanic diſcoverer here is a young man, called Ritter, at Jena, in Saxony: about two years ſince he publiſhed the reſult of his almoſt innumerable experiments, in which he eſtabliſhed all its laws, and anticipated almoſt all the newer experiments. Unfortunately the book was written very obſcurely, and was ſtill more obſcured by the language of the newer philoſophy; and the author having neither enemies who were intereſted in bringing him into diſcredit, nor friends who were deſirous of drawing him out of obſcurity, his work remained little known and leſs noticed; he, therefore, gave up in a great meaſure the ſubject, and applied himſelf to other purſuits, till a few months ſince the general intereſt beſtowed upon the ſubject reanimated his zeal. Having ſatisfied himſelf that by paſſing the galvanic influence by means of gold wires through water, the oxygen gas alone was given off at the extremity of the wire connected with the zinc end, and the hydrogen gas from that communicating with the ſilver end of the pile, he reaſoned thus. If each particle of water be compoſed of a particle of hydrogen and another of oxygen, how can the correſponding gaſeous particles firſt make their appearance at the extremities of the reſpective wires, placed at one inch diſtance? can it be ſuppoſed that the particle of water is at one and the ſame time at the extremity of each wire?—this ſuggeſted to him to interpoſe ſome ſubſtance between the extremities of the wires, which was at the ſame time capable of conducting the galvanic influence, and of remaining perfectly unaltered by it. Such a ſubſtance preſented itſelf in highly concentrated ſulphuric acid: in a tube of the adjoining figure 6, plate XXI. he poured a portion of the acid, the upper parts he filled carefully with water, in which, by means of cork, he introduced two gold wires within a ſhort diſtance of the ſurface of the acid. On ſuffering the galvanic influence to paſs through it, the water in the leg *a* being connected with the zinc afforded oxygen gas alone, while that in the leg *b* communicating with the ſilver, gave hydrogen gas; and this evolution of gas, as in all other caſes, continues ſo long as the galvanic chain remains uninterrupted. To obviate the objections which might be made, that perhaps one or other of the gaſes made its way through the acid, he contrived the following experiment. He filled two tubes *a* and *b*, fig. 5, half with acid, and the remainder with water; in each two gold wires were introduced, in ſuch a manner that the extremities of the upper were ſurrounded with water, while the lower remained immerſed in acid, and were connected at *z*. The effect was preciſely the ſame: ſo long as the tubes ſtood in the chain, and the wire *a* was connected with the zinc, the water in that tube furniſhed oxygen gas alone, and the water in *b* hydrogen gas; but no ſooner was the manner of connection reverſed, than the reverſe took place. Laſtly, to remove the objection that thoſe two proceſſes are dependant on, and inſeparable from, one another, he placed a ſingle tube filled in the above manner in the chain. As long as the wire immerſed in water was connected with the zinc, oxygen gas alone preſented itſelf; but when it was turned towards the ſilver, hydrogen gas; thus it is proved, that water under certain circumſtances, may be wholly converted into oxygen gas, and under others, into hydrogen gas: the rationale of this phænomenon is as yet in obſcurity. One philoſopher accounts for it thus; that water + light gives oxygen gas, and water + heat hydrogen gas; when theſe two gaſes are decompoſed, the ponderable part preſents itſelf once more under the form of water, and the fire eſcapes. Others propoſe the following; that oxygen gas is water + poſitive electricity; and hydrogen gas, water + negative electricity. This ſeems countenanced by numerous obſervations, among others by ſome already made in England, viz. that the electrometer is affected negatively on being preſented to the zinc extremity of the galvanic battery, and poſitively at the ſilver end. I ſuppoſe I need not tell you that the atomiſtic ſyſtem is almoſt entirely exploded in Germany, and the dynamic ſubſtituted in its ſtead; conſequently, in place of the term *matter*, the phraſe *ſpacefilling* is employed. Thus, according to the neweſt ſect of natural philoſophy, water is the only ſpacefilling in nature that gravitates, and when united to electricity, furniſhes the principal gaſes as ſeen above; but when to water, magnetiſm is ſuperinduced, their coheſion is the conſequence, and the different ſolids are the products: the varieties in theſe reſult from the predominance of carbon or azote; the former being water + the north pole of the magnet, and the latter water + the ſouth pole. Water itſelf is perfect indifference, being neither electric nor magnetic, yet the fruitful parent of all!—This theory, certainly very ſimple, and poſſibly very ſublime, will probably appear as extraordinary to you, who are ſunk in the quagmire of matter, as it did to me. You are probably well acquainted with the progreſs the Pariſian chemiſts have made in the decompoſition of the alkalis and alkaline earths: their compound nature has ever been the opinion of Mr. Werner, from the incontrovertible facts which geognoſtic obſervations furniſh; there can alſo be no doubt but the ſilex, argil, &c. are equally compounded. I have lately repeated Guyton's experiments with the alkaline ſolutions of ſilex and argil: when theſe are well concentrated and mixed in equal proportion, a firm, gelatinous, opuleſcent, maſs reſults in a very few minutes; this is perfectly inſoluble in water, yet ſoluble in acids, whether concentrated or diluted, nay, even in diſtilled vinegar, and yet conſiſts of both ſilex and argil: here, therefore, the properties of the ſilex muſt be conſiderably altered. This muſt render all analyſis with alkalis ſuſpicious, and ſhows on what fallacious grounds the proud dominion of chemiſtry reſts, which ſhe has exerciſed ſo long in ſuch an arbitrary and overbearing manner in the mineral kingdom. * * * * *

IX.

On the Chemical Effects of the Pile of Volta. By a Correſpondent.

LETTER II.

To Mr. NICHOLSON.

SIR,

I HOPE I ſhall not treſpaſs too much on your time by inſiſting a little further on the difficulty, which I took the liberty of ſubmitting to you reſpecting the decompoſition of water by the Pile of Volta, and which you honoured by inſerting in your truly valuable Journal. I am not at all diſpoſed to believe that chemical effects, dependant on each other, ſhould always take place cloſe to each other; although I contend that we ſhould be able to aſcertain the cauſe which produces a deviation from local proximity.

That ſome diſtance of time intervenes between chemical effects, ſome experiments, in which I have been engaged for a conſiderable time, have fully convinced me. I have long thought, that as Nature produces ſome of her moſt curious and perfect combinations by very ſlow and regular proceſſes, that as ſhe deſtroys her fineſt productions when ſhe operates at high temperatures in volcanic countries, where every thing alike aſſumes the forms of cinders, aſhes and lava, that the uſual modes of operating chemically are defective in as far as we wiſh, by the application of great degrees of heat and ſudden ſtrong attractions by mixture, to produce immediate reſults in new combinations and new ſubſtances. I have ſucceeded in reverſing many acknowledged attractions, by only giving time for the proper arrangement of the particles. I ſhall mention one decompoſition which requires a very conſiderable time for its production. Why, if the attractions tend one way, they do not inſtantly arrange themſelves, I cannot at preſent determine. Having found that a ſolution of ſulphuret of pot-aſh attracts oxygen from the atmoſphere, I thought it might be employed for the decompoſition of the carbonic acid. With this view, I incloſed in a bottle a quantity of carbonate of lime, and a ſtrong ſolution of ſulphuret of pot-aſh. I ſhook them well together, but I did not obſerve any change after a conſiderable time. The bottle, however, remained in my room cloſely ſtopped for ſix months, when I obſerved that the ſides of it were covered as high as the ſolution reached with a black powder, which had the properties of charcoal. I could produce many other inſtances to prove that combinations will take place, as it were ſpontaneouſly, if time be allowed for proper arrangement. Organized ſubſtances in particular afford very ſtriking inſtances. Phoſphorus, though ſo ſtrongly attached to oxygen in animal ſubſtances, is nevertheleſs frequently ſeparated by a ſlow decompoſition.

In the experiment of the vegetations of a metal by means of zinc, which you produce as a parallel fact to the decompoſition of water from the pile, I find ſome modes of reaſoning which ſeem ſufficient to account for the phænomenon. The laws of cryſtallization will apply to explain why the lead aſſumes a regular figure, as other metals, for inſtance tin, aſſume their own forms. You may alſo have remarked that, when a metal is diſſolving in an acid, the ſolution deſcends through the ſolvent (as happens in the oxidation of copper by wires of that metal in water, and connected with the pile) according to the increaſed ſpecific gravity of the newly formed compound. It is alſo known, that there are different degrees of ſaturation between metals and acids, which may take place as the heavier fluid deſcends; for you will obſerve that the vegetations generally take place downwards. I have obſerved that, when zinc is placed in a ſaturated ſolution of lead or tin, a very confuſed maſs of precipitated metal forms very ſoon around it, and that, in order to produce fine cryſtallizations, it is neceſſary to add a large proportion of water: this ſeems to ſhew that the deſcent of the new metallic ſolution has ſome ſhare in producing the effect. But in theſe experiments continuity is preſerved, and the ſame effects are produced in every cryſtallization of any ſalts.

It appears to me therefore that the ſeparation of the two gaſes from the connection with the pile of Volta, is an anomalous effect in chemiſtry, as yet unexplained according to the doctrine of the decompoſition of water. It has been a long time known that poſitive and negative electricity have different properties; it is now clearly ſhewn that they produce different chemical effects. I do not ſee how, in ſtrict philoſophy, we are warranted in ſaying more of the experiment with the pile, than that one kind of electricity and water produce one kind of gas, and the other another gas. Is it not an aſſumption in this experiment to ſay, that the baſes of the gaſes are the component parts of water? I could ſtart other difficulties even where there appears to be a clearer proof of the decompoſition of water, but, fearful leſt I ſhould have already too much impoſed upon your kindneſs, I haſten to ſubſcribe myſelf,

Your obedient humble Servant,

AN EXPERIMENTALIST.

January 20, 1801.

X.

Analyſis of the Mellite, or in German, Honigſtein. By CIT. VAUQUELIN*.

THE analyſes which Meſſrs. Abich and Lampadius have given of this intereſting foſſil are well known. The firſt obtained from one hundred parts, 16 carbonate of alumine, 4 carbone, 3 oxyde of iron, 40 carbonic acid, 28 water of cryſtallization, having a ſmell of bitter almonds, and 5.5 of Naptha.

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* Annales de Chimie XXXVI. 203.

The ſecond had for his reſult 86.4 of charcoal, 3.5 petroleum, 2 ſilex, and 3 of water of cryſtallization. An enormous difference *.

Mr. Abich propoſes on account of the incombuſtibility of the honigſtein, that it ſhould be removed from the claſs of combuſtibles, and placed in that of the alumines.

But Mr. Klaproth, whoſe labours are intitled to the higheſt confidence, wrote me ſeveral months ago, that he has diſcovered that this pretended ſtone is compoſed of a peculiar vegetable acid united to alumine.

M. Abildgnaard, to whom I am indebted for many intereſting minerals of Norway, ſent me ſome decades ago, by favor of M. Mantey, a ſmall quantity of honigſtein, of which he had deſigned part for analyſis: and I haſtened to comply with his wiſh in that reſpect.

Deſcription of the Honigſtein.

This ſubſtance has a light yellow colour, which has cauſed it to obtain the name of mellite, or honey ſtone. It is uſually cryſtallized in an octahedron, of which the angles are ſometimes replaced by faces, owing to laws of diminution, of which the laws have not yet been limited.

Its ſpecific gravity is not conſiderable. It is according to M. Abich 1. *666*. It is found in Thuringia in ſtrata of foſſil bituminous wood.

I. *Chemical Character of Honigſtein.*

When expoſed to fire with the contact of air it becomes white, and burns without becoming ſenſibly charred; it leaves a white matter, which produces a ſlight efferveſcence with acids. It has no ſenſible taſte; though if it be kept for ſome time on the tongue, it occaſions a ſlight impreſſion of acidity.

II. ANALYSIS.

I took two grammes of the honigſtein reduced to powder, and mixed them with four grammes of ſaturated carbonate of pot-aſh diſſolved in a ſufficient quantity of water. As ſoon as the mixture was made, an efferveſcence of conſiderable activity followed, without the aſſiſtance of foreign heat; but to accelerate, and more effectually to complete the decompoſition of this ſubſtance, I ſlightly heated it on a ſand bath.

The filtered liquor after cooling was of a browniſh colour, and left on the filtering paper a brown matter, which when dried in the ſun weighed very nearly 0.8 of a gramme.

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* If M. Lampadius operated upon the ſame ſubſtance as Mr. Abich and myſelf have analyzed, it is impoſſible he could have had 86.4 of carbone; for in forty parts of carbonic acid, and four of carbone, obtained by Mr. Abich, there is not the matter to form eighty-ſix of carbone: and as from my analyſis it appears to me that there is not more than 55 per cent. of real acid in the honigſtein, it is evident that it cannot afford eighty-ſix of charcoal. It muſt therefore neceſſarily follow, either that Mr. Lampadius did operate upon another ſubſtance, or that he did not heat it ſufficiently to decompoſe the acid, if he did in fact operate upon the honigſtein.

This

III.

This 0.8 of a gramme of brown matter calcined in a crucible became white, and then weighed only 0.33 gramme. When mixed with diluted ſulphuric acid, it produced a ſlight effervefcence. The mixture was afterwards evaporated to dryneſs.

I expected from the information of Mr. Klaproth, that by the addition of water, nearly the whole of this matter would have been diſſolved; but the contrary happened, for the greateſt part remained in the form of a white powder.

The fluid being evaporated to the point, at which there remained at moſt not more than three or four grammes, I added one drop of ſulphate of pot-aſh, and obtained, by leaving it to ſpontaneous evaporation, about 0.1 gramme of alum, mixed with a ſmall quantity of ſulphate of lime.

I afterwards examined what might be the nature of the ſubſtance, which after the treatment with ſulphuric acid remained inſoluble in water.

For this purpoſe I boiled it in a ſolution of carbonate of pot-aſh; then filtered, waſhed, and examined it as follows:

1. The muriatic acid, diluted with two parts of water, attacked it with a ſlight effervefcence; but the ſolution did not become clear; on the contrary it remained ſomewhat milky.

2. This filtered liquor afforded with ammonia a precipitate reſembling in its tranſparency that which alumine affords by the ſame means; but it was not entirely ſoluble in pot-aſh.

But the portion diſſolved by the pot-aſh was the moſt abundant, and preſented all the characters of alumine: for when combined with the ſulphuric acid it afforded alum. The cauſe why this portion did not remain combined with the ſulphuric acid was therefore, moſt probably, that it had been too much heated towards the end of the deſiccation.

The fluid from which the ammonia had ſeparated the alumine laſt mentioned, afforded alſo ſlight precipitates by the carbonate of pot-aſh, and the oxalate of ammonia; which proves that it contained a ſmall portion of lime.

The portion of matter which was not diſſolved by the pot-aſh, weighed at moſt 0.1 gramme, and appeared to me to be ſilex.

The honigſtein therefore contains a ſmall portion of lime and of ſilex.

After having aſcertained the matter which compoſed my reſidue, I directed my operations to the liquid which muſt have contained the acid of mellite combined with pot-aſh; and with the expectation that it would yield its baſis to the mineral acids, I added to a portion of the liquid ſome drops of nitric acid, which produced a very ſlight effervefcence, and diſengaged a very ſmall portion of brown flocculent matter. Some hours afterwards, that which I ſuſpected came to paſs; the acid of the mellite cryſtallized in the form of ſmall ſhort priſms with brilliant faces.

Finding that this method would ſucceed for ſeparating the whole of this acid from the pot-aſh, I ſlightly heated the whole of the fluid, and added nitric acid till there was an exceſs perceptible to the taſte. I then filtered to ſeparate the brown flocculent matter, and obtain the acid in a ſtate of greater purity; and in effect I obtained about 1.34 grammes, which was white enough, though ſtill of a yellow tinge. The following are the properties which preſented themſelves on its mixture with different ſubſtances.

1. This acid has brilliant facets, conſiderable hardneſs, ſlight acid taſte, accompanied with bitterneſs, which may ariſe from ſome portions of bitumen ſtill attached to it, and to which it owes its yellow colour.

2. A portion of this acid expoſed to the flame of the blow pipe, gave at firſt a few ſcintillations like thoſe of ſalt-petre; after which it ſwelled up, and left a ſubſtance that ſoon penetrated the charcoal.

3. When heated in a covered crucible of platina it ſwelled up at firſt, afterwards became charred, without affording an oily flame, and left a light coal which is very alkaline [*]. This acid was therefore combined with a certain quantity of the pot-aſh, notwithſtanding the exceſs of nitric acid which had been added to its ſolution. The ſame effect alſo takes place with the tartareous and oxalic acids, which by ſimilar treatment are made to paſs to the ſtate of acidules.

4. This acid is ſparingly ſoluble; but I have not exactly determined the quantity of water which it requires.

5. Several grammes of the ſame acid, diſſolved in water, were mixed, 1. with a ſolution of lime: in which a white flocculent precipitate was immediately formed, that ſoon fell to the bottom of the fluid. 2. With a ſolution of ſulphate of lime, in which a light granulated and cryſtallized precipitate was found, that left the water ſtill in a certain degree tranſparent; but which was increaſed and rendered flocculent by the addition of one drop of ammonia [†]. 3. With the ſolution of muriate of barytes it affords little precipitate the firſt moment, but ſoon afterwards a ſhower of cryſtals in needles. 4. With the ſolution of ſilver it gives a white ſilky precipitate, which ſhines like a ſolution of ſoap: and ſometime afterwards it ſubſides in the form of powder. 5. With the nitric ſolution of lead it affords a white pulverulent very heavy precipitate. 6. With that of mercury a white precipitate, which a ſingle dop of ammonia turns black.

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[*] This acid muſt not be confounded with the acidulous tartrite of pot-aſh by this property. For the latter ſwells up much more, and emits at the time of its decompoſition a denſe fume, of a peculiar and very diſtinguiſhable odor.

[†] The acidulous tartrite of pot-aſh does not immediately produce a precipitate in the ſolution of ſulphate of lime; but twenty-four hours afterwards there is found in the mixture cryſtals with very brilliant facets, compoſed of lime and tartareous acid. Though cryſtallized, this tartrite of lime does not reſemble the combination formed with the acid of honigſtein, and the ſame baſe. It differs from it in ſwelling up in the fire, whereas that is decompoſed without ſwelling, in which reſpect it has analogy with the oxalate of lime.

From the reſult of theſe experiments we ſee, that the acid of the honigſtein has many properties analogous to thoſe of the acid of ſorrel, and by the compariſon I have made, I have perceived no difference but the following: 1. The precipitate which it cauſes in the ſolution of ſulphate of lime is leſs ſpeedily manifeſted, and is cryſtalline, inſtead of being pulverulent, like that which is formed by the acidulous oxalate of pot-aſh. 2. It appears leſs acid to the taſte than the acidulous oxalate of pot-aſh; but this may depend upon my not having added enough of nitric acid to the combination with pot-aſh, to deprive it of a ſufficient quantity of the alkali; and 3. It ſwells up rather more by heat than the acidulous oxalate of pot-aſh.

In other reſpects the ſublimed ſalt, the great quantity of carbonic acid, and the little coal which the mellite affords by diſtillation, are facts that ſeem to unite in proving the identity of the two acids: for the habitude of the ſalt of ſorrel by fire is the ſame *.

The octahedral figure of the honigſtein appears alſo to have analogy with that of the oxalic acid, which is a rectangular priſm terminated by pyramids with four faces. It is only neceſſary to examine the inclination of theſe faces in order to obtain a certainty.

Nevertheleſs, as I have had no more than about 1.34 gramme of this acid at my diſpoſal, I have not been able to ſubject it to all the trials which would have been required to demonſtrate in the moſt abſolute manner its identity in all reſpects with the oxalic acid. For though they have preſented ſimilar phænomena in all the comparative trials I have made, it is poſſible that by other that might yet be made, there might be found a ſingle difference which would deſtroy the ſimilitude.

It is therefore chiefly with a view to engage the chemiſts of Germany, where this ſubſtance is more common, to recommence the analyſis of honigſtein, and to compare its acid in every poſſible reſpect with thoſe of ſorrel that I publiſh this notice.

If my opinion ſhould be confirmed by new experiments, we ſhall then poſſeſs the oxalic acid in all the three kingdoms; namely, in the ſtate of acidulous oxalate of pot-aſh in ſeveral kinds of vegetables; in that of oxalate of lime in the human urinary calculi; and laſtly, in the ſtate of oxalate of alumine in the bowels of the earth among bituminous wood. But in whatever part it is found, it appears to owe its origin to vegetable ſubſtances.

Note, Since the above notice was drawn up, I have thought of mixing the acid of honigſtein combined with a ſmall quantity of pot-aſh, with a ſolution of pure ſulphate of alumine, and a very abundant flocculent precipitate was immediately afforded. On the other hand, I put into the ſolution of the ſame ſalt the acidulous oxalate of pot-aſh; but no precipitation was ſeen. Now theſe different effects muſt afford doubts ſufficiently ſtrong reſpecting the identity of the acid of honigſtein with that of ſorrel: and I muſt confeſs, that I ſhall ſuſpend my judgment in this matter, notwithſtanding the diſpoſition I had at firſt to believe, that theſe two acids are of the ſame nature. Before we decide, we muſt therefore wait for new proofs derived from a larger ſcale of experiments, in order to throw a more deciſive light on the ſubject.

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* The acid of ſorrel, or oxalic acid, is that which affords by diſtillation the largeſt proportion of carbonic acid and water, becauſe it contains more oxigen than any other known vegetable acid.

Cit. Hauy has carefully examined this ſubſtance. Some cryſtals of it were preſented to him by M. Abildgaard, and he procured ſome others from the collection made in Germany by Cit. Launoy. He has diſcovered, that the mellite has a very obvious double refraction, whence there ariſes a diſtinct character between this mineral and amber, of which the refraction is ſimple. He has obſerved likewiſe, that the cryſtals of mellite being inſulated, acquire with much facility the reſinous electricity, but he was not able when they were not inſulated, to excite in ſome of them more than a weak and varying electricity; ſo that it was neceſſary to bring quickly together the cryſtal and ſmall copper needle that is uſed in theſe kind of experiments, in order that the latter may be more ſenſibly attracted. What ſeveral learned men have advanced, that this mellite is not rendered electrical by friction, is not exactly true: it may then acquire an electricity of the ſame nature, as that of amber, but it would be conſiderably weaker if the cryſtal be not inſulated.

According to the obſervation of the ſame naturaliſt, the primitive form of the mellite determined according to the poſition of the natural junctures, is that of a rectangular octahedron, in which the incidence of the face of one pyramid with the face of the other, is about 93½°. This octahedron is ſometimes truncated by means of a decreaſe, by an arrangement on all its ſolid angles, in which caſe the two facets which are placed on its ſummits are often curve lined. When that the diminution only acts on the four lateral angles a dodecahedron is produced, which conſiderably reſembles the rhomboidal figure. Suppoſing this reſemblance perfect, this would be a fourth origin of that dodecahedron, which exiſts as primitive in granite and ſulphurated zinc, and has in other ſubſtances, where it becomes a ſecondary form, ſometimes as nucleus a cube, and at others a regular octahedron.

Bulletin des Sciences, *No.* 43. *An.* 9.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Method of aſcertaining the Inclination of the Magnetic Needle. By Cit. Coulomb.

THOUGH the inſtrument which is uſually employed to meaſure the inclination of the magnetic needle is very ſimple in its conſtruction, it is nevertheleſs liable to great errors, which in general ariſe from the almoſt abſolute impoſſibility of placing the needle in all the poſitions it can take in equilibrium with regard to the effect of gravitation, that is to ſay, ſo that its centre of gravity may always exactly agree with the point on which it turns. When the dimenſions are conſiderable, a new inconvenience ariſes from a degree of flexure which, though ſcarcely ſenſible, is nevertheleſs productive of very great effects from the ſlighteſt diſplacement of the centre of gravity producing a combination of the power of gravitation with that of magnetiſm.

To obviate theſe difficulties, Cit. Coulomb, inſtead of endeavouring to aſcertain immediately, as has been hitherto done, the direction of the magnetic needle in the vertical plane which paſſes through the magnetic pole, conceives the force of this pole to be decompoſed or reſolved into two others in the ſame plane, the one acting in a horizontal, and the other in a vertical direction. He determines ſeparately the intenſity of each of theſe laſt forces, and the reſult gives the direction in which the magnetic force acts, and which a needle governed ſingly by this force would take.

Cit. Coulomb has proved, in the Memoirs of the Academy of Sciences for the year 1789, that the magnetic needle ſuſpended by its centre of gravity is inceſſantly brought back to its true direction by a conſtant force at the ſame place and time. It thence follows that by obſerving the number of oſcillations made in a given time by a needle horizontally ſuſpended, the ratio of the horizontal component part of the magnetic power with gravity may be obtained. As to the vertical component part, it is meaſured by determining with care the weight neceſſary to be added to the ſouthern part of the magnetic needle, to maintain it in a perfectly horizontal poſition. That being done, if A and B repreſent the reſpective meaſures of the horizontal and vertical component parts of the magnetic power $\frac{B}{A}$ will be the tangent of the angle made by their reſult with the horizontal force, and, conſequently, it will be the inclination of the magnetic needle.

In the experiments made by Cit. Coulomb the needle had the form of a right angled parallelipipedon, very thin in proportion to its breadth, and always ſuſpended, ſo that its breadth was kept in a vertical plane. Let P repreſent the weight of the needle, l the half of its length, λ the length of a pendulum that performs its oſcillations in the ſame time as the needle when it obeys the magnetic power in an horizontal plane. Cit. Coulomb then gives the formulæ $\frac{P\,l^2}{3\,\lambda}$ to calculate the momentum of the magnetic force referred to the arm of a lever of one millimeter in length. The length of the needle was 427 millimetres, its breadth 13, and its weight 88753 milligrammes. It was ſuſpended horizontally by a thread of ſilk in a box well cloſed, and it made 30 oſcillations in 286 ſeconds, and by applying theſe data to the preceding formula, Cit. Coulomb finds that the logarithm of the momentum of the horizontal magnetic force is 4,1740.

Cit. Coulomb having placed his needle in a clip, having knife edges, which reſted on two cylinders of glaſs, in the manner of the beam of a balance, endeavoured firſt to bring it to an equilibrium in an horizontal ſituation coinciding with the magnetic meridian, by placing the edges in a proper manner, and when they were ſufficiently near the point where the equilibrium took place, he completed it by the addition of ſmall weights. He then reverſed the poles of the needle by the magnetic touch, but without altering the poſition of the clip, and again bringing it to an equilibrium in this new ſtate, the ſum of the momenta of the additional weights placed in theſe two operations gave him the double of the momentum of the vertical component parts of the magnetic force, valued at $\frac{74467}{2}$. The reſult of this force, and of the horizontal force, is inclined 68° 9′.

In repeating theſe operations three times, Cit. Coulomb obtained ſucceſſively 68° 9′; 68° 13′; and 68° 11′. Though the differences of theſe reſults are very trifling, he does think they are to be entirely attributed to errors in the obſervation; for he is aſſured that they do not amount to ſo much. It is poſſible that the needle is ſubject to variations in the vertical ſimilar to thoſe which are known to take place in the horizontal plane.

Bulletin de la Soc. Phil. No. 31. *An.* 8.

New Production of Ammonia.

Mr. Lampadius, at Freyberg, has obſerved that when crude, or which is better, purified acidulous tartrite of pot-aſh, is heated till no more fumes or flame appear, and water is then added to it, ammoniac is produced. This fact is beſt obſerved when heated maſs is ſtill warm (about 120 R.)

The experiment may be performed with the ſame tartrite repeatedly, or at leaſt as long as any carbonaceous matter remain, by merely heating it, and then wetting it with a few drops of water. Carbon mechanically joined to pot-aſh does not produce this effect; but the acidulous oxalate of pot-aſh, treated in the ſame manner, affords a ſimilar reſult.

Crell's Annals of Chemiſtry, No. 8, 1800.

Upon trial of the above before ſome philoſophical gentleman at my houſe, the fumes which aroſe upon the addition of the water did not indeed ſmell of ammonia, but they formed a white cloud round the ſtopper of a bottle which was wetted with ſtrong muriatic acid. The indication was not ſo manifeſt as to remove all doubts in the minds of the aſſiſtants; but Mr. Accum has ſince informed me that he has found it much ſtronger and more conſtant when paper, wetted with muriatic acid, was uſed.

Diſcovery of a new Alkali.

Dr. Hahneman *, at Altona, in Germany, has diſcovered a new ſpecies of alkali, which he diſtinguiſhes from the others by the name of *pneum-alkali*, on account of the ſingular property it poſſeſſes, of increaſing in bulk to twenty times its ſize when heated. This alkali differs greatly from any of the others, by the following properties.

Its cryſtals are hexaedral priſms, terminated by two inclined faces, one of which appears to be trihedral, the other pentahedral:

It is not volatile by heat.

It neither attracts moiſture, nor effloreſces by expoſure to the air.

Two parts of diſtilled water diſſolve one of this alkali at † 300° F. Five hundred parts of water diſſolve one hundred and forty parts at 60° F. At a lower temperature water acts upon it very ſlightly: it is, therefore, ſeparated from its former ſolution by mere cold: it may thus likewiſe be ſeparated when combined with any of the other alkalies.

It effervesces but ſlightly with concentrated acids; but it is not yet determined if the diſengaged aeriform fluid is carbonic acid, or any other gaſeous fluid.

The ſulphate of pneum-alkali is totally inſoluble in ardent ſpirit. Water diſſolves only a ſmall quantity.

The nitric, muriatic, phoſphoric, and acetous acid, readily combine with it, and the neutral ſalts reſulting from theſe combinations are totally ſoluble in water and ardent ſpirit. The phoſphate and acetite of pneum-alkali are in conſiderable quantity even ſoluble by cold in ſpirit of wine.

The phoſphate of pneum-alkali has a bitter taſte.

The muriate cryſtalliſes in plumoſe cryſtals.

All the preceding combinations of this alkali with acids (the phoſphate excepted) are decompoſable by mere heat alone. They all part with their acids, and leave the pneum-alkali behind in a pure ſtate. The inventor of this alkali employed for this purpoſe a red heat. If a leſs heat will do, I do not know.

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* Scherer's Chem. Journal, III.

† Quere!

The nitrate of pneum-alkali liquifies in its own water of cryſtallization at 300° Fr. It firſt bubbles much, nitrogen gas is evolved, and the ſalt is long decompoſed before the maſs is ignited. The nitrate of pneum-alkali does not detonate on ignited coal, it does not decrepitate, nor does it become phoſphoreſcent, nor does it emit ſparks. Its decompoſition by heat is affected gradually and quietly.

The union of pneum-alkali with carbonic acid is extremely difficult. It is beſt effected by decompoſing a ſaline combination of this alkali with an exceſs of acid, by means of any of the other carbonated alkalies: the firſt precipitate which is obtained by this means, is the *pure pneum-alkali* in a cryſtalline form; but ſoon afterwards the carbonate appears in the ſtate of a light, white, pulverulent, earthy ſubſtance. The carbonate of pneum-alkali readily parts with its carbonic acid at the common temperature of our climate, and becomes converted into pure pneum-alkali.

It ſeparates earths and metallic ſubſtances from their ſaline combinations.

Mercury, when agitated with the aqueous ſolution of this alkali, does not loſe its luſtre.

It precipitates the oxigenated muriate of quickſilver of a carmine red, the nitrate of mercury black, the nitrate of ſilver white, &c. &c.

It decompoſes muriate of ammoniac at 100° F; but water, impregnated with ammoniac gas, ſeparates the pneum-alkali from the neutral combinations by mere cold.

A ſaturated ſolution of this alkali unites with fat oils by mere agitation, and forms a ſaponaceous compound, ſoluble in ſpirit and in water. Ardent ſpirit has no effect upon it. It changes blue vegetable tinctures green, and poſſeſſes ſome of the properties common to the other alkalies, but again many others, widely different, which entitle it to the notice of the philoſophical chemiſt.

This alkali is ſold at C. G. Hilſcher in Leipzic, in bottles containing one ounce, at one Frederic d'or.

Flour from the Bread Fruit.

Citizen Van Noorden, phyſician at Rotterdam, has written to the (French) Society of Arts, that a ſurgeon, who lately arrived from Surinam, informs him, that the bread fruit tree has ſucceeded ſo well in that colony, that there are conſiderable plantations, which are productive beyond all expectation. They make bread from it, which is as good as that afforded by wheat. For this purpoſe the fruit is ſliced, then dried in the ſun, and afterwards pounded; and the flour, when made into paſte with water, riſes as well as the flour of wheat, and may be kept for a conſiderable time. The expectation which has juſtly been entertained of introducing this valuable tree into the weſtern colonies, and even into Europe, will give additional intereſt to this account of the new application of the bread fruit at Surinam.

Bulletin de la Societé Philom. No. 44, *An.* 9.

Extract from a Memoir of Cit. Thénard, on the ſeveral Degrees of Oxigenation of the Oxide of Antimony, and on its Combinations with ſulphurated Hydrogen.

Cit. Thénard divides his memoir into four paragraphs.

In the firſt he relates the principal experiments that have been made ſince Geoffroy to the preſent time.

In the ſecond he treats of the ſeveral oxides of antimony, and ſhews that this metal is capable of being combined at leaſt in ſix different proportions with oxygen; that when oxided to the minimum, it is firſt black, then cheſnut brown, orange, yellow, and white; and at the maximum alſo white;—that diaphoretic antimony is a combination of this laſt with pot-aſh, and not a pure oxide, as has hitherto been imagined: that the ſecond, white oxide which is leaſt oxided, comprehends the ſublimed oxide of antimony, that which enters with the compoſition of the emetic tartar, and alſo of the butter of antimony, which, conſequently, ought to be eraſed from the liſt of oxigenated muriates, where it is placed; that all theſe oxides, when heated in a well cloſed crucible, are reduced, and the more eaſily the leſs they are oxided; and thus produce the oxides of a yellow, orange, and cheſnut brown, and the black oxide, which is alſo obtained with more facility by precipitating the ſolutions of antimony by iron, and poſſeſſes the remarkable property of being pyrophoric.

In the third part, the author gives the analyſis of kermes and the golden ſulphur, and ſhews that the alterations to which theſe bodies are ſubject from the action of air and light are owing to the decompoſition of theſe fluids: that in the kermes the oxide is in the ſtate of the brown oxide, and in the golden ſulphur in the ſtate of the orange oxide; that the cauſe of different colours of the kermes, which are obtained, ariſes from the different coloured oxides which theſe kermes reſpectively contain. He afterwards gives analyſes of the ſulphuric acid, the ſulphate of barytes, and of ſulphurated hydrogen, together with the ſpecific gravity of this laſt, and then proceeds to ſhew the action of the alkaline baſes on the ſulphuret of antimony, and ſhews that the kermes is held in ſolution by the ſulphurated hydro-ſulphuret of the baſe, formed by the decompoſition of water; that accordingly as this ſulphurated hydro-ſulphuret has, or has not, the property of being more ſoluble with heat than without, the depoſition by cooling is found to take place on the contrary; and, laſtly, that the liquor when cold throws down, by means of the acids, the golden tartar, and not kermes, becauſe the brown oxide becomes more oxided by the oxygen of the water and paſſes to the ſtate of the orange coloured oxide.

In the third paragraph Cit. Thenard makes a recapitulation of his experiments.

Bulletin des Sciences, An. 8. *No.* 31.

On the Structure of the upper Pyreneans, by CIT. RAYMOND. *Communicated to the National Inſtitute of France.*

Pallas in Aſia, Sauſſure, Deluc, and Dolomieu in Europe, have obſerved, that in large chains of mountains, there is uſually in the center a more elevated chain of granite, accompanied on each ſide by a collateral ſchiſtus chain, and ſtill lower by another, which is calcareous.

The Pyreneans appear to accord with this law. Their higheſt points are certainly calcareous, and this circumſtance has embarraſſed obſervers.

Cit. Raymond has aſcertained, that the reſpective diſpoſition of the five orders of mountains, exiſts no leſs in theſe than in the other chains, but with this difference, that the calcareous chain on the Spaniſh ſide is the moſt elevated of the five, and that in returning on the ſide towards France the ſouthern ſchiſtus chain is found, then the granite, or middle, and the northern ſchiſtus and calcareous chains, gradually diminiſhing in height, which is the cauſe why the geological axis of the Pyrenees, or the granite, is not the ſame as the geographical axis, or that which determines the courſe of the waters.

In order to demonſtrate the accuracy of his obſervations he has drawn on the chart five lines, correſponding with the five orders of the mountains, each of which is found to paſs through the ſummits or maſſes of the ſubſtance, which forms the character of that order which the line indicates.

The granitic axis paſſes through the ſummits of Néouvielle, Pic long, Bergons, and Monné, the ſchiſtoſe chain, and the northern Gneiſs, through the Pic du Midi, and the ſouthern through thoſe of Troumouſe, Pic mené, Vignemâle, and the Pic du Midi de Pau.

The calcareous ridges on the ſide of France are thoſe of Campan and of Sarrancolin, ſo much celebrated for the marble they produce, and thoſe on the ſide of Spain form Mont Perdu, Marboré, and le Pic blanc, which are among the moſt elevated ſummits of theſe mountains.

Abridged in the Bulletin de la Soc. Philom. No. 41, *An.* 8.

Letter

Letter from Mr. DAVY, *Superintendant of the Pneumatic Inſtitution, containing Notices concerning Galvaniſm.*

To MR. NICHOLSON.

SIR,

In purſuing my enquiries concerning the production of galvanic influence during metallic oxidation, I have found that many of the difficultly oxidable metals may be made to act as Voltean combinations, by being connected in pairs in the common order with fluids capable of oxydating one of the alternate metals.

Ten ſilver plates attached to thin gold wires, and arranged in glaſſes containing diluted nitric acid, produced when their agency was applied in the uſual mode, a ſtrong cauſtic ſenſation on the tongue, and effected, though feebly, the uſual changes in water.

Twenty pieces of copper in contact with ſilver wires, when connected with weak ſolutions of nitrate of mercury acted powerfully, and that for a great length of time, *i. e.* till almoſt all the mercury was precipitated on the copper. The influence produced ſenſible ſhocks. When it was paſſed through water by means of gold wires, oxygen was given out at the place of the copper, and hydrogen at the place of the ſilver. Whereas in the combination with ſilver and gold, the oxygen was produced at the place of the ſilver, and the hydrogen at that of the gold.

The agency of galvaniſm upon inorganic bodies appears to be ſimilar under all its different modes of excitement. I have found that the gaſes evolved from water by the action of ſeries in which the oxidating fluid media are acids, or metallic ſolutions, do not differ in kind or properties from thoſe produced by that of combinations in which the fluid media are conſtituted by common water.

I have lately made many experiments on the ſingle oxidating circles of Aſh, and on the influence of thoſe circles on galvanic animal irritation. Theſe experiments will at ſome time be made public; they go far towards proving not only that the circles of Aſh are governed by the ſame laws as the pile of Volta; but likewiſe that there exiſts in living matter galvanic action independent of all influence generated by metallic oxidation. I have produced the phænomena of taſte and muſcular irritation by means of metals, in caſes when they were apparently incapable of undergoing chemical change.

I am, SIR,

with reſpect your's,

HUMPHRY DAVY.

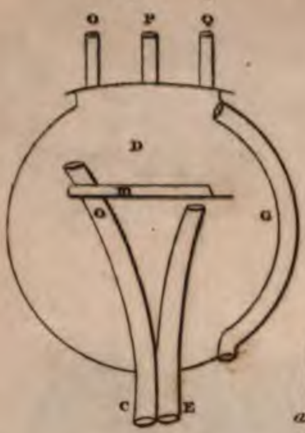
Dowry Square, Hotwells,
Jan. 23, 1801.

A new edition of Parkinſon's Chemical Pocket Book, with improvements, is on the point of being publiſhed.

Mr. William Henry, of Mancheſter, has in the preſs, and in conſiderable forwardneſs, a ſmall work, intended, partly, to facilitate the acquirement of chemical knowledge to perſons entering on the ſtudy, without the aid of an inſtructor; and, partly, as a portable companion, for the uſe of more advanced ſtudents. The firſt part will contain directions reſpecting the beſt mode of ſtudying chemiſtry; and, alſo, an arranged ſeries of experiments, neceſſary to be performed, by thoſe who intend to become acquainted, by actual obſervation, with the chemical properties and habitudes of bodies. More minute directions will be given for conducting theſe experiments with ſucceſs, than are to be found in other elementary books. The ſecond part will comprize ſummary inſtructions for the analyſis of mineral waters, and of mineral bodies in general: and the third part will point out ſome of the uſeful applications of chemical agents, in detecting adulterations, in diſcovering poiſons, &c. The work will form one ſmall pocket volume; and the author thinks proper to obſerve, that it will not at all interfere with the excellent little manual, lately publiſhed by Mr. Parkinſon, the plan and objects of which are perfectly different.

⁂ J. W. L. requeſts the Correſpondents of this Journal to point out *a method of obtaining aluminous earth perfectly free from any heterogeneous mixture*, and obſerves, that the methods pointed out by ſyſtematical writers are inſufficient for that purpoſe. As this difficulty will no doubt be new to many chemiſts, it would be a deſirable communication if he were to ſtate the imperfections which his practice may have led him to detect.

Fig. 1.



Galvanism

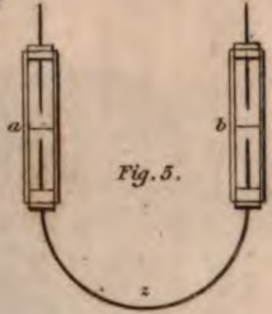


Fig. 5.

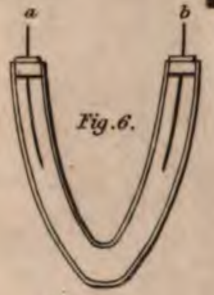
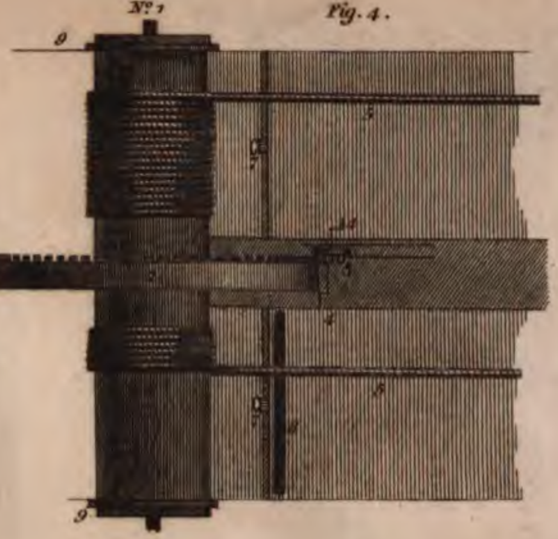


Fig. 6.

Fig. 4.



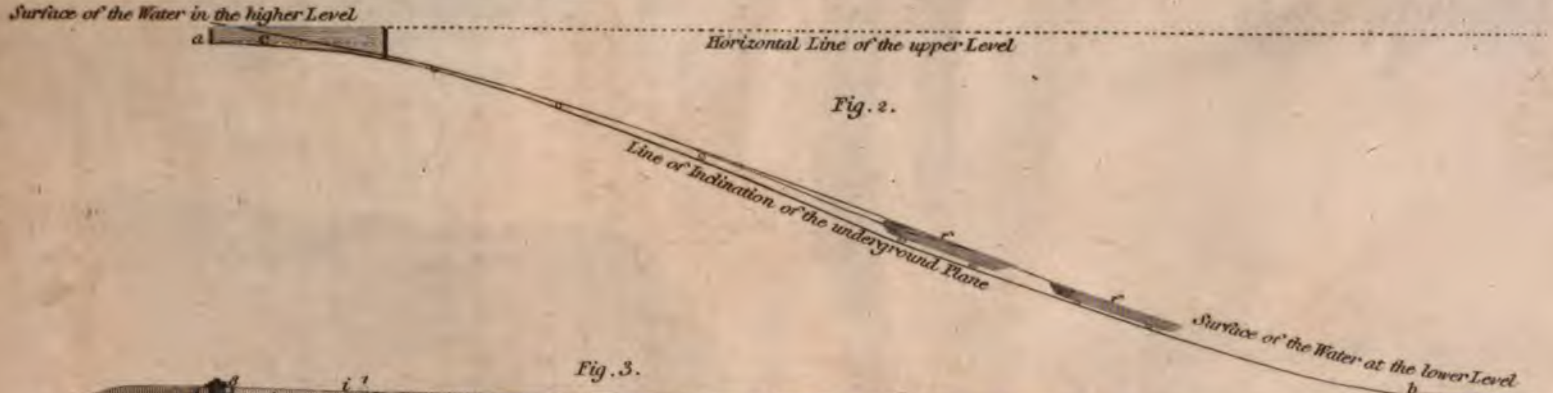
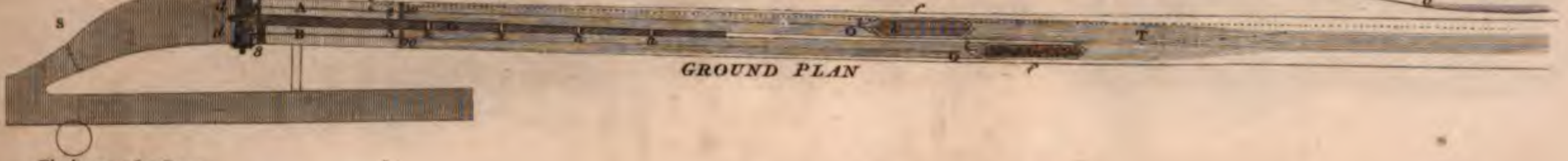

Surface of the Water in the higher Level
a
C
Horizontal Line of the upper Level
Line of Inclination of the underground Plane
Surface of the Water at the lower Level
b

Fig. 2.

Fig. 3.



GROUND PLAN

Mutlow Sculp.

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ASTOR,
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Philos. Journal Vol. IV. Pl. XXII. facing p. 3

Fig. 1.

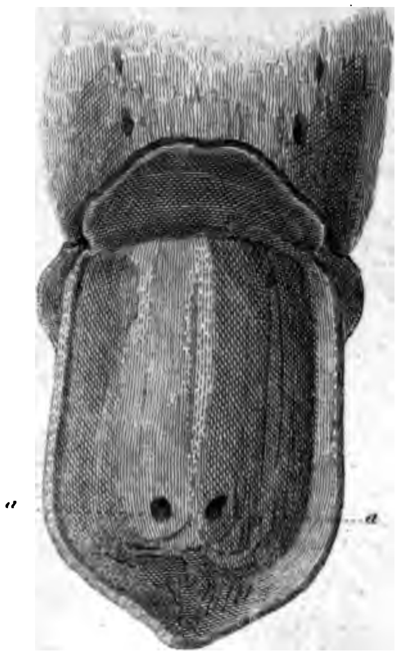


Fig. 2.

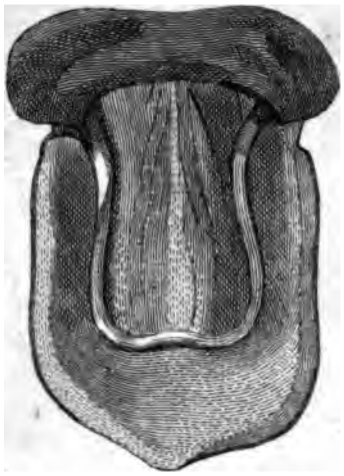
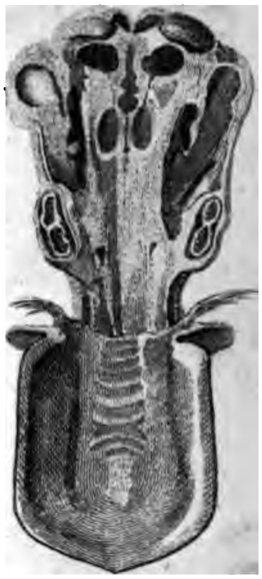


Fig. 4.



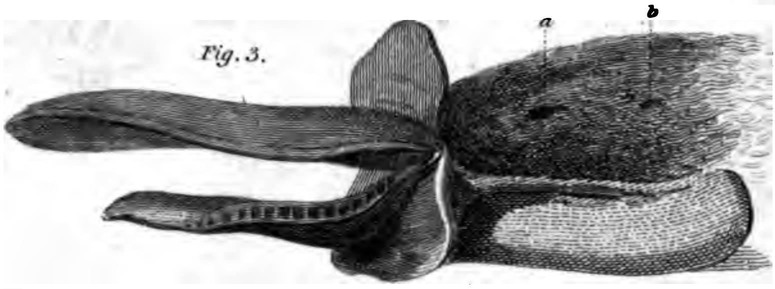


Fig. 3.

Fig. 6.

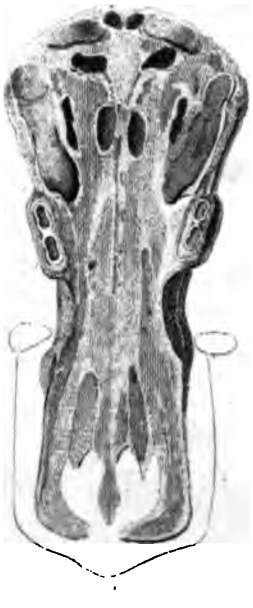


Fig. 5.

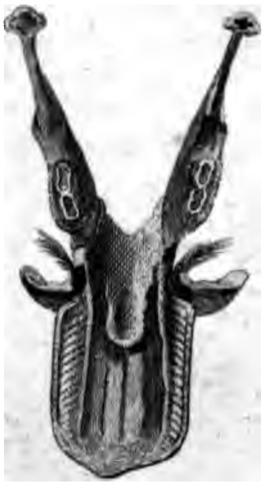
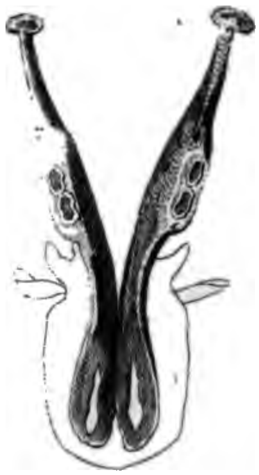


Fig. 7.



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ASTOR, LENOX AND
TILDEN FOUNDATIONS

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH 1801.

ARTICLE I.

Experiments to determine whether or not Fluids be Conductors of Caloric. By THOMAS THOMSON, *M. D. Lecturer on Chemiſtry in Edinburgh. Communicated by the Author.*

IT is rather ſurpriſing that the experiments publiſhed by Count Rumford, in order to prove that fluids are non-conductors of caloric, ſhould have been in the poſſeſſion of men of ſcience for more than two years, without any attempt either to confirm or refute them. Theſe experiments were conducted with ſuch admirable ſimplicity and ingenuity, by a man of ſuch acknowledged talents and candour, and of ſuch well-merited celebrity, that they might have been expected to excite peculiar attention. The ſubject, too, is of the higheſt importance; not only becauſe it is calculated to throw additional light on the nature of caloric, but becauſe it is intimately connected with ſome of the moſt important operations in nature. It is impoſſible for us to inveſtigate ſpontaneous evaporation, rain, winds, the changes of temperature in the atmoſphere, and ſome of the moſt important phenomena of chemiſtry, at leaſt with any chance of ſucceſs, till we have ſettled the previous queſtion, whether fluids be conductors of caloric or not. Perhaps it will be ſaid, that this point has been completely eſtabliſhed by Count Rumford. This is the very thing which I propoſe to examine. But it will be proper, in the firſt place, in order to prevent verbal diſputes, to explain what I mean by the *conducting power* of bodies. Count Rumford has neglected to do this; and I am not abſolutely certain that he affixes the ſame meaning to the phraſe that I do.

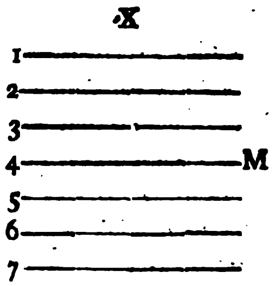
Dr. Herſchel appears to me to have demonſtrated, that caloric is emitted from the ſun in rays, and that it moves with the ſame velocity as light, or at the rate of nearly 200,000 miles in a ſecond. It follows, from his experiments, and from thoſe of Scheele and Pictet, that caloric moves through air without any ſenſible diminution of its velocity. It has been demonſtrated, too, that it is capable of moving through glaſs of a certain temperature, without any ſenſible diminution of its celerity. Now, whenever caloric paſſes through a body with undiminiſhed celerity, we may ſay that it is *tranſmitted* through the body. *Air*, then, and *glaſs*, have the property of *tranſmitting* caloric through them. When caloric is tranſmitted through a body, as its velocity is too great to be meaſured, it appears to paſs through the body inſtantaneouſly. Thus M. Pictet found that caloric moved inſtantaneouſly through 69 feet of air.

When the end of an iron rod, 20 inches long, is put into the fire, while a thermometer is applied to the other extremity, it is four minutes before the thermometer begins to riſe, and it takes 15 minutes before it riſes 10°. Caloric, then, does not paſs through a rod of iron inſtantaneouſly; conſequently, it is not *tranſmitted* through the iron, but moves through it in a different manner. Its velocity is prodigiouſly diminiſhed, ſince inſtead of moving at the rate of 200,000 miles in a ſecond, it moves only at the rate of 20 inches in 4′. Now, whenever caloric paſſes in this manner through bodies, with its velocity prodigiouſly diminiſhed, it is ſaid to to be *conducted* through them. It appears, then, that caloric is *tranſmitted* through bodies, and *conducted* through them, in quite a different manner.

If we take an iron rod, and another of baked clay, of preciſely the ſame ſize and ſhape, and, putting one end of each into the fire, apply thermometers to the other extremity, we ſhall find that the thermometer applied to the iron rod riſes ſooner than that applied to the clay rod. Caloric is then conducted with more rapidity through iron than through baked clay. Conſequently, iron is a *better conductor* of caloric than baked clay. When a body does not allow caloric to paſs through it at all, it is called a *non-conductor*.

It is not difficult to form ſome notion of the manner in which caloric is conducted through bodies. As its motion is prodigiouſly retarded, it is clear that it does not move without reſtraint. It muſt be detained for ſome time by the particles of the conducting body, and, conſequently, muſt be attracted by them: hence it follows, that there is an *attraction* or *affinity* between caloric and every conductor of caloric. Now it is in conſequence of this affinity that the caloric is conducted through the body.

Let



Let M be a body (a maſs of iron for inſtance) compoſed of an indefinite number of particles, arranged in the ſtrata 1, 2, 3, 4, 5, 6, 7, &c. Let caloric be communicated to it in the direction X. The firſt ſtratum of particles 1 combines with a certain doſe of this caloric, and forms a compound, which we ſhall call A. This compound cannot be decompoſed by the ſecond ſtratum, becauſe all the ſtrata, before the application of the heat, were at the ſame temperature; conſequently, the affinity of all for caloric muſt have been equal. Now it would be abſurd to ſuppoſe a compound deſtroyed by an affinity not greater than that which produced it. If, therefore, only one doſe of caloric combined with ſtratum 1, no caloric could paſs beyond that ſtratum. But the compound A has ſtill an affinity for caloric; it therefore combines with another doſe of it, and forms a new compound, which we ſhall call B. Now it is a general law, to which I know not a ſingle exception, that when a body combines with different doſes of another body ſucceſſively, the firſt doſe is retained by a ſtronger affinity than the ſecond, the ſecond by a ſtronger than the third, and ſo on. Thus iron, by combining with 25 per cent. of oxygen, forms the green oxide of iron: by combining with 48 per cent. it forms the brown oxide. Here, then, are two doſes of oxygen which combine ſucceſſively with the iron; the firſt is 28 per cent. the ſecond 23 per cent. Now the ſecond doſe is not retained by an affinity nearly ſo great as the firſt doſe. For many ſubſtances are capable of abſtracting the ſecond doſe, and, conſequently, of converting brown oxide of iron into green oxide which have no action on the green oxide: but all the bodies capable of decompoſing the green oxide are capable alſo of decompoſing the brown oxide.

Let us apply this general law to the compound B, into which ſtratum 1 has entered. This ſtratum is now combined with two doſes of caloric, the firſt of which is retained by a ſtronger affinity than the ſecond. Stratum 2, therefore, though incapable of decompoſing the compound A, has a ſtronger affinity for caloric than compound A has for the ſecond doſe of caloric; it therefore ſeizes upon this ſecond doſe, combines with it, and forms the compound A. Here, now, are two ſtrata of particles, combined each with a doſe of caloric, and, conſequently, conſtituting the compound A. The third ſtratum is unable to decompoſe the ſecond, for the ſame reaſon that the ſecond was unable to decompoſe the firſt. Stratum 1 again combines with a doſe of caloric, and forms compound B: ſtratum 2 is unable to decompoſe this componnd, becauſe being already compound A, its affinity for caloric cannot be greater than that of compound A. Caloric, then, cannot paſs farther through the body; but ſtratum 1 combines with a new doſe of caloric, and forms a compound, which we ſhall call C. The affinity of this third doſe being inferior to that of the ſecond, ſtratum 2 decompoſes compound C of ſtratum 1, and forms itſelf compound B. This compound is decompoſed by ſtratum 3, which now forms compound A. Stratum 1 again forms compound C, which is again decompoſed by ſtratum 2, which ſtratum forms a new compound B. Compound C is a third time formed by ſtratum 1. Three ſtrata are now heated. Stratum 1 is combined with three doſes, ſtratum 2 with two doſes, ſtratum 3 with one doſe. The heat can paſs no farther, for ſtratum 4 cannot decompoſe compound A, nor compound A compound B, nor compound B compound C; but ſtratum 1 combines with a fourth doſe of caloric, and forms a new compound, which we ſhall call D. This new compound is decompoſed by ſtratum 2, which forms compound C. Stratum 2 is decompoſed by ſtratum 3, which forms compound B, and ſtratum 3 by ſtratum 4, which forms compound A. Stratum 1 again forms compound D, which is again decompoſed by ſtratum 2. This, in its turn, is decompoſed by ſtratum 3, which forms compound B. Stratum 1 again forms compound D, which is again decompoſed by ſtratum 2, in order to form compound C. Compound D is again formed by ſtratum 1, and is not now decompoſed. Here are four ſtrata combined with caloric. Stratum 1 with four doſes, ſtratum 2 with three doſes, ſtratum 3 with two doſes, and ſtratum 4 with one doſe. In this manner may the heating progreſs go on till any number of ſtrata whatever are combined with caloric.

Thus we ſee that caloric is conducted through bodies merely by repeated chemical combinations and decompoſitions. Hence the reaſon of the ſlowneſs of its progreſs. We ſee, too, that the temperature of the ſtrata muſt diminiſh in a kind of arithmetical ratio according to their diſtance from the ſource of caloric; and this, in fact, holds in every inſtance. When one end of an iron rod is held in the fire, the temperature gradually diminiſhes as we approach the other extremity. We ſee, too, that the diſtance to which a body can conduct caloric muſt in all caſes depend upon the degree of heat to which the firſt ſtratum of its particles can be ſubjected, before it changes its ſtate. If it can bear a very great heat, the body will conduct to a very conſiderable diſtance; whereas, if if it can bear but little heat, the body will conduct but a very ſhort way. Thus brick will conduct much farther than wood, becauſe when the temperature is raiſed above a certain degree, the wood catches fire, and is diſſipated; whereas, the brick undergoes no change. The diſtance, then, to which a body conducts caloric, is not a criterion of the goodneſs of its conducting power, ſo much as of the degree of heat to which it can be ſubjected without a change of ſtate, unleſs the heat be far inferior to what is capable of producing ſuch a change. It appears, too, from this account, that the conducting power of a body is, in all caſes, inverſely as its affinity for caloric. Conſequently, the beſt conductors are thoſe bodies which have the weakeſt affinity for caloric.

It can ſcarcely be doubted that caloric is conducted through all *ſolid* bodies in the manner above deſcribed. All ſolid bodies hitherto examined are conductors of caloric. Fluids, too, were univerſally conſidered as conductors, till Count Rumford drew the contrary concluſion from his very ingenious experiments. If his concluſions be juſt, this conſtitutes a very curious and important diſtinction between ſolid and fluid bodies. But the more important the concluſion is, the more rigidly ought we to examine the premiſes before we adopt it.

Count Rumford has proved, by a ſet of very ingenious and happily-contrived experiments, that when heat is applied to a fluid, the fluid is heated in conſequence of every individual particle going in its turn to the ſource of heat, combining with caloric, and then giving place to other particles, which combine with caloric in their turn. It is evident, then, that ſolid and fluid bodies are not heated in the ſame manner. The firſt ſet of bodies can only be heated by their conducting power, in the ſecond ſet every individual particle goes directly to the ſource of heat. Though fluids, then, when placed upon the fire, acquire heat very ſpeedily, we are not entitled to conclude, from this circumſtance, that they are conductors of caloric. This point can only be determined by direct experiments.

The experiments which Count Rumford * made for this purpoſe may be divided into two ſets. The firſt ſet was made in order to ſhow that when the internal motions of fluids are impeded, they are not heated ſo ſoon as when their internal motions are unimpeded. This, I think, he has very completely eſtabliſhed. But as fluids are not heated by their conducting power, it does not follow, from theſe experiments, that fluids are non-conductors. They demonſtrate merely that, if fluids be conductors, their conducting power is much inferior to the power by which they are heated. Count Rumford appears to have ſeen this himſelf, and to have looked upon theſe experiments rather as auxiliaries then as ſatisfactory proofs.

The ſecond ſet of experiments was made in order to prove that caloric cannot paſs *downwards* through fluids. Now, as caloric can paſs downwards through all conducting bodies, and muſt indeed, from the very nature of the thing, paſs in every direction through them, it follows that, if it cannot move downwards through fluids, they are, in fact, *non-conductors*. Theſe experiments, then, are perfectly in point. Let us conſider them a little.

To ſee whether caloric moves downwards, through water, he fixed a cake of ice in the bottom of a glaſs jar, covered ¼ inch thick with cold water. Over this was poured gently a conſiderable quantity of boiling water. Now, if water were a non-conductor, no caloric would paſs through the cold water, and conſequently none of the ice would be melted. The melting of the ice, then, was to determine whether water be a conductor or not. In two hours about half of the ice was melted. This, one would think, at firſt ſight, a deciſive proof that water is a conductor. But the Count has fallen upon a very ingenious method of accounting for the melting of the ice, "without being under the neceſſity," as he tells us, "of renouncing his theory that fluids are non-conductors."

It is well known that the ſpecific gravity of water at 40° is a maximum: if it be either heated above 40°, or cooled down below 40°, its denſity diminiſhes. Therefore, whenever a particle of water arrives at the temperature of 40°, it will ſink to the bottom of the veſſel. Now, as the water next the ice was at 32°, it is evident that whenever any part of the hot water was cooled down to 40°, it would ſink, diſplace the water at 32°, come into contact with the ice, and, of courſe, melt it. The Count's ingenuity, never without reſources, enabled him to prove completely that the ice employed in his experiment was actually melted in that manner. For when he covered the ice partially with ſlips of wood, that part which was ſhaded by the wood was not melted; and when he covered the whole of the ice with a thin plate of tin, having a circular hole in the middle, only the part exactly under the hole was melted. From theſe facts it certainly may be concluded that the ice was melted by deſcending currents of water.

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* Count Rumford's experiments are detailed in our Journal; for which ſee the Indexes.---N.

But the point to be proved is not whether there were deſcending currents, but whether water be a conductor or not. Now, if water be a non-conductor, I aſk how the hot water was cooled down to 40°? Not at the ſurface; for the Count himſelf tells us, that there the temperature was never under 108°; not by the ſides of the veſſel; for the deſcending current in one experiment was exactly in the axis; and it follows irreſiſtibly from the experiment with the ſlips of wood, that theſe deſcending currents fell equally upon every part of the ſurface of the ice; which would have been impoſſible, if theſe currents had been cooled by the ſides of the veſſel. The hot water, then, muſt have been cooled down to 40° by the cold water below it; conſequently, it muſt have imparted caloric to this cold water. If ſo, one particle of water is capable of abſorbing caloric from another; that is, water is a *conductor of caloric*. After the hot water had ſtood an hour over the ice, its temperature was as follows:

At the ſurface of the ice - - - - - -	40°
1 inch above the ice - - - - - -	80
2 inches - - - - - - - - -	118
3 inches - - - - - - - - -	128
4 inches - - - - - - - - -	130
7 inches - - - - - - - - -	131

How is it poſſible to account for this gradual diminution of heat as we approach the ice, if water be a non-conductor? The water, it may be ſaid, gives out caloric at its ſurface, falls down and arranges itſelf according to its ſpecific gravity. If ſo, how comes it, that there is only one degree of difference between the temperature at 4 and at 7 inches above the ice? Thus it appears that the Count's experiment, inſtead of demonſtrating that water is a non-conductor, rather favours the ſuppoſition that it is a conductor.

The Count drew, as a corollary, from this experiment, that water at 41° will melt as much ice in a given time as boiling water, when both ſtand over the ice. He found this actually to hold; or rather he concluded from his experiments, aſſiſted, however, by a little calculation, that water at 41° will melt more ice in a given time than boiling water. It would not be difficult, perhaps, to find ſome flaws in the calculation; but granting the truth of the fact, I do not perceive how it contributes in the ſmalleſt degree to prove that water is a non-conductor. For water at 41° (being denſer than water at 32°) melts the ice, not by its conducting power; but by actually travelling to it particle after particle, and giving out its caloric: whereas, boiling water can only act by its conducting power, till ſome part of it be cooled down to 40°. And as water is confeſſedly a very bad conductor, it is a very conſiderable time before much of it is cooled down ſo low. Accordingly we find that after 20′ the boiling water at the diſtance of only one inch above the ice was 130°, and of half an inch 46°. Water at 41°, then, has an advantage over boiling water, and this advantage is the greater the worſe a conductor of caloric the water is.

I ſhall not be ſo particular in my examination of Count Rumford's experiments made in order to prove that *mercury* and *oil* are non-conductors of caloric; becauſe they are not ſuſceptible of ſufficient accuracy to decide the point. The iron cylinder would retain its heat too ſhort a time to melt much ice, even if theſe fluids had been excellent conductors: and I really do not ſee how it was poſſible to detect a very ſmall quantity of water, even if ſome of the ice had been melted. But if the Count had made uſe of a thermometer inſtead of ice, it would have riſen ſeveral degrees, as I have found by making the experiment, even though the hot iron had been at a greater diſtance from the bulb, than his was from the nipple of ice.

It appears, then, that Count Rumford's experiments are not deciſive, or if any concluſion can be drawn from them, it is rather favourable to the ſuppoſition that fluids are *conductors of caloric*. It occured to me, that by varying Count Rumford's experiments a little, the point in queſtion might be decided with certainty. I took a tubulated glaſs receiver M, Fig. 2, Pl. XXIII. into which there paſſed the thermometer B. This thermometer was ſo placed, that its bulb was exactly in the axis of the veſſel. The mouth at which it entered was cloſed with cement. The degrees were marked upon the tube itſelf, and were all on that part of it which was without the receiver. Into this veſſel I poured the fluid, whoſe conducting power I wiſhed to examine, till it ſtood at a certain height above the thermometer B, ſuppoſe at D. I then poured cautiouſly over it (ſuppoſe to the height E) another fluid of inferior ſpecific gravity, and heated to a certain temperature. Now, if the thermometer B did not riſe, it would follow that the fluid was a non-conductor; if it did riſe, that it was a conductor. A variety of precautions were neceſſary to prevent being miſled; but theſe will appear from the experiments themſelves, which I ſhall now detail.

The firſt fluid made choice of was mercury. I filled the veſſel M with mercury till it ſtood 0.2 inch above the bulb of the thermometer B. Over this was poured cold water, till it ſtood ½ inch above the ſurface of the mercury. I then cautiouſly poured in boiling water till it ſtood about two inches above the ſurface of the mercury. The thermometer A was ſuſpended in the water, ſo that its bulb approached very near the ſurface of the mercury. There was alſo a third thermometer C, whoſe bulb, which was oblong, reſted againſt the bottom of the veſſel. The temperature of the mercury and of the external air was 42°. That of the hot water, as ſoon as it could be obſerved after pouring it in (which took up ſome time), was 174°. The reſult of the experiment was as follows:

EXPERIMENT

EXPERIMENT I.

Thermometer B roſe to 80° during the pouring in of the water.

Time.	Therm. A.	Therm. B.	Therm. C.	Water 1 inch below the ſurface.
In 2′	126°	85°	67°	174°
5	111	90	83	163
10	106	93½	88	152
12		95		
15	104	96	91	144
21	102	96	91¾	133
25	100	96	91¾	128
28	98	95	91¾	122
35	97	93	89½	116
40	95	91	88	100
45	93	90	87	106
50	91	88	85	102
55	88	86	84	99
60	86	85	82	96
65	84	83½	81	92
70	83	82	79	90
75	80	80	78	88
80	79	78	76	85
85	77½	77	75	83
90	76	76	73	80
95	74	74	72	76

Here the thermometer B roſe in 15 minutes 16°; and in 21′ the thermometer C roſe 24.75°: all this time the thermometer A was gradually falling, and approaching nearer and nearer the temperature of B, and after 95′ the two were preciſely of the ſame temperature. The maximum of B was 96°, of C 91.75°. B firſt acquired its maximum, then C: B began to fall in 25′, C in 28′. Now all theſe circumſtances are preciſely what ought to happen if mercury be a conductor of caloric. The experiment, therefore, ſeems to prove that mercury actually conducts caloric: for we ſee that caloric paſſes downwards through mercury, which could not happen if mercury were a non-conductor.

Though this experiment ſeemed deciſive, I wiſhed to repeat it in a different manner, in order, if poſſible, to determine the internal motions of the hot liquid, that I might aſcertain whether or not theſe motions had any influence on the riſe of the thermometers. Count Rumford's ingenious method readily preſented itſelf. Accordingly I poured over the mercury a ſaturated ſolution of carbonate of pot-aſh in water, till it ſtood ½ inch above its ſurface. Then boiling carbonate of pot-aſh was cautiouſly poured over all, till it ſtood two inches above the ſurface of the mercury. As this ſolution had not been filtered, there were floating through it a number of opaque particles, nearly of the ſame ſpecific gravity with the ſolution. By the motion of theſe I expected to be able to judge of the currents in the hot liquid. The temperature of the mercury and of the external air was 41½°. That of the carbonate, when firſt tried after being poured in, 174°. The reſult of the experiment was as follows:

EXPERIMENT II.

Thermometer B roſe during the pouring in of the carbonate to 75°.

Time.	Therm. A.	Therm. B.	Therm. C.
In 1′	118°	78	55°
2	109	80	67
6	103	85	78
10	100	88	84
15	98	90	85
20	96	90	87
25	94	90	86
30	92	89	86
35	90	87	85
40	87	85	83
45	85	84	82
50	84	83	80
55	82	80	78½
60	80	79½	77
65	78	77	75½
70	76½	76	74
75	75	74½	73½
80	73	73	70½
85	72	72	70
90	70	70	68½
95	69	69	67
100	68	67½	66
105	66½	66	65
110	65	65	64
115	64½	64	63
120	63	63	62

Here the reſult is almoſt the ſame as in experiment firſt. It is therefore a confirmation of it, and makes the concluſion drawn from it, that mercury conducts caloric, more to be depended on.

Soon after pouring in the hot carbonate, I obſerved the opaque particles formerly mentioned, moving pretty rapidly upwards and downwards, indicating Count Rumford's currents. Theſe motions did not depend upon the ſides of the veſſel, for particles were aſcending and deſcending in every part of the liquid. They aſcended with great rapidity till they came within a certain diſtance of the ſurface; then they deſcended again as rapidly as they had aſcended, till they came within a certain diſtance of the mercury; here they became ſtationary for a moment, and then aſcended again with as much velocity as at firſt. Sometimes a few particles aſcended to the very ſurface, and then deſcended again with very great rapidity. But they conſtantly ſtopped, and began to move upwards when they came to a certain diſtance from the mercury; and they all ſtopped at the ſame diſtance from the mercury. At firſt this diſtance was about half an inch, but it became gradually greater as the proceſs of cooling went on, and at laſt the particles did not deſcend lower than the middle of the liquid. If any particle happened to go beyond this boundary, and to approach a little nearer the mercury, it ſoon became ſtationary, and did not riſe any more. The rapidity of the motions of theſe particles gradually diminiſhed, as well as the ſpace through which they moved: after 40′ I could not obſerve them moving at all; though, when I marked the ſituation of a particle, and then obſerved it ſome time after, it became evident that it had changed its place.

Theſe motions are curious, and give us a good deal of information about the proceſs of cooling in the hot liquid. I pretend not to explain their rapidity; but, waving that difficulty, it ſeems pretty clear that the particles near the ſurface giving out their heat, became ſpecifically heavier, and tumbled down towards the mercury. They did not reach the mercury, however, becauſe at ſome diſtance from it they came to a part of the liquid colder than themſelves. If theſe deſcending particles happened, in conſequence of a momentum accidentally greater than uſual, to penetrate into this cold ſtratum, they loſt their exceſs of caloric before they could riſe again, and, of courſe, continued ever after ſtationary in the cold ſtratum. Theſe deſcending currents neceſſarily produced aſcending currents; but theſe aſcending particles could ſcarcely reach the ſurface, unleſs their momentum happened to be unuſually great, becauſe the water being hotteſt juſt below the ſurface, they ſoon came to particles ſpecifically lighter than themſelves. As the cooling advanced, the cold ſtratum, of courſe, increaſed, and the currents moved more ſlowly, becauſe the water at the ſurface, approaching to the temperature of the air, was cooled more ſlowly. We ſee, then, that the deſcending currents do not reach the mercury; conſequently, they cannot affect its heating except indirectly; far leſs can they affect the temperature of the thermometer.

Wiſhing, now, to try a hot liquid of a different kind, I poured lintſeed oil, at 230°, over the mercury, till it ſtood an inch and a half above its ſurface. The temperature of the mercury, and of the external air, was 41¼°; that of the oil, as ſoon as it could be aſcertained, after pouring it in, was 180°. The reſult was follows:

EXPERIMENT

EXPERIMENT III.

Thermometer B roſe during the pouring to 50°.

Time.	Therm. A.	Therm. B.	Therm. C.	
2′		56°	50°	
5	91°	60	56	
10	82	63	60½	
15	77	64	63	
20	74	64½	62¾	
25	70	64½	62¾	
30	67¼	63½	62	
35	67½	63	61½	
40	64	62	60¾	
45	62½	61	59¾	
50	61	60	58¾	
55	59½	58½	57¾	
60	58½	58	57	
65	57	57	56	Oil
70	56	56	55	58

Here the general reſult is preciſely the ſame as in the former experiments. The only remarkable difference is the ſmall riſe of the thermometer during the pouring in of the hot oil. During that operation it roſe only 8½ °, whereas in the two former experiments its riſe was nearly 30°.

Theſe three experiments were varied different ways, but the reſults were ſo nearly the ſame, that it would be both uſeleſs and tireſome to relate the whole. They ſeem to me to put it beyond a doubt that mercury is a *conductor of caloric.*

I wiſhed now to try whether water alſo be a conductor. For this purpoſe I poured water at 40° (the temperature of the air) into the veſſel M, till it ſtood 0.8 inch above the bulb of the thermometer B. Over this I poured a thin ſtratum of lintſeed oil. Then lintſeed oil, heated to 220°, was poured cautiouſly over all, till it ſtood 1.15 inches above the ſurface of the water. The temperature of the oil, as ſoon as it could be aſcertained after it had been poured in, was 160°. The reſult of the experiment was as follows:

EXPERIMENT IV.

Thermometer B roſe during the pouring 0°.

Time.	Therm. A.	Therm. B.	Therm. C.
In 5′		41°	
10	79°	42	
15	70	43	42½
20	68	44	43
25	65	45	43
30	63	46	43
35	61	46½	43
40	59	46½	43
45	57	46½	43½

We ſee from this experiment that water is a conductor of caloric; but a much worſe conductor than mercury. The thermometer B did not riſe during the pouring in of the oil, partly becauſe it was at a conſiderable diſtance from the ſurface, and partly becauſe water conducts worſe than mercury. But it did riſe afterwards 6½ °. It was 5′ before it began to riſe, and 35′ before it reached its maximum. The reſult of this experiment was ſo curious, that I wiſhed to repeat it with ſome variation. I put all the three thermometers in the water: thermometer A 0.2 inch below the ſurface, thermometer B 0.7 inch below the ſurface, and thermometer C 1.725 inches below the ſurface. The cold water was 2.2 inches deep; its temperature was 42° (that of the air). A ſtratum of cold lintſeed oil was poured over the water, then lintſeed oil, at 220°, was poured over all, till it ſtood 2.125 inches above the ſurface of the water. The reſult was as follows:

EXPERIMENT V.

Time.	Therm. A.	Therm. B.	Therm. C.
In 2′	58°	45°	
5	59	46	42
10	60	49	42.75
15	60	50	43
20	60	51	43.5
25	59	51½	44
30	59	52	44.5
35	58	52	45
40	57	52	45.5
45	56½	52	46
50	56	52	46.5
55	55½	51½	46.75

Time.

Time.	Therm. A.	Therm. B.	Therm. C.
In 60′	55°	51½°	47°
65	54	51¼	47.25
70	53½	51	47.5
75	53	51	48
80	53	51	48
85	52½	50½	48
90	52¼	50¼	48.25
95	52	50½	48.25
100	51½	50	48.25
105	51	50	48.5
110	51	49½	48.5
115	51	49¼	48.5
120	50½	49	48.25
125	50	48¾	48.25
130	50	48¾	48.25
223	47	46	46

Here the thermometer A roſe 18° and reached its maximum in 11′
B 10 31
C 6.5 106

This experiment ſeems to me to prove, in a very clear manner, that water is a conductor. But before we draw any ſuch concluſion from it, or from thoſe before detailed, it will be proper to enquire whether the riſing of the thermometers can be accounted for in any other way. If it cannot, then water and mercury muſt be conſidered as conductors. Now, it may be ſaid, that the thermometers roſe in conſequence of the action of *tranſmitted* caloric; or that caloric was *conducted* to them by means of the glaſs veſſel in which the experiment was made; or that *currents* of the hot liquid made their way to them; or, laſtly, that the cold liquid was thrown into ſuch *agitation* by the pouring in of the hot liquid over it, that currents of it heated directly by the contact of the hot liquid made their way to the thermometers. If none of theſe ſuppoſitions will account for the riſe of the thermometers, we muſt conclude from the experiments that mercury and water are conductors of caloric.

1. If the thermometers had riſen in conſequence of caloric tranſmitted to them through the cold liquids, they ought to have begun to riſe at the ſame inſtant; or, rather, the loweſt, which was the moſt delicate, ought to have begun to riſe firſt. But the contrary is evident from all the experiments. In the laſt experiment, for inſtance, the thermometor B did not begin to riſe till ſome time after the oil had been poured in; and thermometer C not till more than 6′ after I had begun to obſerve the thermometers, and, conſequently, not till at leaſt 12′ after beginning to pour in the oil. Thermometer B reached its maximum in 31′,

31′, C in 106′; a length of time abſolutely incompatible with every idea reſpecting tranſmitted caloric. It is not poſſible, then, that the thermometers could have riſen in conſequence of the action of tranſmitted caloric.

2. If the caloric had been conducted to the thermometers by the veſſel, C, which was always neareſt the veſſel, ought to have riſen firſt, or at leaſt, as its ſituation was preciſely the ſame, it ought to have riſen the ſame number of degrees in experiments 3d and 4th. In theſe the hot oil was the ſame; it ought therefore to have communicated the ſame quantity of caloric to the veſſel: but in experiment 3d, C roſe both ſooner and higher than in experiment 4th. This is eaſily explained, on the ſuppoſition that water is a worſe conductor than mercury; but it is inexplicable, on the ſuppoſition that the heat was communicated by the veſſel. The following experiment proves clearly that this ſuppoſition is without foundation.

To determine whether the cold liquid was thrown into waves, or whether currents were formed in it either by pouring the hot liquid over it, or during the proceſs of heating, I took a ſolution of carbonate of pot-aſh, with amber floating through it, prepared in the way deſcribed by Count Rumford, and poured it into the veſſel M till it ſtood 0.3 inch above the thermometer B. Over this was poured cold water to the height of half an inch. The two ſurfaces kept perfectly diſtinct, as was evident from the amber, ſome of which was floating at the ſurface of the carbonate. Over all I poured boiling water, tinged blue with the juice of red cabbage. The blue colour hung down in clouds through rather more than the half of the pure water; but none of it approached the carbonate. The temperature of the carbonate was 40° (that of the air). Thermometer B roſe during the pouring to 73°. Thermometers A and C placed as in experiment 1ſt. The reſult of this experiment was as follows:

EXPERIMENT VI.

Time.	Therm. A.	Therm. B.	Therm. C.
In 4′	105°	75°	43°
10	102	78	46
15	98	79	47½
20	96	80	50
25	93	80	53
30	90	79	53½
35	88	78	55
40	86	77	56
45	83	76½	57
50	81	76	58
55	79½	75	58½
60	78	74	58¾

Time.	Therm. A.	Therm. B.	Therm. C.
In 65′	76°	73°	59°
70	74	72	59
75	72	70	59
80	71½	69	59
127	62	62	56
130	61¾	61¾	56
135	61	60½	56

During the whole of this experiment the amber continued perfectly motionleſs; conſequently there were no currents. But there muſt have been currents, if the heat had been communicated by the ſides of the veſſel; for in that caſe the particles of the liquid next the ſides muſt have received heat: this would have cauſed them to aſcend, and there would have been deſcending currents in the interior parts of the liquid. It is not poſſible, then, to ſuppoſe that the thermometers roſe in conſequence of caloric conducted to them by the ſides of the veſſel.

3. If the thermometers had riſen in conſequence of currents of the hot liquid ſtriking againſt them, theſe currents would have been viſible in ſeveral of the experiments. If the ſmalleſt particle of oil, for inſtance, had made its way through the water, it would have been detected by its whiteneſs. We are abſolutely certain that no ſuch currents exiſted in the 6th experiment~~; for~~ had the ſmalleſt atom of the water, tinged with red cabbage, made its way through the carbonate, it would have been inſtantly detected by a change of colour from blue to green. But no ſuch change of colour took place: nay, more than ſix hours after the experiment, no green colour could be perceived; but it appeared inſtantly after a very ſlight agitation. Beſides, how could oil or water makes its way downwards through mercury? This ſuppoſition, then, muſt alſo be given up.

4. If the cold liquid had been ſo much agitated by pouring the hot liquid over it, that currents of it, heated directly by the hot liquid, made their way to the thermometers, theſe currents muſt have been viſible in the 6th experiment, which was made on purpoſe to detect them. Now, as the amber remained perfectly motionleſs, even during the pouring in of the hot liquid, and never moved after, it follows that no ſuch currents exiſted. Conſequently, the fourth ſuppoſition cannot be admitted.

Upon the whole, then, I conſider theſe experiments as proving, beyond a doubt, that water and mercury are conductors of caloric. I have repeated them frequently, in order to guard as much as poſſible againſt miſtakes: but I truſt that others, who are intereſted in this ſubject, will take the trouble to repeat them. I may, perhaps, have overlooked ſomething, notwithſtanding all my caution. If I have, the experiments are ſo eaſily made, that it cannot fail to be very ſoon diſcovered. Whatever may be the iſſue of this inveſtigation, the greateſt part of the merit belongs excluſively to Count Rumford. My experiments were made merely in conſequence of his, and I availed myſelf of the contrivances which he himſelf had ſuggeſted.

To ſee whether ſulphuric acid be a conductor of caloric, I made the following experiment:

EXPERIMENT VII.

Into the veſſel M was poured concentrated ſulphuric acid, till it ſtood 0.6 inch above the thermometer B. The temperature was 43° (that of the air). Over this was poured cautiouſly a ſaturated aqueous ſolution of ſulphate of pot-aſh with exceſs of acid, at 43°, till it ſtood 0.4 inch above the acid. The two liquids remained perfectly diſtinct, as was evident from the difference of their colours and conſiſtence; for the acid was a little coloured. The ſurface where they joined reflected light very ſtrongly. The thermometer A was ſuſpended, ſo that its whole bulb was in the ſulphate; thermometer C was 1.366 inches under the ſurface of the acid; the acid began ſlowly to combine with the water above it, and this combination gradually evolved heat. I expected that this heat would make its way downwards, and raiſe the thermometers. The reſult was as follows:

Time.	Therm. A.	Therm. B.	Therm. C.
In 3′		49°	
5		50	
10		54	
15		56	47°
20	82*	58	47¼
25	82	60	49
30	82	60¼	50
35	82	61	51
40	81	61½	51¼
45	80	62	52¼
50	79½	62½	52¾
55	79	63	53½
60	78½	63	54
65	77½	63	54½
70	77	63½	55
75	76½	63½	55¼
80	76	63	55¾
85	75	63	56
90	74	63	56¼
95	73½	62½	56¼
100	73	62½	56¼
105	72½	62½	56¼
110	72	62	56¼
115	71½	62	56¼
120	71	61¾	56¼
125	70	61¾	56
130	70	61½	56
230	62	58	53

Two days after this experiment was made, the ſurfaces of the acid and ſulphate were ſtill as diſtinct as ever; but the ſtratum of ſulphate was nearly double its original thickneſs, owing evidently to its having combined with a conſiderable portion of the acid. This experiment, I think, proves that ſulphuric acid is alſo a conductor of caloric. Indeed, it is more than probable that all fluids are conductors.

II.

Deſcription of a Steam Engine on the Principle of Savary, operating by a ſeparate Condenſer; with other eſſential Improvements. By Mr. JOHN NANCARROW*.

PLATE XXIII. Fig. 3. A. The receiver, which may be made either of wood or iron.
BBBB. Wooden or caſt-iron pipes for conveying the water to the receiver, and from thence to the penſtock.
C. The penſtock or ciſtern.
D. The water-wheel.
E. The boiler, which may be either iron or copper.
F. The hot-well for ſupplying the boiler with water.
GG. Two ciſterns under the level of the water, in which the ſmall bores BB, and the condenſer are contained.
HHH. The ſurface of the water with which the ſteam-engine and water-wheel are ſupplied.
aa. The ſteam-pipe, through which the ſteam is conveyed from the boiler to the receiver.
b. The feeding-pipe, for ſupplying the boiler with hot water.
ccccc. The condenſing apparatus.
dd. The pipe which conveys the hot water from the condenſer to the hot-well.
eee. Valves for admitting and excluding the water.
ff. The injection pipe, and *g* the injection cock.
h. The condenſer.

It does not appear neceſſary to ſay any thing here on the manner in which this machine performs its operations without manual aſſiſtance, as the method of opening the cocks by which the ſteam is admitted into the receiver and condenſed, has been already well deſcribed by ſeveral writers. But it will be neceſſary to remark that the receiver, penſtock, and all the pipes, muſt be previouſly filled † before any water can be delivered on the wheel;

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* From a learned paper on Mills, in the American Tranſactions, IV. page 355. I have not been able to extract the theoretical parts of the paper, becauſe the plates and references are too imperfect.

† Or the air blown out by ſteam; which may perhaps be leſs convenient than the method in the text.--N.

wheel, and when the ſteam in the boiler has acquired a ſufficient ſtrength, the valve at *c* is opened, and the ſteam immediately ruſhes from the boiler at E into the receiver A, the water deſcends through the tubes A and B, and aſcends through the valve *e* and the other pipe or tube B into the penſtock C. This part of the operation being performed and the valve *c* ſhut, that at *a* is ſuddenly opened, through which the ſteam ruſhes down the condenſing pipe *c*, and in its paſſage meets with a jet of cold water from the injection cock *g*, by which it is condenſed. A vacuum being made by this means in the receiver, the water is driven up to fill it a ſecond time through the valves *ee*, by the preſſure of the external air, when the ſteam-valve at *c* is again opened, and the operation repeated for any length of time the machine is required to work.

There are many advantages which a ſteam-engine on this conſtruction poſſeſſes, beyond any thing of the kind hitherto invented; a few of which I ſhall beg leave to enumerate.

1. It is ſubject to little or no friction.

2. It may be erected at a ſmall expence when compared with any other ſort of ſteam-engine.

3. It has every advantage which may be attributed to Boulton and Watt's engines, by condenſing out of the receiver, either in the penſtock or at the level of the water.

4. Another very great advantage is, that the water in the upper part of the pipe[*] adjoining the receiver, acquires a heat by its being in frequent contact with the ſteam, very nearly equal to that of boiling water; hence the receiver is always kept uniformly hot as in the caſe of Boulton and Watt's engines.

5. A very ſmall ſtream of water is ſufficient to ſupply this engine, (even where there is no fall) for all the water raiſed by it is returned into the reſervoir HHH.

From the foregoing reaſons it manifeſtly appears that no kind of ſteam-engine is ſo well adapted to give rotatory motion to machinery of every kind as this. Its form is ſimple, and the materials of which it is compoſed are cheap; the power is more than equal to any other machine of the kind, becauſe there is no deduction to be made for friction, except on account of turning the cocks, which is but trifling.

Its great utility is therefore evident in ſupplying water for every kind of work performed by a water-wheel, ſuch as griſt-mills, ſaw-mills, blaſt-furnaces, forges, &c.

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[*] Not being thrown out by a ſide aperture, as in Plate XVII. Vol. I. of our Journal, but merely raiſed and depreſſed in the pipe AB.---N.

III. *New*

III.

New Application of the Syphon to raiſe Water above the Surface of the Reſervoir. In a Letter from Mr. WILLIAM CLOSE.

To Mr. NICHOLSON.

SIR,

THE ſyphon will raiſe a ſtream of water through an extenſive ſpace in every ſituation where a little deſcent can be procured, but while the operation continues, no water can be taken directly out of the ſtream above the loweſt part of the tube. When, however, the two open ends of a ſyphon are cloſed, a quantity of water may be let out of the higheſt part, and its place ſupplied by introducing a like quantity of no uſe: all the avenues for the purpoſe being then cloſed, and the ſtream ſuffered to flow through the tube; the uſeleſs water will be diſplaced, and a freſh quantity may be ſoon after drawn off. This mode of exchange may be uſeful in furniſhing a ſupply for waſhing, and ſome other purpoſes, but there are ſeveral domeſtic uſes for which the water drawn off will not be thought ſufficiently pure. A method of taking water out of the ſyphon at any height within the limits of the elevation, without retarding the ſtream, or introducing another quantity, has often appeared to me very deſirable, and ſome months ago, I made a number of experiments to determine the practicability of the project.

My hopes of ſucceſs were founded upon the following obſervations:

When water flows through a ſyphon that has an elbow or dilatation containing air, the ſtream moves much ſlower, and is ſmaller on entering into the external air, than when the part is completely filled with water. When the current moves with a certain velocity, its lateral action extracts the air, and carries it down the deſcending column. As the part fills with water, the ſtream becomes fuller, and its velocity encreaſes.

Among a variety of projects for extracting the air out of a veſſel fixed to the ſide of a ſyphon, and filling it with water, by the aſſiſtance of the lateral action of the ſtream, the following ſeemed the moſt likely:

If the connection between the veſſel and ſyphon be made by two tubes, the end of one turned againſt the current, and the end of the other turned from it, the ſtream after ſtriking the end of the firſt tube, will be contracted until it has paſſed by the end of the ſecond. After this it will begin to expand; the air will be drawn out of the veſſel through the tube turned from the current, and water will enter by the other, until the veſſel is filled; the ſtream beyond the contraction occaſioned by the two tubes will then receive a ſupply through the veſſel.

The reſult of an experiment founded upon this theory was very ſatisfactory. In place, however, of a particular account of the firſt experiment, I ſhall give a detail of ſome others, in which a ſtill more eaſy expedient was adopted, to produce the ſame effect:

Two

Soon after pouring in the hot carbonate, I obſerved the opaque particles formerly mentioned, moving pretty rapidly upwards and downwards, indicating Count Rumford's currents. Theſe motions did not depend upon the ſides of the veſſel, for particles were aſcending and deſcending in every part of the liquid. They aſcended with great rapidity till they came within a certain diſtance of the ſurface; then they deſcended again as rapidly as they had aſcended, till they came within a certain diſtance of the mercury; here they became ſtationary for a moment, and then aſcended again with as much velocity as at firſt. Sometimes a few particles aſcended to the very ſurface, and then deſcended again with very great rapidity. But they conſtantly ſtopped, and began to move upwards when they came to a certain diſtance from the mercury; and they all ſtopped at the ſame diſtance from the mercury. At firſt this diſtance was about half an inch, but it became gradually greater as the proceſs of cooling went on, and at laſt the particles did not deſcend lower than the middle of the liquid. If any particle happened to go beyond this boundary, and to approach a little nearer the mercury, it ſoon became ſtationary, and did not riſe any more. The rapidity of the motions of theſe particles gradually diminiſhed, as well as the ſpace through which they moved: after 40′ I could not obſerve them moving at all; though, when I marked the ſituation of a particle, and then obſerved it ſome time after, it became evident that it had changed its place.

Theſe motions are curious, and give us a good deal of information about the proceſs of cooling in the hot liquid. I pretend not to explain their rapidity; but, waving that difficulty, it ſeems pretty clear that the particles near the ſurface giving out their heat, became ſpecifically heavier, and tumbled down towards the mercury. They did not reach the mercury, however, becauſe at ſome diſtance from it they came to a part of the liquid colder than themſelves. If theſe deſcending particles happened, in conſequence of a momentum accidentally greater than uſual, to penetrate into this cold ſtratum, they loſt their exceſs of caloric before they could riſe again, and, of courſe, continued ever after ſtationary in the cold ſtratum. Theſe deſcending currents neceſſarily produced aſcending currents; but theſe aſcending particles could ſcarcely reach the ſurface, unleſs their momentum happened to be unuſually great, becauſe the water being hotteſt juſt below the ſurface, they ſoon came to particles ſpecifically lighter than themſelves. As the cooling advanced, the cold ſtratum, of courſe, increaſed, and the currents moved more ſlowly, becauſe the water at the ſurface, approaching to the temperature of the air, was cooled more ſlowly. We ſee, then, that the deſcending currents do not reach the mercury; conſequently, they cannot affect its heating except indirectly; far leſs can they affect the temperature of the thermometer.

Wiſhing, now, to try a hot liquid of a different kind, I poured linſeed oil, at 230°, over the mercury, till it ſtood an inch and a half above its ſurface. The temperature of the mercury, and of the external air, was 41½°; that of the oil, as ſoon as it could be aſcertained, after pouring it in, was 180°. The reſult was follows:

EXPERIMENT

EXPERIMENT III.

Thermometer B roſe during the pouring to 50°.

Time.	Therm. A.	Therm. B.	Therm. C.	
2′		56°	50°	
5	91°	60	56	
10	82	63	60½	
15	77	64	63	
20	74	64½	62¾	
25	70	64½	62¾	
30	67¼	63½	62	
35	67½	63	61½	
40	64	62	60¾	
45	62½	61	59¾	
50	61	60	58¾	
55	59½	58½	57¾	
60	58½	58	57	
65	57	57	56	Oil
70	56	56	55	58

Here the general reſult is preciſely the ſame as in the former experiments. The only remarkable difference is the ſmall riſe of the thermometer during the pouring in of the hot oil. During that operation it roſe only 8½ °, whereas in the two former experiments its riſe was nearly 30°.

Theſe three experiments were varied different ways, but the reſults were ſo nearly the ſame, that it would be both uſeleſs and tireſome to relate the whole. They ſeem to me to put it beyond a doubt that mercury is a *conductor of caloric.*

I wiſhed now to try whether water alſo be a conductor. For this purpoſe I poured water at 40° (the temperature of the air) into the veſſel M, till it ſtood 0.8 inch above the bulb of the thermometer B. Over this I poured a thin ſtratum of lintſeed oil. Then lintſeed oil, heated to 220°, was poured cautiouſly over all, till it ſtood 1.15 inches above the ſurface of the water. The temperature of the oil, as ſoon as it could be aſcertained after it had been poured in, was 160°. The reſult of the experiment was as follows:

In this arrangement the firſt veſſel will empty itſelf into the ſecond, which will cloſe the cocks in the pipes oppoſite A and W, before air is admitted into the veſſel A W. The third will be filled from the ſecond, and the water in the veſſel A W will be let out. Again, the third will deliver its contents in the fourth or loweſt, which will keep the cocks in the ſmall pipes oppoſite A and W cloſe, until after the third veſſel is empty, has riſen up, and the external air can no longer enter the veſſel A W. The fourth being then emptied by its ſyphon, the pipes between the veſſel A W and ſyphon S Y will open.

The diameter of the ſecond veſſel ſhould be ſomething leſs than either that above or below it. The fourth ſhould be filled before the ſecond is empty; the third will deſcend the laſt and riſe the firſt: the ſecond and fourth will riſe together immediately after the third. If the ſecond and fourth were to riſe before the third, the ſyphon would directly receive a quantity of external air, and its operations would ceaſe. It will therefore require much caution to manage the cocks and valves, if another veſſel, ſimilar to A W, is to be filled while this laſt is emptied, and emptied while it is filled.

The veſſel A W ſhould not be large, and in order to overcome the buoyance of the extracted air, it is adviceable to make the length of the deſcending branch of the ſyphon exceed the length of the aſcending one as much as circumſtances will admit, and to let the loweſt part of it be made of a conical divergent form. The velocity of the ſtream will be thus encreaſed; the veſſel A W will be ſooner filled with water; and the depreſſion of the two columns will be leſs liable to happen from very ſlight imperfections of workmanſhip.

I am, SIR,

Your humble ſervant,

Dalton, Feb. 17, 1801. WILLIAM CLOSE.

IV.

On the Power of penetrating into Space by Teleſcopes; with a comparative Determination of the Extent of that Power in natural Viſion, and in Teleſcopes of various Sizes and Conſtructions; illuſtrated by ſelect Obſervations. By WILLIAM HERSCHEL, *LL.D. F.R.S.*

(Continued from page 506.*)*

WE may now proceed to determine the powers of the inſtruments that have been uſed in my aſtronomical obſervations; but, as this ſubject will be beſt explained by a report of the obſervations themſelves, I ſhall ſelect a ſeries of them for that purpoſe, and relate them in the order which will be moſt illuſtrating.

Firſt, with regard to the eye, it is certain that its power, like all our other faculties, is limited by nature, and is regulated by the permanent brightneſs of objects; as has been ſhewn already, when its extent with reflected light was compared to its exertion on ſelf-luminous objects. It is further limited on borrowed light, by the occaſional ſtate of illumination; for, when that becomes defective at any time, the power of the eye will then be contracted into a narrower compaſs; an inſtance of which is the following:

In the year 1776, when I had erected a teleſcope of 20 feet focal length, of the Newtonian conſtruction, one of its effects by trial was, that when towards evening, on account of darkneſs, the natural eye could not penetrate far into ſpace, the teleſcope poſſeſſed that power ſufficiently to ſhew, by the dial of a diſtant church ſteeple, what o'clock it was, notwithſtanding the naked eye could no longer ſee the ſteeple itſelf. Here I only ſpeak of the penetrating power; for, though it might require magnifying power to ſee the figures on the dial, it could require none to ſee the ſteeple. Now the aperture of the teleſcope being 12 inches, and the conſtruction of the Newtonian form, its penetrating power, when calculated according to the given formula, will be $\frac{\sqrt{,429 \times \overline{120^2 - 15^2}}}{2} = 38,99.$ *A*, *b*, and *a*, being all expreſſed in tenths of an inch*.

From the reſult of this computation it appears, that the circumſtance of ſeeing ſo well, in the duſk of the evening, may be eaſily accounted for, by a power of this teleſcope to penetrate 39 times farther into ſpace than the natural eye could reach, with objects ſo faintly illuminated.

This obſervation completely refutes an objection to teleſcopic viſion, that may be drawn from what has alſo been demonſtrated by optical writers: namely, that no teleſcope can ſhew an object brighter than it is to the naked eye. For, in order to reconcile this optical theory with experience, I have only to ſay, that the objection is intirely founded on the ſame ambiguity of the word brightneſs that has before been detected. It is perfectly true, that the *intrinſic* illumination of the picture on the retina, which is made by a teleſcope, cannot exceed that of natural viſion; but the *abſolute* brightneſs of the magnified picture by which teleſcopic viſion is performed, muſt exceed that of the picture in natural viſion, in the ſame ratio in which the area of the magnified picture exceeds that of the natural one; ſuppoſing the *intrinſic* brightneſs of both pictures to be the ſame. In our preſent inſtance, the ſteeple and clock-dial were rendered viſible by the increaſed abſolute brightneſs of the object, which in natural viſion was 15 hundred times inferior to what it was in the teleſcope. And this eſtabliſhes beyond a doubt, that teleſcopic viſion is performed by the abſolute brightneſs of objects; for, in the preſent caſe, I find by computation, that the *intrinſic* brightneſs, ſo far from being equal in the teleſcope to that of natural viſion, was inferior to it in the ratio of three to ſeven.

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* I have given the figures, in all the following equations of the calculated penetrating powers, in order to ſhew the conſtructions of my inſtruments to thoſe who may wiſh to be acquainted with them.

The diſtinction between magnifying power, and a power of penetrating into ſpace, could not but be felt long ago, though its theory has not been inquired into. This undoubtedly gave riſe to the invention of thoſe very uſeful ſhort teleſcopes called night-glaſſes. When the darkneſs of the evening curtails the natural penetrating power, they come in very ſeaſonably, to the relief of mariners that are on the look-out for objects which it is their intereſt to diſcover. Night-glaſſes, ſuch as they are now generally made, will have a power of penetrating ſix or ſeven times farther into ſpace than the natural eye. For, by the conſtruction of the double eye-glaſs, theſe teleſcopes will magnify 7 or 8 times; and the object glaſs being $2\frac{1}{2}$ inches in diameter, the breadth of the optic pencil will be $3\frac{1}{8}$ or $3\frac{4}{7}$ tenths of an inch. As this cannot enter the eye, on a ſuppoſition of an opening of the iris of 2 tenths, we are obliged to increaſe the value of a, in order to make the teleſcope have its proper effect. Now, whether nature will admit of ſuch an enlargement becomes an object of experiment; but, at all events, a cannot be aſſumed leſs than $\frac{A}{m}$. Then, if x be taken as has been determined for three refractions, we ſhall have

$$\frac{\sqrt{,853\times 25^2}}{a} = 6,46 \text{ or } 7,39.$$

Soon after the diſcovery of the Georgian planet, a very celebrated obſerver of the heavens, who has added conſiderably to our number of teleſcopic comets and nebulæ, expreſſed his wiſh, in a letter to me, to know by what method I had been led to ſuſpect this object not to be a ſtar, like others of the ſame appearance. I have no doubt but that the inſtrument through which this aſtronomer generally looked out for comets, had a penetrating power much more than ſufficient to ſhew the new planet, ſince even the natural eye will reach it. But here we have an inſtance of the great difference in the effect of the two ſorts of powers of teleſcopes; for, on account of the ſmallneſs of the planet, a different ſort of power, namely, that of magnifying, was required; and, about the time of its diſcovery, I had been remarkably attentive to an improvement of this power, as I happened to be then much in want of it for my very cloſe double ſtars [*].

On examining the nebulæ which had been diſcovered by many celebrated authors, and comparing my obſervations with the account of them in the *Connoiſſance des Temps* for 1783, I found that moſt of thoſe which I could not reſolve into ſtars with inſtruments of a ſmall penetrating power, were eaſily reſolved with teleſcopes of a higher power of this ſort; and, that the effect was not owing to the magnifying power I uſed upon theſe occaſions, will fully appear from the obſervations: for, when the cloſeneſs of the ſtars was ſuch as to require a conſiderable degree of magnifying as well as penetrating power, it always appeared plainly, that the inſtrument which had the higheſt penetrating power reſolved them beſt, provided it had as much of the other power as was required for the purpoſe.

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[*] Magnifying powers of 460, 625, 932, 1159, 1504, 2010, 2398, 3168, 4294, 5489, 6450, 6652, were uſed upon ε Bootis, γ Leonis, α Lyræ, &c. See Cat. of double ſtars, Phil. Tranſ. Vol. LXXII. page 115, and 147; and Vol. LXXV, page 48.

Sept. 20, 1783, I viewed the nebula between Flamſteed's 99th and 105th Piſcium, diſcovered by Mr. Mechain, in 1780.

" It is not viſible in the finder of my 7-feet teleſcope; but that of my 20-feet ſhews it."

Oct. 28, 1784, I viewed the ſame object with the 7-feet teleſcope.

" It is extremely faint. With a magnifying power of 120, it ſeems to be a collection of " very ſmall ſtars: I ſee many of them."

At the time of theſe obſervations, my 7-feet teleſcope had only a common finder, with an aperture of the object glaſs of about $\frac{3}{4}$ of an inch in diameter, and a ſingle eye-lens; therefore its penetrating power was $\frac{\sqrt{,899 \times \overline{7,5}^2}}{2} = 3,56$. The finder of the 20-feet inſtrument, being achromatic, had an object glaſs 1,17 inch in diameter: its penetrating power, therefore, was $\frac{\sqrt{,85 \times \overline{11,7}^2}}{2} = 4,50$.

Now, that one of them ſhewed the nebula and not the other, can only be aſcribed to ſpace-penetrating power, as both inſtruments were equal in magnifying power, and that ſo low as not to require an achromatic object glaſs to render the image ſufficiently diſtinct.

The 7-feet reflector evidently reached the ſtars of the nebula; but its penetrating and magnifying powers are very conſiderable, as will be ſhewn preſently.

July 30, 1783, I viewed the nebula ſouth preceding Flamſteed's 24 Aquarii, diſcovered by Mr. Maraldi, in 1746.

" In the ſmall *ſweeper* *, this nebula appears like a teleſcopic comet."

Oct. 27, 1794. The ſame nebula with a 7-feet reflector.

" I can ſee that it is a cluſter of ſtars, many of them being viſible."

If we compare the penetrating power of the two inſtruments, we find that we have in the firſt $\frac{\sqrt{,41 \times \overline{42^2 - 12^2}}}{2} = 12,84$; and in the latter $\frac{\sqrt{,43 \times \overline{63^2 - 12^2}}}{2} = 20,25$. However, the magnifying power was partly concerned in this inſtance; for, in the *ſweeper* it was not ſufficient to ſeparate the ſtars properly.

March 4, 1783. With a 7-feet reflector, I viewed the nebula near the 5th Serpentis, diſcovered by Mr. Meſſier, in 1764.

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* The ſmall ſweeper is a Newtonian reflector, of two feet focal length; and, with an aperture of 4,2 inches, has only a magnifying power of 24, and a field of view 2° 12'. Its diſtinctneſs is ſo perfect, that it will ſhew letters at a moderate diſtance, with a magnifying power of 2000; and its movements are ſo convenient, that the eye remains at reſt while the inſtrument makes a ſweep from the horizon to the zenith.

A large one of the ſame conſtruction has an aperture of 9,2 inches, with a focal length of 5 feet 3 inches. It is alſo charged low enough for the eye to take in the whole optic pencil; and its penetrating power, with a double eye glaſs, is $\frac{\sqrt{,41 \times \overline{92^2 - 21^2}}}{2} = 28,57$.

"It has ſeveral ſtars in it; they are however ſo ſmall that I can but juſt perceive ſome, "and ſuſpect others."

May 31, 1783. The ſame nebula with a 10-feet reflector; penetrating power

$$\frac{\sqrt{,43\times\overline{89^2-16^2}}}{2}=28,67.$$

"With a magnifying power of 250, it is all reſolved into ſtars; they are very cloſe, "and the appearance is beautiful. With 600, perfectly reſolved. There is a conſide-"rable ſtar not far from the middle; another not far from one ſide, but out of the cluſter; "another pretty bright one; and a great number of ſmall ones."

Here we have a caſe where the penetrating power of 20 fell ſhort, when 29 reſolved the nebula completely. This object requires alſo great magnifying power to ſhew the ſtars of it well; but that power had before been tried, in the 7-feet, as far as 460, without ſucceſs, and could only give an indication of its being compoſed of ſtars; whereas the lower magnifying power of 250, with a greater penetrating power, in the 10-feet inſtrument, reſolved the whole nebula into ſtars.

May 3, 1783. I viewed the nebula between η and ϱ Ophiuchi, diſcovered by Mr. Meſſier, in 1764.

"With a 10-feet reflector, and a magnifying power of 250, I ſee ſeveral ſtars in it, "and make no doubt a higher power, and more light, will reſolve it all into ſtars. This "ſeems to be a good nebula for the purpoſe of eſtabliſhing the connection between nebulæ "and cluſters of ſtars in general."

June 18, 1784. The ſame nebula viewed with a large Newtonian 20-feet reflector; penetrating power $\frac{\sqrt{,43\times\overline{188^2-21^2}}}{2}=61,18$; and a magnifying power of 157.

"A very large and very bright cluſter of exceſſively compreſſed ſtars. The ſtars are but "juſt viſible, and are of unequal magnitudes: the large ſtars are red; and the cluſter is a "miniature of that near Flamſteed's 42d Comæ Berenices. R A $17^h\ 6'\ 32''$; PD $108^\circ\ 18'$."

Here, a penetrating power of 29, with a magnifying power of 250, would barely ſhew a few ſtars; when, in the other inſtrument, a power 61 of the firſt ſort, and only 157 of the latter, ſhewed them completely well.

July 4, 1783. I viewed the nebula between Flamſteed's 25 and 26 Sagittarii, diſcovered by Abraham Ihle, in 1665.

"With a ſmall 20-feet Newtonian teleſcope, power 200, it is all reſolved into ſtars, "that are very ſmall and cloſe. There muſt be ſome hundreds of them. With 350, I "ſee the ſtars very plainly; but the nebula is too low in this latitude for ſuch a power."

(To be concluded in our next.)

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Sketch of Ornithorhyncus Paradoxus. By M. BLUMENBACH. See our Journal, Vol. IV. page 506.

THIS animal, ſee Pl. XXIV. Fig. 4. was preſented to the author by the Right Hon. Sir Joſeph Banks. It abounds in a lake of New Holland, near Botany Bay. The form of its body, with the exception of the head, is nearly ſimilar to that of a ſmall otter. Its mouth is large, flat, covered with a naked ſkin, and is nearly ſimilar to the beak of a duck. The edges of its lower jaw, are furniſhed, as in the abovementioned fowl, with ſmall indentation, reſembling the teeth of a ſaw. But notwithſtanding the external reſemblance, the head when diſſected reſembles that of other quadrupeds, but with this remarkable anomaly, that the two intermaxillary bones have an interval between them, which is filled only by cartilages. A great number of nerves, proceeding from the fifth pair, are diſtributed to the beak, and afford the animal all the ſenſibility neceſſary to ſeek its proper food at the bottom of the waters it inhabits.

This creature, from the form of its body, the ſhortneſs of its feet, and the membranes which unite its toes, is in ſome degree analogous to the *porcupine ant-eater*, *myrmecophaga aculeata* of Shaw, Natur. Miſcell. No. 36. *Echidna* Cuv. tab. Zool. But it differs conſiderably from it in the form of its mouth, and the nature of its integuments. It is proper to obſerve, that the family of toothleſs animals, of which few ſpecies were known in the old world, is found to have ſeveral repreſentatives in the vaſt continent of New Holland: thus alſo the claſs of didelphia may be ſaid to exiſt in only few ſpecimens in America and the Indies, compared to the various forms under which they are met with in this newly diſcovered country.

Bulletin de Sciences, An. 8. *No.* 39.

On a new Variety of Zircon, by CITIZEN HAUY.

The chryſtals of Zircon which have hitherto been found in Ceylon, France, and elſewhere, have been tranſported by water into their different ſituations, and we had no indication of their native place, nor of the ſubſtances which ſerved them either for ſupport or matrix. The intereſting travels which have lately been made by Citizen Laſterie into Sweden and Norway, have given us the knowledge of the primitive ſituations of this kind of mineral. Amongſt the objects of natural hiſtory which he has collected, was a granite found at Fridichſwern in Norway, and compoſed of red feld-ſpath and amphibolite with brown chryſtals, known in that country by the name of Veſuvian, which name has been given by the celebrated Werner to the ſubſtance which we call *Idocraſis*. Citizen Hauy has diſcovered that theſe cryſtals eſſentially differ in ſtructure, as well as in their other characteriſtics, both from idocraſis and from brown tin grains, with which, at firſt ſight, they might be confounded; and that they belong to the zircon, of which they preſent a new variety. Their fragments expoſed to the flame of a candle, inſtantly loſe their colour, and this alſo is the caſe with fragments of zircon. Their primitive ſhape, indicated by the direction of the natural junctures, is a rectangular octahedron, (Plate XXIV. Fig. 3,) having the ſame angles as thoſe of the zircon, and like it, diviſible by planes, which proceeding from the ſummits, coincide with the baſes of the triangles, which form the faces of the octahedron.

The variety of which he treats, and which is repreſented by Fig. 3; has thirty-ſix faces.

Its ſign is $\begin{matrix} \overset{1}{D} & \overset{2}{D}{}^{\cdot} & E^{\cdot} & P \\ \overset{:}{l} & u & x & P \end{matrix}$. Citizen Hauy calls it the *ſubtractive zircon*, a denomination which he has adopted in caſes, like the preſent, where one of the exponents which accompany the indicative letters, the letter E for inſtance, is leſs by an unity than the ſum of the other exponents.

The following are the meaſures of the principal angles determined by the help of theoretical calculation. Incidence of *l* upon *l*, 90^{d}, of P upon *l*, 131^{d} 25′ of *x* upon P, 150^{d} 5′, and upon *l*, 142^{d} 55′, of *u* upon *l*, 159^{d} 17′, and upon P 152^{d} 8′.

The length of one of theſe cryſtals taken between the ſummits of the two pyramids is 18 millimetres, and the thickneſs 8 millimetres. Their colour is brown, mixed with orange; they are tranſparent, and they contain brilliant ſpangles, interſperſed through their ſubſtance, which give the appearance of an aventurine.

Bulletin des Sciences, No. 39, p. 16.

Extract of a Letter from CIT. HECTH, *jun. Apothecary at Straſburg, to* CIT. VAUQUELIN.

I lately received a letter from M. Klaproth, in which he informs me, that having purchaſed at the ſhop of M. Hahneman, a certain quantity of the new kind of fixed alkali, which he pretends to have diſcovered, he found, by analyſis, that it was nothing but refined borax.

Thus it is found, that this new fixed alkali, ſo pompouſly announced in the Journals by M. Hahneman, under the name of *pneum*, and ſold by him for twenty francs the ounce, is reduced by the analyſis of M. Klaproth to its proper value.

Mr. John Nancarrow's Improved Steam Engine on Savary's principle, with a separate Condenser &c. &c.

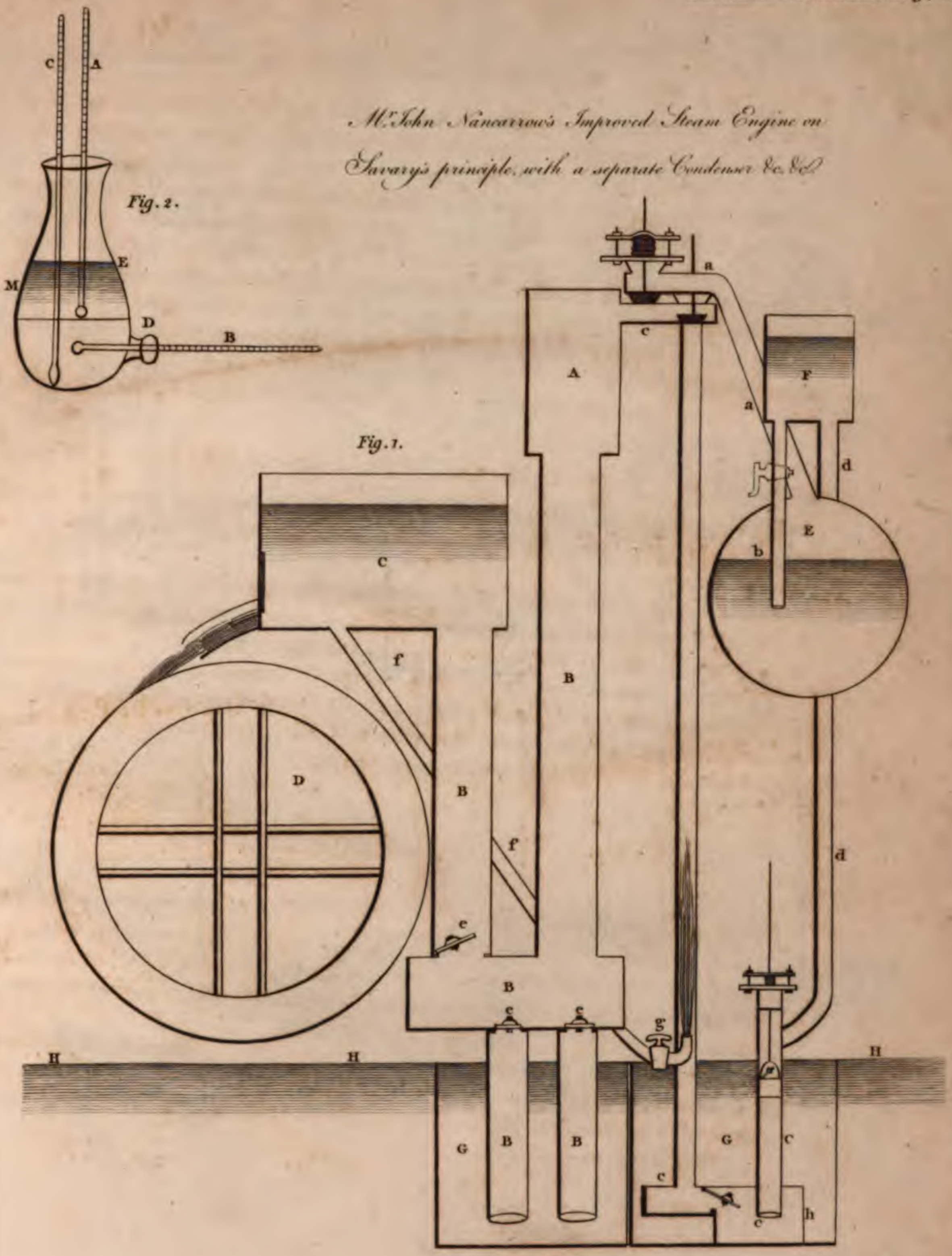

Fig. 2.

Fig. 1.

Mutlow Sculp.

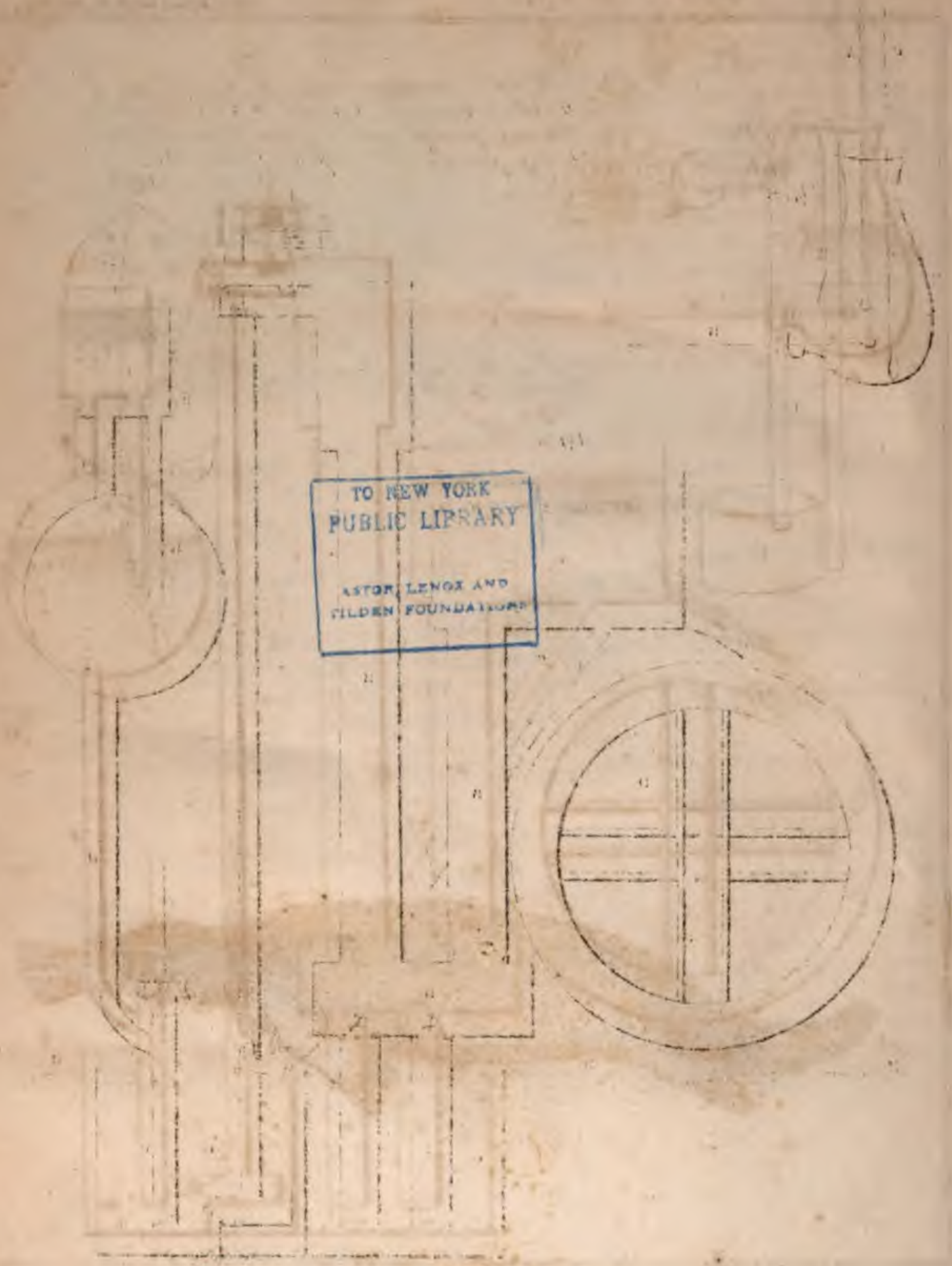

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Fig. 1.

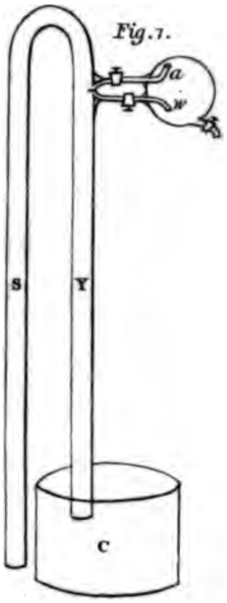


Fig. 2.

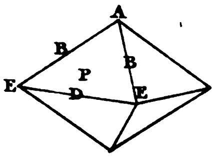


Fig. 3.

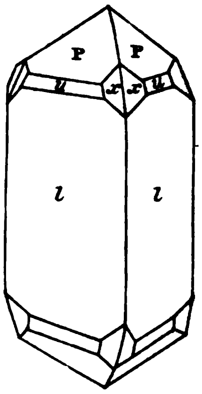
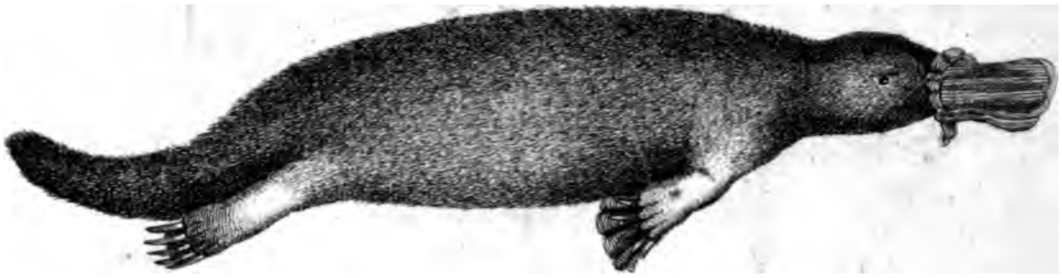


Fig. 4.



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I. On the Antiquity of the Art of etching upon Glaſs, by Mr. Fred. Accum. page 1

Receipt for making an acid to corrode glaſs, publiſhed in 1725. Inveſtigation of the real inventor, Henry Swanhard, of Nuremberg, in 1670.

II. A Deſcription of a new Inſtrument called the Blaſt Ventilator. Invented by J. W. Boſwell. - - - - - - p. 4

A ſtream of air being received through a conical mouth, and conducted through a ſmaller cylinder, gives motion, by its lateral action, to the air in a larger ſurrounding cylinder. The cavity of this laſt is made to communicate, by means of a pipe, with the place required to be ventilated.

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IV. On the Hydraulic Machine erected at Schemnitz in Hungary. p. 18

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MAY 1800.

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In the combuſtion of diamonds under a muffle, the phenomena were obſerved. It begins to take place at about 14° Wedgwood. Guyton's experiment of the converſion of iron into ſteel by the diamond, was repeated with complete ſucceſs: temperature 150° Wedgwood. The ſame treatment of iron, without the diamond, does not afford ſteel; neither does carbon penetrate the veſſels in ſtrong fire. Oxide of iron reduced by diamond powder.

III. Account of certain Phenomena obſerved in the Air Vault of the Furnaces of the Devon Iron Works, together with ſome practical Remarks on the Management of Blaſt Furnaces. By Mr. John Roebuck, in a Letter to Sir James Hall, Bart. - - - - - p. 110

The air vault is a large chamber cut in the ſolid rock. When Mr. R. was in this vault while in work, the air being forced in by a ſteam engine, ſo as to ſupport a column of between five and ſix inches of mercury, with a receprocation of half an inch, a number of remarkable facts preſented themſelves. Great advantages in the expenditure of power when the ſtream of air is made to paſs with a moderate velocity.

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III. On the Genuineneſs and Purity of Drugs and Medicines. By Mr. Fred. Accum. (Concluded from page 36.) p. 159

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Deſcription of a pile of plates of zinc, ſilver, and wetted cards, which give the electric ſhock repeatedly without limit of number or new excitation. Effects of this electric ſtream on the organs of ſenſe. Examination of the inſtrument. Diſcovery that it decompoſes water, between two wires in a tube. Oxidation of metals. Separation of oxygen and hydrogen. Other experiments and enquiries.

X. Some Experiments and Obſervations on Galvanic Electricity. By Mr. W. Cruickſhank, Woolwich. Communicated by the Author. - p. 187

Experiments made by paſſing the current of Volta's pile through wires with a fluid interpoſed. The pile deſcribed. It gives the electric exploſion and ſpark at pleaſure. Nature of the gaſes extricated from water. Experiments with water tinged with litmus---with Brazil wood. Reduction of metals from their ſolutions. Decompoſition of other ſalts, &c.

Scientific News, Accounts of Books, &c. - - p. 191, 192

Magnetic Dip at Prince of Wales's Iſland.---Infuſibility of Tungſten.---*Geometrie deſcriptive*, of Gaſpard Monge.

AUGUST 1800.

Engravings of the following Objects: 1. A new Inſtrument for examining mineral Strata after boring. 2. An apparatus for raiſing Water on the Principle of the Syphon. And, 3. Dr. Hooke's Principles of portable Time Pieces, written in the univerſal Character of Biſhop Wilkins.

I. Experiments on the Production of Air by the Freezing of Water. By Joſeph Prieſtley, L.L.D. F.R.S. &c. &c. - - - p. 193.

Whenever

Whenever water is frozen and again thawed, there is an extinction or production of azote, which is alike in quantity at each ſucceſſive proceſs. Remarks and obſervations.

II. On the Abſorbent Powers of different Earths. By Mr. John Leſlie. p. 196

III. On a New Fulminating Mercury. By Edward Howard, Eſq. F. R. S. (Continued from p. 178.) - - - - - p. 200

IV. Account of a Series of Experiments, undertaken with the View of decompoſing the Muriatic Acid. By Mr. William Henry. - p. 209

V. Eudiometric Obſervations. By Citizen Berthollet. - p. 214

VI. Analyſis of a new Variety of Lead Ore. By Richard Chevenix, Eſq. p. 219

VII. Experiments on the Chemical Effects of Galvanic Electricity. By Mr. William Henry. - - - - - - - p. 223

The Galvanic current acts moſt ſtrongly on water through pointed metal.---Decompoſition of concentrated ſulphuric acid;---of nitric acid;---of the oxigenated, but not the common, muriatic acid. Attempt to ſubject the gaſes to the Galvanic energy. Remarkable facts in the decompoſition of the alcalis.

VIII. Deſcription of a new Inſtrument for repeating the Examination of the Strata which have been penetrated by the common boring Inſtrument uſed in Mine Works. By A. Baillet, Inſpector of Mines, and Profeſſor at the Mineral School at Paris. - - - - - - p. 227

A cylindrical piece carries two knives, which are forced outwards at any depth, in a hole already bored. Theſe cut off a portion of the ground, which is brought up for examination.

IX. General Principles and Conſtruction of a Sub-marine Veſſel, communicated by D. Buſhnell, of Connecticut, the Inventor, in a Letter of October, 1787, to Thomas Jefferſon, then Miniſter Plenipotentiary of the United States at Paris. - - - - - - - - p. 229

X. Queſtion reſpecting the Purification of Copper for alloying Gold. By X. Y. p. 235

XI. On the Art of Hat making. Supplementary Letter. By N. L. p. 236

XII. On the Inventions of Robert Hooke, for regulating Time Pieces. By a Correſpondent. - - - - - p. 237

Scientific Publications. - - - - - - p. 239, 240.

Philoſophical Tranſactions of the Royal Society of London for the year 1800. Part I.---Reſearches, Chemical and Philoſophical, concerning Nitrous Oxide, or Dephlogiſticated Nitrous Air, and its Reſpiration.---The Doctrine of Phlogiſton eſtabliſhed, and that of the Compoſition of Water refuted.---Tranſactions of the Royal Society of Edinburgh. Vol. V. Part I. for 1799.

SEPTEMBER 1800.

Engravings of the following Objects: 1. Two new Air Pumps. By Mr. R. A. Clare, of Jamaica. And, 2. A Machine for performing the Proceſs of Kneading.

I. Experiments and Obſervations made with the newly diſcovered Pile of Sig. Volta. By Lieut. Col. Henry Haldane. With Remarks by W. N. p. 242

Deſcription of the Apparatus. Severe irritation felt inſtead of the ſhock, when two needles were thruſt under the ſkin. Exhibition of the pile under water; in vacuo. Various metals tried. Electrical experiments, &c. An enquiry how far the known laws of electricity are ſufficient to account for the Galvanic ſhock. Laws according to which electric jars are charged, ſo as to give equal ſhocks, however different the ſurfaces, &c. Experiment of charging one ſquare foot of glaſs, ſo as to give an equal ſhock with the Galvanic pile. Meaſure of the ſpark which would charge the gold leaf electrometer, is one three-thouſandth of an inch. The Galvanic intenſity is only one twenty-fifth part of this. The Galvanic capacity is equal to that of a battery of three millions ſquare feet, and the productive energy of the pile two hundred times as great as that of a ſtrong electrical machine.

II. Account of a Series of Experiments, undertaken with the View of decompoſing the Muriatic Acid. By Mr. William Henry. (Concluded from p. 214.) p. 245

From theſe experiments, in which the muriatic acid gas was ſubmitted to electricity, as well with as without the addition of inflammable ſubſtances, it was found, 1. That this gas in the drieſt poſſible ſtate ſtill contains water. 2. That electric ſhocks decompoſe this water, the hydrogen becoming elaſtic, and the oxygen uniting with the acid, which diſſolves the mercury. 3. That the electricity ſerves as a medium to combine the oxygen with the muriatic acid. 4. The real muriatic acid undergoes no decompoſition by electricity. 5. Electrical ſhocks are paſſed through a mixture of carbonated hydrogen and muriatic acid gaſes, the ſuſpended water is decompoſed by the carbon, and the reſults are carbonic acid and hydrogen gas. 6. When the water has been all deſtroyed, no farther effect follows; or if by previous electrization the gaſes be ſeparately deprived of water, no farther effect follows from electrifying their mixture. 7. If muriatic acid be an oxygenated radical, its attraction for oxygen is greater than that of charcoal. 8. The fluoric acid is not decompoſed by this proceſs, but ſeems capable of uniting with a ſurplus of oxygen. 9. Carbonic acid appears not ſuſceptible of two ſtates of oxygenation.

III. On a New Fulminating Mercury. By Edward Howard, Eſq. F.R.S. (Concluded from p. 209.) - - - - - - - - p. 249

IV. Additional Remarks on Galvanic Electricity. By Mr. Wm. Cruickſhank, Woolwich. Communicated by the Author. - - p. 254

Chemical Examination of the gaſes afforded by the decompoſition of water by Galvaniſm. Other intereſting experiments. General reſults. 1. That hydrogen, with a very ſmall portion of oxygen and ammonia, is ſeparated from the ſilver ſide. 2. The wire which ſeparates hydrogen from water will reduce metallic ſolutions, whatever may be the wire. 3. That the ſolutions of magneſia and argil are the only earths precipitated by this influence. 4. That when the wire connected with the zinc ſide is gold, or platina, a quantity of oxygen gas, mixed with a little azote and nitrous acid is diſengaged, and the quantity of gas thus diſengaged, is rather more in bulk than one third of the hydrogen from the other wire. 6. That the gaſes obtained by gold or platina wire being exploded together over mercury, the whole nearly diſappears, and forms water, with probably a little nitrous acid. The reſidue is azote. Hypotheſis reſpecting the action of Galvaniſm, and the neceſſity of oxygen for its conduct. A new and more convenient Galvanic apparatus. Production of fulminating ſilver. Decompoſition of ammonia; and of ſulphuric acid. Nitrous acid conducts the Galvanic power too well to admit of its decompoſing action.

V. Deſcription of a Mercurial Air Pump: and of a Double-barreled Air Pump. By Richard Auguſtus Clare, Surgeon, Jamaica. Communicated by the Author - - - - - - p. 264

The mercurial air pump conſiſts of two vertical pipes containing mercury, in one of which is a ſolid piſton, and at the extremity of the other is a valve opening upwards. The mercury in effect anſwers the purpoſe of a piſton, fitted with a degree of accuracy not to be expected in ſolid work. The double barreled pump has the contrivance of a hole inſtead of a lower valve. Its piſtons have valves, but by the very happy diſpoſition of a cock, the effect of the apparatus may be ſo changed that, inſtead of both barrels working from the receiver, the one is made to clear the other very powerfully from the reſidual air. Obſervations to ſhew certain imperfections, and the inefficacy of the lower valve in Cuthbertſon's Air Pump.

VI. A Memoir, in which the Queſtion is examined, whether Azote be a ſimple or compound Body. By Chriſtopher Girtanner, Doctor of Phyſic at Göttingen. (Concluded from p. 171.) - - p. 268

VII. Account of ſome Experiments made with the Galvanic Apparatus of Signor Volta. By Mr. Davy, Superintendant of the Pneumatic Inſtitution. Communicated by the Author - - - p. 275

The leading peculiarity of Mr. Davy's method of operating, conſiſts in his having ſeparated the two portions of water from each other, which include the conducting wires. Between the waters the circuit was made good by the human body, or by animal or vegetable fibre; and the effects, generally ſpeaking, were nearly the ſame as if the connection of water had been entire! He has examined the gaſes from water, and alſo the operation of this method on the alkalis, the ancient mineral acids, &c.

VIII. Deſcription of the Machine for kneading Dough, which is made Uſe of in the Public Bake-houſes of Genoa; from a Model preſented to the Patriotic Society of Milan - - - - - p. 281

IX. On the Uſe of Caoutchouc in Manufactures, and on the Amelioration of Spirits by Age. By J. A. - - - - p. 282

X. Abſtract of a Memoir on a Method of uſing the Syphon for raiſing Water in the Machine of C. Trouville. By Citizen Prony - p. 283

Scientific News, Accounts of Books, &c. - - - p. 286, 288

Klaproth's Analytical Eſſays.---Multiplication of Engravings in Relief.---Rational Toys.

OCTOBER 1800.

Engravings of the following Objects: 1. Mr. Cloſe's new Engine for raiſing Water by the lateral Communication of Motion. 2. Dr. Wollaſton's Figures to illuſtrate the Appearance of double Images produced by Atmoſpherical Refraction. And, 3. Dr. Herſchel's Apparatus for aſcertaining the Effects of the inviſible Rays of the Sun

I. A Project for extending the Breed of fine-wooled Spaniſh Sheep, now in the Poſſeſſion of his Majeſty, into all Parts of Great Britain, where the Growth of fine clothing Wool is found to be profitable - - p. 289

The King having procured ſome Merino ſheep of the firſt character for the fineneſs of their wool, and having cauſed a number of judicious trials to be made, by which it is aſcertained, during the courſe of above ſeven years, that the quality does not degenerate, but maintains all its original fineneſs in this country; and the value of the wool being now known to our manufacturers, His Majeſty has been pleaſed to give away to different perſons, for experiments, more than one hundred rams and ſome ewes: and ſtill further to extend this valuable improvement in our ſtaple commodity, he is now pleaſed to permit ſome ewes and rams to be ſold at moderate prices. The management of this buſineſs is committed to the Right Honourable Sir Joſeph Banks, to whom letters may be addreſſed.

II. Deſcription of an Engine for raiſing Water by the lateral Motion of a Stream of Water through a conical Tube. In two Letters from Mr. William Cloſe p. 293

Water flows out of a ciſtern through the conical pipe of Venturi. Into the throat, or ſmalleſt part of this pipe, is inſerted an air tube, proceeding from a veſſel in a more elevated ſituation. The conſequence of this arrangement is that the air in the upper veſſel is rarefied, and water riſes into that veſſel through a vertical pipe. As ſoon as the veſſel is nearly filled, its contents are delivered by an apparatus, which is contrived to perform the requiſite alternations without attendance.

III. On double Images cauſed by Atmoſpherical Refraction. By William Hyde Wollaſton, M. D. F. R. S. - - p. 298

Short enumeration of the facts. General laws of the theory of Refractions, near the place of contact and admixture of fluids differing in denſity. Experiments to confirm the theory, by including ſyrup and water and alcohol in the ſame veſſel; by means of which double images and inverſion of objects are produced. Other fluids. Hot water carefully placed upon cold. Heated air in the vicinity of red hot iron. Obſervations on the ordinary effects of ſolar heat. Experiments and obſervations with ether, alcohol, and water, to ſhew how far evaporation is concerned in theſe effects.

IV. A Memoir on the Nature of the colouring Principle of the Lapis-lazuli. By Citizen Guyton - - - - p. 308

The ſulphate of lime of Montolier is coloured by a red oxide of iron, which, by treatment with charcoal, affords a ſulphuret of iron, in which the metal is leſs oxided, and which, by ſolution in acids, and precipitation by a pruſſiate, affords not Pruſſian blue, but a green depoſit of a different nature; which ſulphuret preſerves its blue colour in pot-aſh even at the heat of fuſion. A

ſimilar

ſimilar combination is produced from ſulphuret of iron prepared in the direct way. The colouring matter of lapis-lazuli is found by every experiment to be this blue ſulphuret, and nothing elſe.

V. Experiments made with the Metallic Pile of Sig. Volta, principally directed to aſcertain the Powers of different Metallic Bodies. By Lieut. Col. Henry Haldane - - - - - - - p. 313

Account of the effects of piles conſiſting of zinc, gold, ſilver, iron, copper, lead, tin, and mercury, ſeverally combined with each other in pairs. Exploſion of the gaſes. Similar piles were found to act moderately well in atmoſpheric air; better in oxygen gas; and not at all in azote. The reſpirable gaſes were diminiſhed.

VI. Inveſtigation of the Powers of the Priſmatic Colours to heat and illuminate Objects: with Remarks that prove the different Refrangibility of Radiant Heat. To which are added, an Inquiry into the Method of viewing the Sun advantageouſly with Teleſcopes of large Apertures and high magnifying Powers, and Experiments on the Refrangibility of the inviſible Rays of the Sun. By William Herſchel, LL.D. F.R.S. - - - p. 320

VII. Additional Experiments on Galvanic Electricity. By Mr. Davy, Superintendant of the Pneumatic Inſtitution - - - p. 326

Scientific News, Accounts of Books, &c. - - - p. 329—336

Board of Longitude of France. Prize of Aſtronomy.---Tranſlation of a Note of Cit. G. Cuvier, relative to Foſſil Subjects of Natural Hiſtory.---Hydraulic Engine operating by Mercury.—Philoſophical Tranſactions of the Royal Society of London for the Year 1800. Part the Firſt.—Royal Society of Copenhagen.---Decompoſition of the fixed Alkalis.---Separation of Butter.—Beet Sugar and ardent Spirits.---Fourcroy's Synoptic Tables of Chemiſtry.---Note from a Correſpondent reſpecting the Bolognian Phoſphorus.---Extract of a Letter from Mr. William Henry, dated Sept. 25, 1800, to correct an Inference in his Paper on Galvaniſm.

NOVEMBER 1800.

Engravings of the following Objects: 1. An Improvement of Mr. Goodwyn's Hydraulic Engine, by which its Powers are extended, and it is made to work itſelf. 2. Cryſtals of the Arragonite. And, 3. Herſchel's Apparatus for meaſuring the Effects of radiant Heat.

I. Notice of ſome Obſervations on the Cauſes of the Galvanic Phenomena, and on certain Modes of increaſing the Powers of the Galvanic Pile of Volta. By Mr. Davy, Superintendant of the Pneumatic Inſtitution. Communicated by the Author - - - - p. 337

Zinc in the Galvanic circuit undergoes no oxidation at common temperatures with *pure* water; but it is oxided when the water contains either atmoſpheric air, oxygen, nitrous gas, nitrous acid, or marine acid, &c. Theſe ſubſtances at the ſame time undergo chemical change. The pile of Volta does not act with pure water; but it does when the water contains any of the laſt enumerated ſubſtances. Its power appears to be proportional to the oxidation of the zinc; which may therefore be eſteemed the cauſe. This concluſion leads to methods of conſtructing piles of immenſe power. Experiments in proof. Pile with nitrous acid at leaſt ſix times as ſtrong as the common pile.

II. On raiſing Water by the Engine of H. Goodwyn, Eſq. through double or treble the Space of the deſcending Column, and on the proper Arrangement to make it require no Attendance. By a Correſpondent - 342

III. Obſervations and Experiments on Light and Heat, with ſome Remarks on the Enquiries of Dr. Herſchel, reſpecting thoſe Objects. In a Letter from Mr. John Leſlie - - - - - p. 344

Experiments by the photometer, to determine the abſorbent and reflecting powers of coloured ſubſtances with regard to radiant heat; and the heat afforded by the different rays of the ſolar ſpectrum. Remarks on the optical diſcoveries of Newton, and the late experiments of Dr. Herſchel; againſt the latter of which and the inferences thence drawn, various objections are ſtrongly urged. Recommendation of the optics of Bouguer and the Photometria of Lambert.

IV. Account of a Memoir of M. Prouſt, on ſeveral intereſting Points in Chemiſtry - - - - - - p. 350

Oil of camphor. Method of obtaining the pure taning principle. Theory and formation of ink with galls and ſulphate of iron. Sulphuric acid profitably obtained from the reſidue of ſulphuric ether. Detection of phoſphoret of carbon in the diſtillation of phoſphorus. Eſſential oil in crude iron. Native iron of Peru, which is conſiderably ductile, and does not ruſt; it is found to contain nickel. Experiments on fleſh. Pyrite of the Incas. Purification of zinc.

V. Experiments on the ſolar and on the terreſtrial Rays that occaſion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occaſion them, are ſubject, in order to determine whether they are the ſame or different. By William Herſchel, LL. D. F. R. S. p. 360

A ſeries of experiments were made in which the light of the ſun, of a candle, of the ſolar priſmatic ſpectrum, of a red hot poker, and of a coal fire, were ſeverally reflected from metallic mirrors, and produced heat. The inviſible radiance from the ſame objects was alſo made to undergo optical reflection and condenſation, and produced heat. Experiments of the ſame kind were made by diverting the viſible as well as inviſible radiance, of the ſame objects out of its courſe by refraction, and condenſing it by the ſame means, in all which caſes heat was produced.

VI. On the Arragonite of Werner. By Cit. Hauy - p. 367

VII. On the true Origin of Reſin, known by the Name of *Sandarac*, and on that of Gum Arabic, by M. Schouſboe - - p. 369

VIII. Remarks on the Memoir in which M. Girtanner examines whether Azote be a ſimple or compound Body. By Cit. Berthollet p. 371

IX. On the Decompoſition of the Muriatic Acid. By Mr. John Pitchford, Junior - - - - - p. 374

X. Obſervations on the Action which the Sulphate of Iron exerciſes upon Nitrous gas, and on the Formation of Muriatic Acid. By Cit. Berthollet p. 377

Scientific News, Accounts of Books, &c. - - p. 379—384

Philoſophical Tranſactions of the Royal Society of London, for the Year 1800. Part the Second. Foſſil Wood, found at a very great Elevation.---Extract of a Letter from Mr. Humphry Davy, dated October 23. Supplementary to his Paper on Galvaniſm, in the preſent Number.---Analyſis of various Foſſils.---Lime in the Nut Gall.---Extraction of the Alkali of Sea Salt by Lime. Method of obtaining Phoſphorus by Phoſphorate of Lead.---On the Mineral Kermes, and the golden Sulphur of Antimony.---Elaſtic Reſin in Opium.---Diſcovery of a New Earth in the Beryl of Georgen-Stadt.---On the Method of hearing by the Teeth.

DECEMBER 1800.

Engravings of the following Objects: 1. Conſtruction of a Wheel to repreſent the Planetary Motions. And, 2. Method of graduating the Hydrometer, ſo as to indicate the Strength of Spirit by Inſpection at any Temperature.

I. On the Number of the Primitive Colorific Rays in Solar Light. By the Rev. Matthew Young, D. D. S. F. T. C. D. and M. R. I. A. p. 385

Whether the primitive colours be compoſed of three original tints or more? cannot be determined by mixture of powders, &c. That light of one colour is not all of the ſame refrangibility, is inferred from the extent of each colour on the ſpectrum. Probability that red, yellow and blue are the primitive colours, becauſe any two of theſe adjacent to each other will compoſe the intermediate tinge; though theſe ſuppoſed originals cannot be ſo compoſed of the others. Theoretic obſervations, experimental teſt of this doctrine by coloured glaſſes. Ocular ſpectra. Hypotheſis of three colours will ſolve the phænomena of chromatics.

II. An Account of ſome Additional Experiments and Obſervations on the Galvanic Phenomena. By Mr. Davy, Superintendant of the Pneumatic Inſtitution. Communicated by the Author - p. 394

Sulphuric acid excites the pile very little, but becomes active by the addition of water. Liquid ſulphurets are equal in power to water. Nitric and ſulphuric acids act in vacuo. Obſervations, &c. Enquiries to determine what happens in the pile itſelf. Examination of the power of diffcerent Galvanic combinations.

III. On the Quotients ariſing from the Diviſion of an Unit by prime Numbers. By H. Goodwyn, Eſq. - - - p. 402

IV. Conſtruction of a Wheel adapted to expreſs, by its Rotation, the unequal angular Motion of the Planets. By M. Roemer - p. 404

V. On the Solutions and Precipitates of Mercury. By Cit. Berthollet p. 405

VI. A Chemical Analyſis of Three Species of Whinſtone, and Two of Lava. By Robert Kennedy, M.D. F.R.S. & F.A.S. Edin. p. 407

VII. Further Remarks on the Enquiries of Dr. Herſchel, reſpecting Light and Heat. In a Letter from Mr. John Leſlie - p. 416

VIII. Experiments and Obſervations on the Light which is ſpontaneouſly emitted, with ſome Degree of Permanency, from various Bodies. By Nathaniel Hulme, M.D. F.R.S. and A.S. - - p. 421

IX. On Areometry; more particularly as it relates to Alcohol of different Strengths and Temperatures. By Cit. Haſſenfratz - p. 428

JANUARY 1801.

Engravings of the following Objects: 1. Mr. Edgeworth's Panorganon, or Apparatus for teaching the firſt Principles of Mechanics. And, 2. A new rotatory Engine for raiſing Water.

I. Conſtruction and Uſe of an univerſal Table of Intereſt. By H. Goodwyn, Eſq. - - - - - p. 433

II. A Chemical Analyſis of Three Species of Whinſtone, and Two of Lava. By Robert Kennedy, M.D. F.R.S. & F.A.S. Edin. (Concluded from p. 415.) p. 438

III. Deſcription and Uſe of a cheap and ſimple Apparatus for teaching the firſt Principles of Mechanics. By Richard Lovell Edgeworth, Eſq. F.R.S. and M.R.I.A. - - - - p. 443

IV. Experiments and Obſervations on the Light which is ſpontaneouſly emitted, with ſome Degree of Permanency, from various Bodies. By Nathaniel Hulme, M.D. F.R.S. and A.S. (Concluded from page 427) p. 451

V. Obſervations on the Theory of Electric Attraction and Repulſion. By the Rev. George Miller, F.T.C.D. - - - p. 461

VI. Deſcription of a new rotatory Engine for raiſing water, and for other Purpoſes. By a Correſpondent - - p. 466

VII. Account of a new Method of bleaching Cotton, as publiſhed by Chaptal, Member of the National Inſtitute. By J. C. Delametherie p. 469

VIII. On

VIII. On the Chemical Effects of the Pile of Volta. By a Correſpondent p. 472

IX. Memoir of ſeveral newly diſcovered Properties of phoſphorated Hydrogen Gas. By Citizen Raymond, Profeſſor of Chemiſtry at the Central School of the Department of the Ardeche - - p. 474

Scientific News, Accounts of Books, &c. - - p. 475—480

Premiums offered by the Board of Agriculture.---Prizes offered by the Claſs of Mathematical and Phyſical Sciences of the National Inſtitute of France in its Public Sitting, 15th Germinal in the year 8 (April 4, 1800.)---Candles with wooden Wicks.---Ink capable of reſiſting the Action of Oxygenated Muriatic Acid.

FEBRUARY 1801.

Engravings of the following Objects: 1. An improved Hydraulic Engine, by Mr. Cloſe. 2. The Duke of Bridgewater's underground inclined Plane. 3. Figures illuſtrative of Galvanic Diſcoveries. And, 4 Mr. Home's Exhibition of the Structure of that ſingular Animal the Ornithorhynchus paradoxus.

I. Obſervations on a Method of reſtoring the Utility of Wells, which have been abandoned in conſequence of the Mephitization of the Ground. By Cit. Cadet-de-Vaux, of the Society of Agriculture of the Department of the Seine in France) Communicated at the Sitting of that Society on the 16 Brumaire, in the Year 8 - - - - - p. 481

The method conſiſts in boring and then driving a wooden pipe into the cavity. Through this pipe the borer is made to act and increaſe the depth. Another pipe is driven in ſo as to ſink the firſt ſtill lower. By a continuation of this proceſs the length of pipe is carried to very great depths, if neceſſary, and water is conducted from the lower ſprings to the ſurface.

II. Memoir of ſeveral newly diſcovered Properties of Phoſphorated Hydrogen Gas. By Citizen Raymond, Profeſſor of Chemiſtry at the Central School of the Department of the Ardéche. (Concluded from page 475) p. 483

III. Deſcription of the underground inclined Plane, executed at Walkden Moor in Lancaſhire, by His Grace the Duke of Bridgewater. By the Rev. Francis Egerton - - - - - p. 486

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